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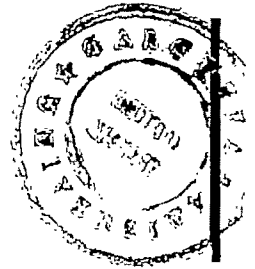
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79

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Vol. 79

March 1985

No. 1

CONTENTS

<i>Power and the Monopoly of Information</i>	Philip E. Converse	1
APSA Presidential Address, 1984		
<i>A New View of Political Accountability for Economic Performance</i>	Henry W. Chappell, Jr., & William R. Keech	10
<i>A Reevaluation of Realignments in American Politics: Evidence from the House of Representatives</i>	David W. Brady	28
<i>Perceived Representativeness and Voting: An Assessment of the Impact of "Choices" vs. "Echoes"</i>	John F. Zipp	50
<i>Voter Participation and Strategic Uncertainty</i>	Thomas Palfrey & Howard Rosenthal	62
<i>The Political Economy of Group Membership</i>	John Mark Hansen	79
<i>Aggregate Stability and Individual-Level Flux in Mass Belief Systems: The Level of Analysis Paradox</i>	Ronald Inglehart	97
<i>The Political Bases of Citizen Contacting. A Cross-National Analysis</i>	Alan S. Zuckerman & Darrell M. West	117
<i>A Theoretical Analysis of the "Green Lobby"</i>	V. Kerry Smith	132
<i>Provision of Public Goods and the MCS Experimental Paradigm</i>	Amnon Rapoport	148
<i>The War Trap Revisited: A Revised Expected Utility Model</i>	Bruce Bueno de Mesquita	156
<i>The Transmission of Legal Precedent: A Study of State Supreme Courts</i>	Gregory A. Caldeira	178
Book Reviews		
American Government and Politics		195
Comparative and Other Area Studies		232
International Relations		251
Normative Theory		276
Empirical Theory and Methodology		287

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BOOK REVIEWS

American Government and Politics

- Abernathy, M. Glenn, Dilys M. Hill, & Phil Williams, eds.
The Carter Years: The President and Policy Making Leslie Schuster 195
- Agresto, John. *The Supreme Court and Constitutional Democracy* Christopher Wolfe 196
- Barber, Benjamin R. *Strong Democracy: Participatory Politics for a New Age* Norman N. Gill 197
- Barger, Harold M. *The Impossible Presidency: Illusions and Realities of Executive Power* William D. Pederson 197
- Berry, Jeffrey M. *The Interest Group Society* Raymond H. Gusteson 198
- Blank, Robert H. *Redefining Human Life: Reproductive Technologies and Social Policy*
- Petchesky, Rosalind Pollack. *Abortion and Woman's Choice: The State, Sexuality and Reproductive Freedom* Sue Tolleson Rinehart 199
- Brigham, John. *Civil Liberties and American Democracy* Richard J. Maiman 200
- Chackerian, Richard, & Gilbert Abcarian. *Bureaucratic Power in Society* Gerald W. Johnson 201
- Clark, Terry Nichols, & Lorna Crowley Ferguson. *City Money: Political Processes, Fiscal Strain and Retrenchment* J. Edwin Benton 202
- Edwards, George C., III, & Stephen J. Wayne, eds.
Studying the Presidency Bruce E. Altschuler 203
- Gawthrop, Louis C. *Public Sector Management, Systems, and Ethics* Richard C. Kearney 204
- Goldenberg, Edie N., & Michael W. Traugott.
Campaigning for Congress Lyn Ragsdale 205
- Hargrove, Erwin C., & Paul K. Conkin, eds. *TVA: Fifty Years of Grass-roots Bureaucracy* Robert W. Kweit 206
- Harmel, Robert, ed. *Presidents and Their Parties: Leadership or Neglect?* John Orman 207
- Hawley, Willis D., et al. *Strategies for Effective Desegregation: Lessons from Research* Emmett H. Buell, Jr. 208
- Jacob, Herbert. *The Frustration of Policy: Responses to Crime by American Cities*
- Scheingold, Stuart A. *The Politics of Law and Order: Street Crime and Public Policy* Roy B. Flemming 208
- Kates, Don B., Jr., ed. *Firearms and Violence: Issues of Public Policy* Ted Robert Gurr 210
- Keeter, Scott, & Cliff Zukin. *Uninformed Choice: The Failure of the New Presidential Nominating System* Paul Allen Beck 211
- Kelley, George Armstrong. *Politics and Religious Consciousness in America* James L. Guth 211
- Kettl, Donald F. *The Regulation of American Federalism* George S. Blair 212
- Kingdon, John W. *Agendas, Alternatives, and Public Policy* Clifford J. Wirth 213
- Klehr, Harvey. *The Heyday of American Communism: The Depression Decade* James P. Young 214
- Malbin, Michael J., ed. *Money and Politics in the United States: Financing Elections in the 1980s* John C. Green 214
- Margiotta, Franklin D., James Brown, & Michael J. Collins, eds. *Changing U.S. Military Manpower Realities*
- Yarmolinsky, Adam, & Gregory D. Foster. *Paradoxes of Power: The Military Establishment in the Eighties* Abbott A. Brayton 215
- McLauchlan, William P. *Federal Court Caseloads* David S. Mann 216
- Meltsner, Arnold J., & Christopher Bellavita. *The Policy Organization* Frank T. Colon 217

Merelman, Richard M. <i>Making Something of Ourselves: On Culture and Politics in the United States.</i>	W. Richard Merriman	218
Monroe, Kristen Renwick. <i>Presidential Popularity and the Economy.</i>	David Menninger	219
Nelson, Barbara J. <i>Making an Issue of Child Abuse: Political Agenda Setting for Social Problems.</i>	David C. Colby	220
Page, Benjamin I., & Mark P. Petracca. <i>The American Presidency.</i>	James A. Davis	221
Price, Don K. <i>America's Unwritten Constitution: Science, Religion, and Political Responsibility.</i>	M. Susan Power	222
Pritchett, C. Herman. <i>Constitutional Law of the Federal System.</i>		
Pritchett, C. Herman. <i>Constitutional Civil Liberties.</i>	Stephen A. Graham	223
Rosenstone, Steven J., Roy L. Behr, & Edward H. Lazarus. <i>Third Parties in America: Citizen Response to Major Party Failure.</i>	John W. Epperson	224
Rourke, John. <i>Congress and the Presidency in U.S. Foreign Policymaking: A Study of Interaction and Influence, 1945-1982.</i>	James M. McCormick	224
Schick, Allen, ed. <i>Making Economic Policy in Congress.</i>	Steven E. Schier	225
Silverstein, Mark. <i>Constitutional Faiths: Felix Frankfurter, Hugo Black, and the Process of Judicial Decision Making.</i>	John Brigham	226
Smith, Steven S., & Christopher J. Deering. <i>Committees in Congress.</i>	Jack R. Van Der Slik	227
Strum, Philippa. <i>Louis D. Brandeis: Justice for the People.</i>	Mark Silverstein	228
Tonry, Michael, & Franklin E. Zimring, eds. <i>A Review of Reform and Punishment: Essays on Criminal Sentencing.</i>	William E. Kelly	229
Vig, Norman J., & Michael E. Kraft, eds. <i>Environmental Policy in the 1980s: Reagan's New Agenda.</i>	Ann Bowman	230
Willie, Charles Vert. <i>School Desegregation Plans that Work.</i>	Hindy Lauer Schachter	231
Woolley, John T. <i>Monetary Politics: The Federal Reserve and the Politics of Monetary Policy.</i>	Michael Wolkoff	231

Comparative and Other Area Studies

Abel, Christopher, & Nissa Torrents, eds. <i>Spain: Conditional Democracy.</i>		
Bell, David S., ed. <i>Democratic Politics in Spain: Spanish Politics after Franco.</i>	Donald Share	232
Alderman, Geoffrey. <i>The Jewish Community in British Politics.</i>	Roger King	234
Banac, Ivo. <i>The National Question in Yugoslavia: Origins, History, Politics.</i>	Alex N. Dragnich	234
Benningsen, Alexandre, & Marie Broxup. <i>The Islamic Threat to the Soviet State.</i>	Norma C. Noonan	235
Bogdanor, Vernon, & David Butler, eds. <i>Democracy and Elections: Electoral Systems and Their Political Consequences.</i>	Joel D. Wolfe	236
Chapman, John W., ed. <i>The Western University on Trial.</i>	Leonard A. Cole	237
Chilcote, Ronald H., & Dale L. Johnson, eds. <i>Theories of Development: Mode of Production or Dependency?</i>		
Higgott, Richard A. <i>Political Development Theory.</i>	Douglas Friedman	238
Chotiner, Barbara Ann. <i>Khrushchev's Party Reform: Coalition Building and Institutional Innovation.</i>		
Linden, Carl A. <i>The Soviet Party-State: The Politics of Ideocratic Despotism.</i>	John M. Carfora	239

Connor, Walker. <i>The National Question in Marxist-Leninist Theory and Strategy</i>	David L. Williams	241
Damis, John. <i>Conflict in Northwest Africa: The Western Sahara Dispute</i>	Benyamin Neuberger	242
della Porta, Donatella, & Gianfranco Pasquino, eds. <i>Terrorismo E Violenza Politica Tre casi a confronto: Stati Uniti, Germania e Giappone</i>	Leonard Weinberg	242
Dogon, Mattei, & Dominique Pelassy. <i>How to Compare Nations: Strategies in Comparative Politics</i>	Gary M. Klass	243
Jacobs, Everett M., ed. <i>Soviet Local Politics and Government</i>	Aron G. Tannenbaum	244
Johnson, Kay Ann. <i>Women, the Family and Peasant Revolution in China</i>	Jeanne L. Wilson	245
Kochanek, Stanley A. <i>Interest Groups and Development: Business and Politics in Pakistan</i>	Theodore P. Wright, Jr.	246
Kornberg, Allan, & Harold D. Clarke, eds. <i>Political Support in Canada: The Crisis Years</i>	George Steven Swan	247
Piscatori, James P., ed. <i>Islam in the Political Process</i>	Sheikh R. Ali	247
Turner, Frederick C., & Jose Enrique Miguens, eds. <i>Juan Peron and the Reshaping of Argentina</i>	Robert E. Biles	248
Wesson, Robert, & David V. Fleischer. <i>Brazil in Transition</i>	Elizabeth G. Ferris	249
Whiteley, Paul. <i>The Labour Party in Crisis</i>	Peter Mair	249
Zwick, Peter. <i>National Communism</i>	R. Judson Mitchell	250

International Relations

Al-Sowayegh, Abdulaziz. <i>Arab Petropolitics</i>	Sheikh R. Ali	251
Arlinghaus, Bruce E., ed. <i>African Security Issues: Sovereignty, Stability, and Solidarity</i>	Henry L. Bretton	252
Arlinghaus, Bruce E., ed. <i>Arms for Africa: Military Assistance and Foreign Policy in the Developing World</i>	Deborah J. Gerner	253
Bar-Siman-Tov, Yaacov. <i>Linkage Politics in the Middle East: Syria between Domestic and External Conflict, 1961-70</i>	Joseph M. Schonick, Jr.	254
Baugh, William H. <i>The Politics of Nuclear Balance: Ambiguity and Continuity in Strategic Policies</i>	Paul Michael Kozar	254
Brown, Harold. <i>Thinking about National Security: Defense and Foreign Policy in a Dangerous World</i>	Robert H. Puckett	255
Callaghy, Thomas M., ed. <i>South Africa in Southern Africa: The Intensifying Vortex of Violence</i>	Richard Dale	256
Craig, Gordon A., & Alexander L. George. <i>Force and Statecraft: Diplomatic Problems of Our Time</i>	Robert Mandel	256
Doran, Charles F., George Modelski, & Cal Clark, eds. <i>North/South Relations: Studies in Dependency Reversal</i>	Vincent A. Mahler	257
Erisman, H. Michael, ed. <i>The Caribbean Challenge: U.S. Policy in a Volatile Region</i>	Sherrie L. Bayer	258
Freedman, Robert O., ed. <i>The Middle East since Camp David</i>	J. E. Peterson	259
Frei, Daniel, & Christian Catrina. <i>Risks of Unintentional Nuclear War</i>		
Segal, Gerald et al. <i>Nuclear War and Nuclear Peace</i>	Benjamin N. Schiff	260
Galbraith, John Kenneth. <i>The Voice of the Poor</i>	Richard Goldstein	261
Gereffi, Gary. <i>The Pharmaceutical Industry and Dependency in the Third World</i>	Marvin Alisky	262
Grayson, George W. <i>The United States and Mexico: Patterns of Influence</i>	C. Richard Bath	263
Hammond, Paul Y. et al. <i>The Reluctant Supplier: U.S. Decisionmaking for Arms Sales</i>	M. Stephen Pendleton	263

Harkabi, Yehoshafat; ed. by David Altshuler. <i>The Bar Kokhba Syndrome: Risk and Realism in International Politics</i>	Michael Rubner	264
Harries-Jenkins, Gwyn, ed. <i>Armed Forces and the Welfare Societies: Challenges in the 1980s: Britain, the Netherlands, Germany, Sweden and the United States</i>	Jon Alexander	265
Knorr, Klaus, & Patrick Morgan. <i>Strategic Military Surprise: Incentives and Opportunities</i>	Stafford T. Thomas	266
Laursen, Finn. <i>Superpower at Sea: U.S. Ocean Policy</i>	Martin Ira Glassner	267
Levy, Jack S. <i>War in the Modern Great Power System, 1495-1975</i>	Michael D. Ward	268
Murphy, Craig. <i>The Emergence of the NIEO Ideology</i>	Robert S. Jordan	269
Nardin, Terry. <i>Law, Morality, and the Relations of States</i>	Michael M. Gunter	270
Nye, Joseph S., Jr., ed. <i>The Making of America's Soviet Policy</i>	Bruce W. Jentleson	271
Rubinstein, Alvin Z., ed. <i>The Arab-Israeli Conflict: Perspectives</i>	Carl F. Pinkele	272
Sanders, Ralph. <i>International Dynamics of Technology</i>	Denis Fred Simon	273
Springer, Allen L. <i>The International Law of Pollution: Protecting the Global Environment in a World of Sovereign States</i>	James P. Lester	274
Steinbruner, John D., & Leon V. Sigal, eds. <i>Alliance Security: NATO and the No-First-Use Question</i>		
Tucker, Robert W., & Linda Wrigley, eds. <i>The Atlantic Alliance and Its Critics</i>	Wolfram F. Hanrieder	275

Normative Theory

Ball, Terence, & James Farr, eds. <i>After Marx</i>	Mark Warren	276
Berger, Fred R. <i>Happiness, Justice and Freedom: The Moral and Political Philosophy of John Stuart Mill</i>	Ethan M. Fishman	277
Bloor, David. <i>Wittgenstein: A Social Theory of Knowledge</i>	Ian Shapiro	278
Clark, Michael D. <i>Coherent Variety: The Idea of Diversity in British and American Conservative Thought</i>	John D. Harman	279
Eden, Robert. <i>Political Leadership and Nihilism: A Study of Weber and Nietzsche</i>	Nancy S. Love	279
Epstein, David F. <i>The Political Theory of The Federalist</i>		
Furtwangler, Albert. <i>The Authority of Publius: A Reading of The Federalist Papers</i>	J. Jackson Barlow	280
Goldberg, Larry. <i>A Commentary on Plato's Protagoras</i>	Patrick Coby	281
Gray, John. <i>Mill on Liberty: A Defense</i>		
Semmel, Bernard. <i>John Stuart Mill and the Pursuit of Virtue</i>	Gregory Claeys	282
Kateb, George. <i>Hannah Arendt: Politics, Conscience, and Evil</i>	Matthew F. Stolz	283
MacLean, Douglas, & Claudia Mills, eds. <i>Liberalism Reconsidered</i>	Gregory B. Smith	284
Schwartz, Joel. <i>The Sexual Politics of Jean-Jacques Rousseau</i>	Richard P. Hiskes	285
Wood, Neal. <i>The Politics of Locke's Philosophy: A Social Study of "An Essay Concerning Human Understanding"</i>	Don Herzog	286

Empirical Theory and Methodology

Ascher, William, & William H. Overholt. <i>Strategic Planning and Forecasting: Political Risk and Economic Opportunity</i>	Glenn P. Hastedt	287
Axelrod, Robert. <i>The Evolution of Cooperation</i>	William C. Mitchell	287

Enelow, James N., & Melvin J. Hinich. <i>The Spatial Theory of Voting: An Introduction</i>	Thomas J. Mortillaro	288
Finsterbusch, Kurt, Lynn G. Llewellyn, & C. P. Wolf, eds. <i>Social Impact Assessment Methods</i>	Mack C. Shelley, II	289
Nagel, Stuart S. <i>Contemporary Public Policy Analysis</i>	James S. Larson	290
Schmookler, Andrew Bard. <i>The Parable of the Tribes: The Problem of Power in Social Evolution</i>	Steven A. Peterson	290
Simonton, Dean Keith. <i>Genius, Creativity, and Leadership: Historiometric Inquiries</i>	Robert S. Robins	291

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Power and the Monopoly of Information

PHILIP E. CONVERSE
The University of Michigan

Presidential Address, American Political Science Association, 1984

The links between power and information are numerous and, for the most part, very obvious. I shall not apologize for returning to the obvious at many points in my remarks, on the grounds that at least a few things that are obvious are also deadly important and deserving of repetition from time to time.

It was about a generation ago that both constructs—power and information—became prize targets for more formal definition and measurement. Indeed, it is a charming coincidence that the publication of Orwell's novel *Nineteen Eighty-Four* (1949), dealing with power and information, coincided exactly with the watershed study by Shannon and Weaver (1949) on the measurement of information and the "mathematical theory of communication." The impact of such measurement has been enormous and continues to grow. Regrettably, however, this form of measurement will be of relatively little use to us here, because its bite is lost in open systems. That is to say, the lanterns in the steeple can convey to the knowledgeable one crucial bit of information—whether the British are coming by land or by sea—but they do not mean much unless the viewer is already apprised that this is a special communication device and knows in advance the full code of potential alternatives. We shall be operating in more open systems, where these helpful constraints are missing.

The numerous attempts from the same period to formalize the treatment of power will be no more helpful, even though most competing definitions of "the power of A over B" did have a similar ring about them, suggesting substantial convergence. We can, however, use one part of that literature to say what we are *not* talking about. Scholars have frequently differentiated among the various bases on which power may rest, and one base that customarily emerges on such lists is expertise. This is, of course, a classic

point of intersection between information and power, and one whose implications have yet to be fully catalogued. This will not, however, be my focus here, although the same phenomena will lurk in the background. Instead, my attention is upon the flow in the opposite direction: the use of power, such as the power of the state, to control information in ways designed to maintain and enhance power. Censorship is what we usually call it, in the small. Writ larger, we talk on the positive side of the "freedom of the press," and larger still in this country, of "First Amendment freedoms."

We cannot expect here to address more than a corner of this large and troubling subject.¹ Discussions of First Amendment freedoms typically come to center upon the expression of opinion and most naturally on those opinions that are unsettling to a regime because they stand in some form of opposition to its policies or premises. I hardly question the importance of such matters. But my primary interest here will be less upon suppression of opinion than upon the suppression of the flow of information about what is going on in the political world of observers both common and uncommon: in short, the suppression of news.

I do not pretend that it is easy to distinguish what I would like to discuss as facts and incontrovertible events from opinions and interpretations, because of course a wide sea of gray lies between these ideal types. "Facts" shade rapidly into "factoids," and on to powerful embroideries of judgment and construction. But shades of gray cloud most First Amendment issues, whether we are asking where pornography begins or "clear and present danger" ends.

So let us take a common-sense view of facts versus opinions. It is likely that government censors do. For example, a revealed copy of the censorship guidelines used 10 years ago by the Polish government makes this distinction routinely. Quite naturally, pure attitudes, in the form of praise of things the regime disliked or criticisms of things it liked, were forbidden, and numerous in-

At various stages I have profited from aid and suggestions from Kenneth A. Bollen, Ada Finifter, Fred Greenstein, Harold K. Jacobson, Seymour Martin Lipset, Mitchell A. Seligson, and William Zimmerman. Responsibility for flaws, however, rests with me.

¹A panoramic view, covering the myriad challenges posed by the flood of new information technologies, is provided by de Sola Pool (1983).

stances are mentioned in detail. But at least an equal share of the required deletions refer to what is called instead "information," to be expunged although its factual accuracy is not challenged. The list of banned topics here is lengthy, and I can only give a glimpse of it. To be obliterated were references to the sheer "existence" of many things, ranging from noted people to detailed forms of trade between the Polish government and other countries. News was not to be conveyed about the actual building or rebuilding of churches, nor was any information about "anti-Semitic excesses." Likewise any hint of the political rights and liberties enjoyed by Polish emigrants in other countries was to be deleted as well as mention of donations they had made back to Poland. Various data compilations were forbidden, such as those that would permit accurate deductions concerning relative international currency values. Also to be deleted was information about industrial accidents, relative rates of occupational disease, and especially information about what were called "direct hazards to human life through the use of chemicals in agriculture" and elsewhere (Pastecka, 1978).

In truth, it will not matter greatly that I prefer to move into the discussion from the side of fact because much of what I have to say can cover both fact and opinion. However, some of the wonderments I shall express remain more compelling where facts are at issue. Hence I shall assume, for example, that there really was a Khrushchev speech before the Twentieth Party Congress in 1956, even though it has yet to be reported to the Soviet public, and in that sense remains a complete nonevent. I shall also assume, and now with the retrospective agreement of all parties, the reality of the United States bombing of Cambodia in 1970, despite official efforts first to conceal the operation and then to deny its rumored existence, less for military security than from fear of public reaction. These events were real, whatever the state of reporting about them.

Now different vocational subcultures have different views of such questions, and it may be a characteristic of the political life to insist on blurring fact and opinion. Perhaps the most vexing triangle of subcultures in the social order is made up of religion, science, and politics. The great misunderstandings along each leg of this abominable triangle are notorious, and no few of these arise from different ways of viewing facts, opinions, and Truth. What distinguishes science is the presumption that there is some core Truth out there which, although never fully knowable, can be approached as a limit, on a staircase of facts. What distinguishes religion is that it is exactly the core Truth which is known, and from within any particular religion, the main problem is the

residual one of deducing the implications of the fixed core of Truth for practices of daily life.

When we come to politics, it must be admitted that the subculture of practicing politicians is not always esteemed for the ways in which it views facts and Truth. Harold Lasswell once put together a collage of quotations about politicians by nonpoliticians over the centuries. Philo of Alexandria, writing in the first century A.D., likened politicians and statesmen to ventriloquists and sorcerers, "men skilled in juggling . . . and tricks of all kinds," and talked of the varied web of political affairs where, "along with the 'smallest possible portion of truth,' falsehoods of every shade of plausibility are interwoven" (Lasswell, 1949). Although Philo may have suffered from an advanced case of political cynicism, his is a view that has echoed down the ages. Facts and Truth are simply viewed more conditionally in the political subculture than in the others, and the conditions are clear: they have to do in the obvious way with whether facts and Truth are agreeable to other ambitions or threaten them.

Nor is it hard to understand the pressures that generate these distinctive views. One is sheer vocational survival. Truth *must* follow function, which is the maintenance of power. At times these postures toward information have an innocuous face. We can admire the agility of the candidate in explaining why an election disaster was actually a triumph, or other forms of blarney and buncombe, especially when accompanied by a twinkling eye. It is said that one inalienable right of the politician is obfuscation, and it may indeed be a necessity.

But there is a darker face to this trifling with information, and it goes well beyond the fact that politicians do not understand why scientists hold raw information sacred, whereas scientists have trouble understanding that the main utility of information is for competitive advantage, be it military or commercial. Add some brute power up front, and we find ourselves moving in the most direct way toward the Orwellian nightmare. What Truth is, is what Headquarters says it is. And if expediency demands, down is up, hot is cold, and white is black.

Before we proceed to a broader canvas, let me add a fourth one to our triangle of subcultures. Undoubtedly the journalistic fraternity is tired of endlessly being counted as the postscript of the Fourth Estate. But it falls to that fraternity to explain the other subcultures to one another. Moreover, journalists from the free media have been at greater pains than anybody else to maintain professional distinctions between fact and opinion. They would prefer first to "tell it like it is," and comment editorially in a separate department. This effort at separation has never been a run-

away success, as I think most journalists would agree. On the other hand, it is hard to argue that there is anything unhealthy about the effort, and once past the first commandment of journalism that the truth not be dull, the views of reality within the Fourth Estate often seem to fit more comfortably with those of scientists than any other.

In politics with a free press, the routine tensions between government and press, once past the peculiar problems posed by wartime, need little detailing. For governments, the media are regularly thought to lack sufficient appreciation of the genius of the administration. They are also, in democracies at least, resented as having power out of all proportion to their nonelective status. How powerful the media actually are is a question that even third parties have not succeeded in defining very well.

We now have collected our main ingredients. With these ingredients in hand, it may be hard to see that there is any mystery whatever in this subject matter. Most Americans would, I suppose, consider the institution of a free press an excellent thing, so there is no particular normative issue to be settled. We might not all be willing to go as far as Thomas Jefferson's celebrated remark, "Were it left to me to decide whether we should have a government without newspapers, or newspapers without a government, I should not hesitate a moment to prefer the latter" (Hulteng & Nelson, 1971, p. 2). Nonetheless, aside once again from the obvious limitations set by security in wartime, it is rather hard to think of appeals to higher values which could possibly underlie the routinized denial of a system of free media. After all, the free press doctrine is a modest one. It does not question the right of governments to broadcast their versions of the news or to justify their ways and good works to their citizens. The government enjoys all of the natural resources of the incumbency advantage, and this is not at all challenged. All that a free press doctrine says is that the power of the state should not be used to prevent other flows of information to its population on matters of public interest.

If there is no normative mystery whatever here, neither is there any motivational one. It is manifestly easier to govern without competing information flows. Still better than having the last word is having the only one. In a world where the only interest is government interest, and it is inconceivable that any citizen or grouping might depart from it even in small ways at the margin, then such prohibitions on media freedom are understandable if, by construction, quite unnecessary. Hence motivation is no mystery.

Finally, there is no capability mystery either. Governments anxious to prevent competing flows

of information typically have the power to do so and make it stick.

And yet, from this cascade of nonmysteries, I found myself plucking one residual mystery. I have already stated it in passing when I noted that it was hard to think of appeals to higher values which could possibly justify the use of force to prevent free media. In wartime, yes. Or if we are satisfied that might makes right, yes. But if there is anything else to be said, I found I could not recollect it.

I regarded even this bit of mystery with some doubt. After all, I am a child of my own political culture, with its great reverence for the Founding Fathers, the mania for checks and balances against absolutist government, and all the rest. I also am saturated with those scientific values that consider direct and unmediated contact with the raw information of reality something quite sacrosanct. Hence for me personal ignorance was as good a hypothesis for this mystery as any, and therefore I set out on a quest aimed at remedying my ignorance. In due time, my quest split into several subquests. The rest of my remarks are a report on these subquests, although most of them can be mercifully brief.

II

My first quest lay toward the classics and history. By classics I mean simply those works that would appear on most lists of the 10 great books in the political philosophy section. These I had largely left untouched since reading them long ago, and it was an interesting exercise to dip back into them with such a sharp focus in mind, and one obviously independent of the reasons why any of these classics were composed or of the historical context that generated them.

I can describe this first quest briefly because it produced little answer to my question. Perhaps a more extensive search could uncover richer lodes than I found. One of the sharpest impressions I received is that in remarkable degree, the whole construct of information seems largely a twentieth-century notion. Plato did develop his celebrated metaphor of cave shadows, addressing the deterioration of information as it is reproduced, but he was dealing with a more general problem. Of course information pervades all of these classic texts by implication, but merely because it is to cognition as air is to breathing. It is scarcely isolated as an entity until studies of propaganda began in our century. I did not, for example, expect much ethical justification from Machiavelli, but I did think he would at least treat deformations of information as an explicit tool of statecraft. In a sense he does, but only as imbedded in

more overloaded concepts such as "flattery" or "fraud."

This relative silence, at least in the major texts I reviewed, may speak to fundamental changes in recent times in the whole macroprocess of information. In an earlier, less interdependent world, persons perhaps witnessed directly many of the events and trends most likely to effect the terms of their lives, and where direct witness did not occur, the means of diffusion, usually oral, were indeed diffuse. It is in the modern world that the events impinging most heavily in a political way upon a citizen's life occur remotely for most people most of the time; furthermore, media channels certainly provide a tempting sort of bottleneck which makes routine government intervention much more feasible than it once was.

The advent of the printing press provided one such handy bottleneck and invited government responses ranging from licensing to confiscation. Such acts helped to crystallize many issues about flows of information and generated a vigorous, if largely unanswered, protest literature. The fact that the Bible was being printed engaged the religious counterpart to the power/information nexus, the ancient question of direct access to the Truth by the rank and file or laity, as opposed to a doctrine that access must be mediated by certified ecclesiastics. Thus, information in sacred texts could in the latter view only be known as revealed by the priests, a proscription that is of course simply the religious version of the issue that is our subject here.

But the concern for direct access, expressed in this period by a whole succession of figures from Martin Luther to George Fox, was hardly new under the sun. You may be aware of the startling view of the early Christian world that scholars have been bringing into focus with the discovery of the trove of Gnostic texts at Nag Hammadi in Upper Egypt. We are all aware that the early centralized Roman Catholic church spent a great deal of time fighting off exotic heresies. What was less obvious was that these heresies were not always isolated challenges, but in part formed a semi-coherent alternative religious community that extended from Lyons in France to Ethiopia, with central norms favoring the direct access of the laity, as well as a participation in ritual so democratic that it even included women on an equal footing (Pagels, 1981). After several generations of hostile coexistence, the hierarchical church at last acquired the police power of the emperor and not only beat down its opponent but also, by outlawing possession of competing gospels, managed to root out most of the information that such a frightening community had ever existed. The Roman bishops were, to draw from Huntington

(1968), "so good at making governments." The Egyptian collection of illegal texts was buried in the sand in the nick of time.

All things considered, my historical quest had brought meager returns. It was clear that the issue was both ancient and central, but I was still without any considered statements justifying the use of power to cut off access to information by the governed. The closest approach, perhaps, was in church governance, where something approaching an appeal to higher principle or authority could be discerned. However, the principle here pivots on a diety and a vigorous appeal to pure faith, much as does the doctrine of the divine right of kings. These terms may be of great importance in theocracies, but did not much satisfy my more secular quest.

III

Moving to the current period, we might first refresh our memories about the distribution of institutions of media freedom in the world today. The picture here is so simple as to be stark. Media freedom co-occurs at a very strong level with a whole series of other familiar political institutions that make up the syndrome we call "democracy." In fact, the co-occurrence is so overpowering as to make it awkward to deal with media freedom as a separable entity. If a government prohibits freedom of the press, it is likely to deny as well a whole waterfront of civil rights and any organized political opposition or independence of the judiciary. This bundle of civil rights is closely tied empirically to another constellation of institutions having to do with the presence of contested elections. If a government denies freedom of the press, it is very unlikely to have elections, or if some are permitted, either the government reserves the right to disqualify candidates it dislikes, or there is rampant government fraud at the polls.

The civil rights bundle and the elections bundle of institutional traits correlate at a level of about $r = .9$ for approximately 140 countries successfully coded in these terms in the most recent *World Handbook of Political and Social Indicators* (Taylor & Jodice, 1983). Such a correlation undoubtedly errs somewhat to the high side in view of likely contaminations in the original coding (Bollen, 1980). But even with some discounting,

The Huntington citation has a different referent: "The real challenge which the communists pose to modernizing countries is not that they are good at overthrowing governments (which is easy), but that they are so good at making governments (which is a far more difficult task)" (1968, p. 8).

the polarization of nations of the world into two distinctive patterns is sharp where fundamental relationships between governors and the governed are concerned. We are guilty of very little caricature when we sort the world simply into "open" and "closed" polities. The "open" polities are the conventional democracies. The "closed" polities are the residual assortment of military dictatorships, socialist states of Marxist-Leninist persuasion, theocracies, and other uncharacterizable traditional states.

Such firm monopolies of public information by governments have a variety of other ramifications; I shall take brief note of only two. I assume first that governments of all kinds, both open and closed, are often tempted to take repressive action against inconvenient minorities, actions that at their most brutal extremes are monitored by an organization like Amnesty International. Let me also assume that rectitude and virtue are randomly distributed across governments of the world. Then if it is at all costly before world opinion to be discovered in any human atrocity, it follows that the commission of such atrocities in the fish-bowl of an open political system is a good deal riskier than it would be behind the windowshades of a closed system and hence should occur less frequently.

At the same time, however, closed systems should be able to hide their atrocities more effectively, so it is not clear how a list of *known* atrocities (always a subset of all atrocities) would differ from open to closed systems. If we take a known list, such as may be culled from Amnesty International's recent report on torture (1984), grading the reports for variations in scope and complicity of the central government, and then compare the list with countries arrayed from closed to open by the *World Handbook* measures, the correlation runs in the mid-.30s, meaning more atrocity reports from closed systems. Moreover, such static correlations would surely be outshadowed by dynamic ones for those numerous polities that oscillate between open and closed, because system closing is often a prelude to government-sponsored brutalization and is motivated by it in an obvious way. All told, more information seems to escape from closed systems than their governors would prefer, or perhaps even expect. If the reach for secrecy exceeds the grasp, it is possible that the penetration of a growing world press is in part responsible.

Another nontrivial characteristic of closed polities is their resistance to any domestic research, especially of a social kind, which is not exclusively controlled by the government, and thereby subject to the possibilities for falsification or at least deletion which that power preserves. Within the past 20 years I have been involved with cross-

national research operations of a public kind, collecting data in both the Soviet Union and in the right-wing dictatorship of Brazil during its most repressive phase. In both cases the local part of the research was supervised by established native scholars operating from well-credentialed indigenous research institutions. The panicked responses and threatening pre-emptive measures taken by officials in both countries to the possibility that the raw materials might be accessible to an international research community were indistinguishable.

This is, of course, hardly surprising for anybody who has tried to study closed systems. But among other implications is the obvious difficulty of knowing what truth value to assign to any self-descriptions, such as government statistics, that do see the light of day, particularly on matters dear to national ideology.

The problem of maintaining at least minimal credibility may suggest one fortunate limit to prevent governments from having their unfettered way with flows of public information within their national boundaries. At least since Socrates, there has been sensitivity to the great watershed between what I think of as first- and second-order ignorance, that is, between not knowing something, and not knowing that you do not know it. Intuitively, the behavioral implications of the difference should be substantial: if you are aware of your ignorance in a matter, you will proceed less blithely and will either stop to search for the missing information or, if it is unattainable, will at least behave in a more tentative manner, out of respect for special uncertainty.

This watershed is of importance for closed systems, given a variety of casual evidence at least that there is a certain leaven of cynicism in such systems about official government information, and no little awareness that there is a good deal that the common citizen is not permitted to know. At the same time, we would expect a certain stratification of public opinion in these regards, and skepticism may run out rather rapidly as we move down into these populations. Indeed, working with our data, collected at the height of the Brazilian repression, Geddes and Zaller (1983) found patterns of susceptibility to the anti-democratic rhetoric of the government-controlled media which nicely parallel work we did on information flows many years ago. That is, the relationship between susceptibility and exposure is basically curvilinear, with susceptibility gaining as exposure increases, but only up to a point: it begins to drop off again as we approach the highest-exposure people, who are the best educated and presumably the most sensitive to what they are not being permitted to know. Nonetheless, the population showing sophisticated indif-

ference to government rhetoric was quite limited, and in general, we must suppose that it must take an uncommonly educated populace before governmental control of information flows begins to boomerang in any serious way.

These are some of the empirical patterns. Before I return to my quest for higher normative principles that justify government deflection of information, let us pause to consider what scholars make of these patterns. If we stay global with the open-closed distinction, rather than focus on media freedom alone, then of course a great deal of analysis and interpretation has been carried out. For example, it is well known that closed systems tend to be less economically developed ones, as well as more "traditional." One popular thesis in recent years has been that open institutions are luxuries that polities struggling to modernize can ill afford, both in the sense of up-front administrative costs, and, in the Huntington (1968) formulation, because open institutions encourage an upwelling of public participation that modernizing political institutions may be too weak to handle.

If such theses are correct we could discern one answer to my quest: closing the system may not be a good thing taken alone, but it is better than not modernizing. This answer is worth jotting down, but it is a rather marginal response unless we can also demonstrate that it is impossible to disengage media freedom from other open institutions, since most of the reasons why such institutions are alleged to be a luxury turn out to be a very poor fit for media freedom taken alone.

This poor fit is obvious where up-front administrative costs are concerned. To be sure, to conduct competitive elections fairly costs something, as do some of the other open institutions. But for media freedom the cost issue is exactly reversed. Maintaining the security forces, censorship offices, and other bureaucratic apparatus to ensure unchallenged state mediation of information is remarkably expensive. Letting public information flow as it will is the obvious cost-free default option.

Of course the costs that are more troubling to the regime, although less certain, occur down the road if uncontrolled flows of information about the government become so very unfavorable that the citizenry tries to replace these leaders with others. Once we add a strong *ceteris paribus*, I accept the doctrine that such alterations of leadership can be dysfunctional for progress in development, although the case is clearest where such alterations are, either in the attempt or the completion, bloody and destructive. We are not without social inventions to permit alteration of leadership without bloodshed.

However, if we continue to restrict our focus to

media freedom, it is worth factoring in other costs to development that state control of information is likely to entail. One cost is a loss of government credibility. From our Brazilian data I would suspect that this cost is not visible in more than a small stratum of the population, but that is the group most crucial for development, including parts of a scientific community most offended by state distortions of information. Another kind of cost can arise if the elite insulated from any alteration comes to believe in its own version of the news and, without normal feedback, drifts off into a delusional system that is clearly maladaptive. Surely in recent decades we have seen some spectacular instances of modernization badly derailed because of eccentric convictions about how to proceed with it on the part of elites shielded by their strong political control from the kinds of new information that are unwelcome and hence demand mid-course corrections.

I cannot weigh these costs against any destabilization that minimal respect for independent information might generate. But if the redeeming goal is truly the noble one of development rather than mere maintenance of power, then it is worth pointing out that these other costs do exist, and that they distort badly any simple balance sheet. At the very least, it seems worth matching a concept of political control too weak for the modernization task, with another concept of overcontrol that can become dysfunctional as well.

We should not leave these empirical patterns without recognizing another cultural justification, which says that subject populations in long-term closed systems are not to be pitied because they actually lack any expectation of voice in the system and would be uncomfortable if suddenly given any. A sophisticated version of the same argument is the Leninist doctrine of the vanguard party, which says in effect that party leadership makes the decisions, not the rank and file, because the rank and file do not understand their own real needs, at least in a short run, although a short run that historically, we must add, has never seemed to terminate. From either version it follows that accurate public information is unnecessary, since there is no public decision-making function.

How real people feel about such matters is so obviously an empirical question that it is a pity it cannot be more directly examined. Nor does it inspire confidence that the rulers of closed systems who give these glib assurances are at the same time the most committed to preventing any actual test. This situation produces an astonishing run of public gibberish. This spring, for example, the Iranian government announced that a number of candidates could not be permitted to run "because they had fallen out of touch with the

people." Even counterelites of some popular liberation fronts, whose main claim to legitimacy is representation of the people against tyranny, threaten to shoot up the countryside to prevent actual people from being asked their preferences at the polls, and escalate threats of disruption even farther when it is suggested that some outside party, like the United Nations, be brought in to assure the fairness of the election.

My own speculation is that these accounts of real people gladly abdicating their own preferences because cultural or ideological arguments tell them they must are grossly false. The scattered instances of relevant data I have seen would certainly bely such descriptions. At least some common subjects in closed systems are feistier than elites often want to make them out to be, and while my documentation is sketchy, I read in the fright of elites and counterelites at any actual test that they share my intuitions.

Is it not this feistiness which translates in the most direct way into the kind of penchant for disorder that Huntington and others find detrimental to development? In one sense yes, but in another sense, not necessarily. There is some truth here because subjects do become aggrieved, and if given any opening, are glad to support counterelite cadres who will do battle for them. On the other hand, there is substantial evidence that in many of these episodes, the common people tire of vicious public disorder much more rapidly than the elites locked in the struggle over power. There is surely nothing to astonish us that this might be so: the common people often suffer much more trauma in such disorder than the elite combatants do. So the elites carry on the power struggle, now quite autonomously, agreeing on little save that the actual people are not to be consulted in the matter. In the degree that this version of the scenario does take place in some cases, it is a pity to say generally that open institutions invite a paralyzing disorder. Their absence can also help prolong the agony.

Another current arena that seemed worth examining in search of ethical justifications for government suppression of public affairs information is the heated debate which has persisted over the past eight years or more, especially within UNESCO, concerning what has been championed by a large number of Third World countries as a "New World Information and Communication Order" (NWICO). One impetus behind this movement is a discouragement within many developing countries at the fact that all of the large international press services are headquartered in the developed countries and thereby report news from the developing countries in what is seem to be an unflattering and often alien perspective. What is sought is better balance in the reporting of world

news. The problem is how to move toward such balance within the framework of a United Nations committed from the outset to freer flows of information in the world, including freedom of the information media.

In abstract principle, when an imbalance develops because A has outrun B, it may be redressed either by expanding B or by constricting A, or some combination of the two. The expansionary redress in this instance, which involves helping with the enlarging of communication infrastructures in less developed countries through both technical aid and the reduction of economic and financial obstacles, is not subject to controversy and has been an explicit element in United Nations programs more or less from the beginning (see Green, 1956). Such expansions are part of the NWICO vision. Some countries however seem less interested in such expansionary solutions than in resolutions that constrict the flow of news by establishing various forms of local political control over it.

Debates over the NWICO vision are rather more confusing than might appear because although a set of general goals has been proposed, actual steps of implementation have frequently been left to be worked out, and therefore their potential meaning remains unclear (Sussman, 1984). Moreover, these debates generate less in the way of ethical justifications for governmental interceptions of information than might appear because the goals usually stated for the establishment of controls are ones of promoting freer flows of information rather than constricting them. Thus one of the clearest tangible proposals to date in the specification of NWICO is for the licensing of journalists by governments. It is billed as a step that would liberate information because the journalist, once licensed, could work "secure" from government harassment. The fact that interest in what seem actually to be restrictive proposals comes predominantly from member states whose governments flout United Nations ideals of media freedom in their own domestic practice does not reassure the observer about the actual intent of steps proposed. However this may be, we search again in vain here for ethical justification of governmental media control. Most countries express support for UN ideals of media freedom, but there is marked variability in views of how such noble goals are best implemented.

IV

The last stage of my quest is again brief, and in a sense, light. I had genuinely imagined at the outset that I could be guided to some rationale beyond naked power for state prevention of independent media. Lest this seem naive, let us

remember that Mosca and other sage observers have argued convincingly that elites have a great capacity for justifying institutions they find useful. Granted that this takes time and routinization: governments are often thrown by circumstance into quick aberrations that are left unexplained. Granted also that the Mosca view is cynical enough as it stands: the dirty grit of necessity comes first, and the pearl of justification only forms in due time to reduce friction. Nonetheless, sooner or later some justification is to arise, and given the importance that information access would seem to have for human dignity, I wanted to see what such justification might be.

Or, to put the matter more simply, imagine a friendly backyard conversation in which a government member sets out to explain to his next-door neighbor why it is in everybody's interest to use part of the citizen's taxes to prevent him from receiving some kind of information about his world. In such a conversation, by construction friendly and relaxed, just what would be said?

I had not searched everywhere for justifications. After all, military dictatorships, while keen on monopolizing public information, tend to come and go in rather short stints and in any event rarely feel much obligation to look beyond naked power as a reason for governance. Human dignity is just not the highest priority. I did not look much to theocracies either, because as we have seen, that answer was not too satisfying for the modern world.

What this left as the most definable residue of closed states were Marxist-Leninist governments. These seemed ideal. They are not as transient as military interludes, so that justifications have had time to form. Moreover, these states place great emphasis on human dignity, and insist on a basic role for normative theory in matters of politics and society. Finally, state monopoly on information flows is a very central part of the blueprint for governance in these states, not just in wartime or under duress, but as a routine matter. Indeed, if one takes what are usually called the stable governments of the world, strict state control of public information is a more sharply distinctive characteristic setting apart Marxist-Leninist governments than anything else commonly coded, such as economic distributions. In practice, if not necessarily theory, this information control is simply the defining signature of such states.

Therefore I sought references to the relevant theoretical literature from colleagues familiar with and, more often than not, personally sympathetic to, current Marxist thought. This turned out to be a remarkably blank wall. One sympathizer could hardly understand my question. After second and third rephrasings, he asked in return with some impatience how there might conceivably be any

justification for such practices. He went on to say that undoubtedly some East European apologist must have written something on the subject, but it certainly was no part of the central Marxist literature. I also received directions to sympathetic descriptive material on the structure and practices of the Soviet press, but with forewarning that it would not address my questions. It did not.

Other efforts at more substantive answers to my query tended to be one or another form of non sequitur. For example, I was told I should not worry about Communist citizens being shut off from world news because much of that simply defied control, for reasons ranging from sailors in foreign ports to European airwaves. I have already assumed this to be true: individual citizens, in an implacably Protestant way, do have their own unmediated interactions with the environment. They do find news and pass it on, and governments, however hard they may try, cannot always stand fully in the way of it. Nonetheless, the limitations on success scarcely address my question about what principles are induced to justify the effort.

I have saved one other response for last because, while again a non sequitur in its own way, it provided as much food for thought as all of the rest of the responses put together. I encountered a television interview between a western journalist and a high official of the Soviet news hierarchy. The journalist more or less asked my question, wondering why the Soviet government permitted no domestic press freedom. The official responded that this was not a very constructive question, because capitalist societies had no free press, either. The journalist began to protest, but the official went on to say that it was self-evident there was no free press, because what was claimed to be a free press was not in fact dedicated to the overthrow of capitalism.

This provides us with a fascinating definition and perception which may explain a good deal, albeit of little help to my quest. It seems to be presumed that a free press is some monolith in ideological terms, instead of the enormous scattering of positions and nonpositions that arises in the West, with a spectrum ranging from organs indeed dedicated to the overthrow of capitalism at one pole all the way to organs that are its staunch defenders, with the great middle range occupied by organs that accept the basic structure of capitalism but, as businessmen bitterly lament, carp persistently at its excesses. Across all of this spectrum, the survival of specific voices depends, if not exclusively at least in significant degree upon maintaining a clientele that resonates to its posture, or upon not offending that large clientele that hopes to order mere news, going light on the ideological garnish.

The official has a totally different vision of what institutions of media freedom actually mean. If we see things from his view, it is clear that accepting media freedom would open his system to information organizations single-mindedly dedicated to ripping Communism out root and branch. If that is the conception of the challenge, then it is not hard to see why it would be unwelcome on the face of it. But my own prediction of what would happen in such an improbable event are quite different. I would wager very heavily that such a true opening would produce not just one voice, but at least several and probably very many. I would also wager, although more cau-

tiously, that the great middle of such an emergent spectrum of free voices in his country would accept communism as a basic structure, but would carp persistently on its excesses and would in general make governance more difficult.

Perhaps this is a prospect sufficiently sinister that it could not be considered. On the other hand, I for one would appreciate hearing the normative side of the rejection, rather than the pure defense of acquired power. To me, the choice implied in a varied stream of information about one's environment is a minimum requirement for human dignity, and one that power should not be used to eliminate.

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A New View of Political Accountability for Economic Performance

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Most political support models imply that in evaluating economic performance, voters use a standard that would provide poor predictions of the future and leave the economy vulnerable to manipulation by vote-hungry politicians. Drawing on macroeconomic theory, we develop a simple standard of evaluation which encompasses a concern not only for current economic outcomes, but also for accurately assessed future consequences of current policies. We find that political support for the president can be explained as well by models that assume that voters use this sophisticated standard as by models that assume voter naivete.

Our analysis questions the wisdom of measures typically used to assess voter evaluation of economic performance in a variety of theoretical contexts. The results also help to explain the absence of convincing evidence that governments exploit voter ignorance in manipulating the economy.

Elections in representative government are simultaneously an opportunity for voters to hold current incumbents accountable for their past performance and to choose among contenders for future office. A growing body of scholarship has emphasized the retrospective evaluation of incumbents as a basis for electoral choice, in part because it seems to be commonly used by voters, and in part because it is seen as a sensible guide to decision.¹ One reason that retrospective evaluation is seen as sensible is that insofar as the past performance of incumbents is a good indicator of their probable future performance, retrospective voting combines the two tasks of holding incumbents accountable and predicting the future.

We will show that the basis for using the past to project the future used in most of the literature on retrospective evaluation of economic performance can be misguided. We present an alternative, more sophisticated standard and show that it explains popular support for the president about as well as, and in some ways better than, the usual standard.

The first section of the article reviews the rationales for retrospective voting, and the second section draws on macroeconomic theory to develop a new standard for evaluation. The third section presents the political support function we use for empirical comparison of sophisticated and naive voter standards, and the last section presents and discusses the results.

Existing Rationales for Retrospective Voting

Downs (1957) and Key (1966) have expressed leading rationales for retrospective voting, the central distinction between them being whether or not the past is used as an indicator of the future. Key makes the more cautious claim. He uses data showing that voters stay with a party or switch on the basis of assessments of past performance to make "the perverse and unorthodox argument . . . that voters are not fools." He makes few positive claims about electoral control and aims his argument at the then-fashionable view that the American electorate is "straightjacketed by social determinants or moved by subconscious urges triggered by devilishly skilled propagandists"

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¹See especially Downs (1957), Fiorina (1981), and Key (1966).

(1966, p. 7). He makes no claim about control of the future; he contends only that voters respond pragmatically with approval or disapproval of what has taken place.

Downs is more ambitious, claiming that "rational behavior is impossible without at least some way of forecasting future events" (1957, p. 106). He defines the logic of voting in terms of a comparison of future utility expected under alternative parties. "Since one of the competing parties is already in power, its performance in period t gives (the voter) the best possible idea of what it will do in the future" (p. 39). Downs acknowledges that this claim assumes that the incumbent's policies will have some continuity, but he argues that the self-interest of the parties will assure "reliability, integrity and responsibility" (pp. 103-109).

Neither Key nor Downs addresses the question of what sorts of standards are or should be used in their rationales for retrospective voting. We will show that the standard used is important, whether the past is used to predict the future or not. Before making this argument, we review briefly a few studies of the relationship between economic performance and political support which are especially relevant to our discussion of standards.

Kramer (1971) developed a careful rationale for retrospective voting which was inspired by Downs and which may be seen as an application of Downs. Kramer rejected as unrealistic a view in which voters choose after comparing party platforms and other information and proposed instead that a more relevant decision rule would be to use the past performance of the incumbent party as a readily available indicator of what it would do if reelected. Voters are assumed to support the incumbent party if its record is deemed satisfactory according to some simple standard the electorate expects it to meet and to oppose it otherwise.

Kramer observes that because it is not possible to observe voters' expectations directly, "a reasonable and convenient hypothesis is that expectations about year t are formed on the basis of experience during the preceding year" (1971, p. 134). As operationalized, Kramer's performance variables are percentage change in real and nominal income, the rate of inflation, and change in the level of unemployment, all measured in the year before the election.

A great many subsequent studies have followed Kramer's lead in relating political support to economic performance, offering many useful refinements. However, very few have been as self-conscious and careful about inferring expectations of the future. Three studies of political support make use of measures of expected future economic performance, but none identifies the processes by which these expectations are formed,

and none derives them from past performance.²

Rabinowitz and Zechman's (n.d., pp. 5-7, 14-15) study of presidential voting addresses directly the question of whether voters consider "the implications of current policies for future performance," and they include in their presidential vote model a variable for expected future performance. They argue that "a growing body of research has indicated that individuals apparently do have reasonably accurate expectations about short-term fluctuations in the performance of the economy and they act upon these expectations." Accordingly, they use actual change in real per capita income during the year following the elections as an unbiased proxy for expectations of future performance; that is, they use post hoc observations of the "future" to measure expectations of the future.

Monroe and Levi (1983) use directly observed economic expectations to explain variations in popular support of presidents, but their inference is only slightly less indirect than that of Rabinowitz and Zechman. Specifically, they use the Joseph Livingston time series of data from surveys of leading businessmen and economists on their expectations of future inflation and economic growth. Although the assumptions are not entirely clear, these elite attitudes seem to be taken as proxies for popular expectations.

Kuklinski and West (1981) report direct survey evidence on voters' own assessments of both their past and their expected future financial well-being. They are thus able to assess directly the relationship between the two in the 1978 National Election Study, from which their data are derived. That relationship was positive, but weak. Kuklinski and West's finding is a sobering one, because it implies that perceptions of past performance may be poor indicators of expectations of the future. It may also undermine Kramer's use of objective measures of past performance to infer expectations of the future, without giving a hint about how one might use the past to infer future expectations more accurately.

These studies are important in providing evidence that expected future well-being is significantly related to political behavior, and they sup-

²In addition to these more narrowly focused studies, Alt (1979) makes great use of expectations in general in explaining the electoral "politics of economic decline" in Britain since 1964, and he makes a careful effort to explain observed expectations of inflation, in particular, with past inflationary experience (1979, chap. 4). Alt's work (1979, 1981) is a rich source of thinking on expectations and a compelling case for their importance in political analysis. See also Minford and Peel (1982), Chappell (1983), Chappell and Keech (in press) and Paldam (1981).

port the notion that voting and other manifestations of political support are at least partly prospectively oriented, as Downs would have predicted. However, these studies leave important issues unaddressed. They do not explain how expectations are formed or related to past experience, and they do not reflect the insights of macroeconomic theory for constraints on possible outcomes, for the possibility that incumbents may not be fully responsible for current outcomes, or for the possibility that good times in the long run may require some short-run distress. We propose and test a model that incorporates these features, at least approximately.

The Insights of Macroeconomics for Measuring Economic Performance

The literature on mass political behavior noted above does not give us clear indications of how, if at all, voters use information on the past to form expectations of the future, but the literature on macroeconomic theory suggests how they might. We will use macroeconomics to show why simple extrapolations of the future from past values of economic variables can be misleading and to identify a simple standard by which voters can combine an assessment of past performance with predictions of future performance.

To know how well an administration performs in managing the economy, we need to understand what patterns of outcomes are feasible and to define a standard of desirability over feasible outcomes. Macroeconomic theory will help us to identify sets of feasible outcomes, and we will present a standard that will reflect a range of reasonable voter preferences given some knowledge of the nature of constraints.

Although there is considerable debate over what macroeconomic theory best describes reality, we believe that the two important assumptions that we make about the economy reflect mainstream views in economics. First, we assume that policymakers can systematically affect economic outcomes in the short run. This point is particularly controversial in current macroeconomic thought, but some theoretical arguments and most empirical evidence seem to support it. This is of course an implicit assumption in all previous studies of political support functions as well. Second, we assume that the tradeoff between unemployment (or output) and inflation differs in the long and short runs.

Although output may be increased (or unemployment decreased) at the cost of higher inflation in the short run, many economists deny that such a tradeoff is possible in the long run. They argue that the long run Phillips Curve is vertical; that there exists a natural rate of unemployment (or

output) that can be associated indefinitely with any rate of inflation. Thus, expansionary monetary and fiscal policies may be able to reduce unemployment below its natural rate at the cost of higher inflation in the short run, but the new combination of lower unemployment and higher inflation is not sustainable in the long run. Once economic agents anticipate the higher rate of inflation and those anticipations are reflected in contracts and market prices, the output-increasing effects of the stimulus are neutralized. Unemployment and output return to their natural rates, while the inflation rate remains higher. Similarly, contractionary policies temporarily increase unemployment and lower output, but in the long run these variables return to their natural rates at a lower inflation rate. The central point is that tradeoffs that are feasible in the short run evaporate in the long run.¹

The practical implications of this result for sensible voting and for the literature relating political support to past economic performance are profound. Almost all of that literature follows Kramer in assuming that voters simply punish incumbents for inflation and unemployment and reward them for growth of output. This behavior would be sensible if inflation, output, and unemployment could each be controlled independently of the others, and if current short-run values of these variables were accurate proxies for their future long-run values. Our preceding arguments have indicated that this is not the case, however. We term such behavior *naive voting* because it implies that voters are unaware of constraints relating outcomes at a given point of time or across time and provides vote-maximizing

¹We have consulted five recent editions of intermediate texts in macroeconomic theory to see how these questions are typically handled. The five texts were Edgeland (1983), Dornbusch and Fisher (1984), Malsel (1982), Gordon (1981), and Froyen (1983). All five texts present the natural rate hypothesis (implying a vertical long-run Phillips curve) as an accepted view. Regarding the implications of rational expectations theories, four of the five texts argued that "policy ineffectiveness" was of dubious validity, based on either empirical findings or theoretical arguments about the inappropriateness of market clearing assumptions. The fifth, Froyen, presented a balanced but agnostic view on that opinion. Most criticism of the results of the rational expectations models were not directly critical of the notion of rational expectations—rather they criticized the associated assumption of continuous market clearing. This leaves open the possibility of assuming rational expectations on the part of voters and economic agents, without assuming away the possibility that policies can systematically affect outcomes. See for example Fischer (1977). This is a view compatible with the sophisticated voter model presented in this article.

politicians with opportunities for manipulation of the economy. Nordhaus (1975) recognized this when he showed that a "political business cycle" could be induced by a government that maximized a voting function very much like those appearing in the literature. Specifically, if voters predict future unemployment and inflation rates by extrapolating from their past values, politicians have an incentive to create apparently desirable, but unsustainable, combinations of inflation and unemployment just before the election. Theory indicates that cyclical manipulation will be most pronounced if voters have short memories, but it may also occur even if voters value or remember all portions of the term equally well.⁴

In contrast to the naive voters characterized by most of the literature, a sophisticated voter would have some sense of feasibility constraints in evaluating outcomes and would be concerned with future as well as current implications of present choices. He would reward incumbents for selecting desirable policies even when times are bad and punish them only for those undesirable outcomes for which they could reasonably be held responsible. For example, an incoming administration might inherit high rates of inflation from its predecessor. Because current inflation is to a large extent a result of past expectations of inflation, the new president should not immediately be punished because of the continuing inflation. Indeed, desirable policy in this instance might call for contractionary policies aimed at gradual reduction of inflation in the long run with modest temporary sacrifices of output. Sophisticated voters would neither punish the president for the inflation (which was caused by predecessors), nor would they reward expansionary increases in output which could lead to further accelerations of inflation in the future. In short, sophisticated voters would recognize that short-run choices are constrained by economic possibilities, and they would reward or punish according to whether selected policies would promote movement toward desired long-run outcomes.

Although the requirements for sophistication may seem very demanding, we can construct a simple measure of economic performance which incorporates the necessary criteria and at the same time might be approximated by ordinary citizens. We will assume that the president, through fiscal and monetary policies, controls real output, Q .⁵ We then define the variable Z_t to serve as an indicator of policy stance, where

$$Z_t = Q_t/QN_t - 1,$$

and QN is natural real gross national product, a level at which there is no tendency for inflation to accelerate or decelerate (Gordon, 1981, p. 11 and Appendixes B and C). When Z_t is greater than 0, output exceeds its natural rate, indicating expansionary macroeconomic policies. When Z_t is less than 0, output is below its natural rate, indicating contractionary policies. A simple linear feedback rule relating Z_t to the lagged rate of inflation, \dot{P}_{t-1} , is used to define desirable policies:

$$Z_t^* = d\dot{P}_{t-1}, d < 0.$$

Z_t^* denotes the optimal value of Z_t as perceived by sophisticated voters. This rule simply states that the higher the prevailing rate of inflation in period $t-1$, the more contractionary policy should be in period t . The slope parameter, d , will be larger in absolute value the greater voters' aversion to inflation relative to recession. In the long run, adherence to such a rule would lead to zero inflation and growth of output to match its natural rate (ignoring the effects of stochastic shocks).

To measure economic performance, then, it is appropriate to judge policymakers on the basis of how far they deviate from the optimum policy Z_t^* . In our empirical work, a performance measure appropriate for sophisticated voters is provided by:

$$S_t = |Z_t - Z_t^*| = |Z_t - d\dot{P}_{t-1}|.$$

Higher values of S imply worse economic performance. This standard of sophisticated evaluation of economic policy may seem complicated, but it is merely a mathematical approximation of a simple rule: voters punish inflationary and unsustainable expansions, and they accept recessions only as a means of reducing inflation.

Ordinary citizens might well be able to use such a rule without being implausibly sophisticated, because the rule is not far removed from common sense. Consider first the fact that it punishes incumbents when the economy is pushed beyond its capacity, or overheated. We would not expect citizens to demand economic expansion when most of the economy is working at capacity. Furthermore, the idea that too much economic activity may be inflationary is a common aspect of public discourse and presumably not beyond the ordinary citizen. Certainly the demand for expansion would be less when the economy is at capacity than when it is in recession.

⁴See Chappell and Keech (1983).

⁵This simplifies reality, since presidents actually have only partial and indirect control of Q . However, it is

weaker than assuming direct control over unemployment, inflation, and output as other voting models implicitly do. In addition, we later consider modifications of the model which relax this assumption.

The other main feature of our sophisticated decision rule is that it implies that voters will be more tolerant of recession when inflation is high than when it is low. This too is only a minor extension of the widely discussed idea that recessions are likely to bring down inflation. In general, the sophisticated decision rule is an operationalization of the simple idea that citizens do not respond to inflation or income growth indiscriminately, regardless of the value of the other.

For purposes of comparison in the empirical analysis, we will also make use of a simple measure of performance appropriate for naive voters:

$$N_t = \gamma_1 P_t + \gamma_2 Q_t / QN_t.$$

Here performance is simply measured as a linear combination of the inflation rate and the output ratio Q_t / QN_t . The conventional hypotheses would predict that voters would punish higher inflation ($\gamma_1 < 0$) and reward higher output ($\gamma_2 > 0$).⁶

The Political Support Function

Our main concern is to compare naive and sophisticated standards by which voters may evaluate economic performance in order to make inferences about political accountability and popular control of elected officials. Presidential approval polls are especially useful for evaluating these questions. They offer rich time series covering long periods with varied economic conditions. These polls are also useful in that they focus directly on performance. The polls simply ask respondents "Do you approve of the way that _____ is handling his job as president?" whereas voting requires comparisons with alternative candidates who may vary in attractiveness and thus not provide a constant benchmark.⁷

Even though our main interest is in the standards voters use for evaluation of economic performance, we need a fully specified model of political support in order to address this question clearly. In developing such a model, we have drawn on a rich body of literature for guidance about how to handle a variety of issues. These issues may be grouped into two major categories: 1) accounting for the ways that time can influence

the evaluation of performance, and 2) determining how noneconomic determinants of support should enter the model. In this section we introduce the general political support model to be used in the empirical analysis and discuss its implications for these issues. The general form for the sophisticated model is

$$POP_t = f(T_t^*) (\alpha_0 + \alpha_1 \bar{S}_t) + X_t \beta + e_t, \quad (1)$$

whereas that for the naive model is

$$POP_t = f(T_t^*) (\alpha_0 + \alpha_1 \bar{N}_t) + X_t \beta + e_t. \quad (2)$$

\bar{S}_t and \bar{N}_t are weighted averages of the economic performance standards, X_t is a vector of noneconomic variables, and T^* is the number of quarters the administration has been in office.⁸

Time

The Weighting of Different Periods of Economic Performance. In our models the weights attached to lagged values of the economic performance measures reflect two separate phenomena: first, the increasing importance attached to average performance as time in office accumulates, and second, the selection of relative weights for past versus present performance in computing that average. The first of these is represented by the term $f(T_t^*)$. When a president first takes office, citizens have little evidence to aid them in evaluating economic performance. The president will not have had time to put his stamp on economic policy, and there will not be much experience to evaluate. But as time in office accumulates, respondents obtain more information and can be expected to attach greater weight to it, so $f(T_t^*)$ should be an increasing function.

Accordingly, for empirical purposes we specify that

$$f(T_t^*) = \sqrt{T_t^*}.$$

Over eight years, $\sqrt{T_t^*}$ will increase at a decreasing rate from one to four at the end of the first term and almost six at the end of the second. Although this is admittedly a somewhat arbitrary specification, we believe it reflects a sensible way for voters to take into account the unreliability of early information, whatever their standard of evaluation.

The relative weights attached to past and present performance are incorporated into \bar{S}_t and \bar{N}_t .

⁶The output ratio is closely related to variables used in many other support models: output or income growth or the unemployment rate. See Gordon (1981, pp. 297-299).

⁷Of course voters may implicitly make comparisons when responding to the polls, especially when elections are imminent. This may account in part for cyclical patterns in the popularity series which are discussed later.

⁸We will consider the Kennedy-Johnson and Nixon-Ford presidencies as single administrations for this purpose.

Both performance measures are modeled as weighted averages and computed over the history of the administration:

$$\bar{S}_t = \frac{\sum_{k=0}^{31} |z_{t-k} - d\dot{p}_{t-k-1}| D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \quad (3)$$

$$\bar{N}_t = \frac{\sum_{k=0}^{31} (\gamma_1 \dot{p}_{t-k} + \gamma_2 Q_{t-k}/QN_{t-k}) D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \quad (4)$$

where $D_{t,t-k} = 1$ if the administration in office in quarter t was in office in quarter $t-k$, $D_{t,t-k} = 0$ otherwise, and $0 \leq g \leq 1$.

The dummy variables $D_{t,t-k}$ assure for both models that the performance of previous administrations does not count in the evaluation of incumbent administrations. The parameter g attaches weights to past outcomes, allowing voters to forget or discount the past. If $g = 1$, each performance measure is a simple average over all quarters of the current administration. If $g = 0$, only the current quarter is evaluated. All values of g between zero and one imply weights that decline geometrically as performance recedes further into the past.

The extent to which voters remember or weight past periods is of limited consequence in the sophisticated voter model, because in each period the performance measure takes into account what kinds of policies are sustainably feasible and therefore the relationship between present policy and the future. The sophisticated voter is not vulnerable to manipulation by vote-seeking incumbents, regardless of the length of his memory.

However, for the naive voter model, the length and weighting of the evaluation period have more important implications. A long memory would reduce the incentives for incumbents to induce early term recessions or preelection expansions whose inflationary costs are postponed until after the election. But a long memory is only a partial substitute for a sense of feasibility constraints. To be invulnerable to this kind of manipulation, the voter needs the kind of ability to anticipate the future consequences of present policies which is provided for by our sophisticated voter standard.

Our political support function is novel in distinguishing voter memories from the weight assigned to the incumbent's performance over time. In other models (Chappell, 1983; Hibbs, 1982a, b, c), geometrically declining weights were specified for periods further in the past, in order to reflect voter discounting or forgetting of past

outcomes. But by not separately specifying the possibility that average performance might be weighted more heavily as time passes (regardless of the pattern of weights used in computing the average), previous models have confounded the two patterns.

Honeymoons and Other Time Trends. Our $f(T^*)$ function imposes an assumption that voters attach a lesser weight to average performance early in an administration's term. We also expect that voters may offer new incumbents a honeymoon period early in a new term, such that the new president will be popular for a time in the absence of a performance record that can be taken seriously. Our model permits the possibility that voters will give a new president a benefit of the doubt until he has a record that can be evaluated.

We model the early-term honeymoon by including in the vector X_t six dummy variables indicating each of the first six quarters in office for a new administration:

$P_k = 1$ if the current administration has been in office for exactly k quarters, otherwise $= 0$; $k = 1, 2, \dots, 6$.

Inclusion of the series of dummies allows flexibility in the specified pattern of honeymoon effects, subject only to the restriction that honeymoon effects are exhausted after six quarters in office. This restriction seems reasonable, because midterm elections are imminent at this point. We expect the coefficients associated with these dummies to be large in the first quarter and to decline as time in office accumulates.⁹

The final time-related feature involves the parameter α_0 , which permits the model to pick up secular trends in popularity, and, following Kernell (1978, p. 520) serves as a "diagnostic for detecting whether any trend producing variables have been left out." This choice is to be contrasted with those of Mueller (1970, 1973) and Stimson (1976), who used functions of time as direct measures of variables meant to allow tests of abstract theories explaining change in presidential popularity. It is to be contrasted also with Hibbs (1982a, b, c), who does not include any trend or independent honeymoon variable, but models performance in such a way that any trends would be picked up in the performance terms.¹⁰

⁹We count the first quarters of Johnson's and Ford's presidencies as new terms for this purpose.

¹⁰However, Hibbs and Vasilatos (1981) include trend variables. In an exchange following (Hibbs, 1982b) he defends his view that any honeymoon must be performance related.

Noneconomic determinants of popularity

We propose that two other dimensions are of sufficient importance that they deserve inclusion in the model: war, peace, and international security, and the corruption or rectitude of the administration. Unfortunately, the relevant data from which indexes of performance on these determinants of popularity might be derived are not easily identified, and neither dimension is as continuously monitored as economic performance is.

For the international security dimension, the presence and intensity of a war effort are surely among the most salient indicators of performance, but how they should be evaluated is less obvious. For a pacifist, war is an unambiguous bad, but for others there may be circumstances under which a war effort is a basis for approval rather than disapproval. Of course, some wars are more popular than others, and the amount of support for a war is likely to be dependent on its cost (Mueller, 1973, chaps. 7, 3).

There was only one war, Vietnam, in the period of our study, and it is widely acknowledged to have had a negative impact on support for the presidents under which it was fought. We include a measure for war and its costs in the form of a quarterly measure of the number killed in action in Vietnam.

For the corruption dimension, the Watergate affair is obviously the most important phenomenon. For lack of a better way to quantify the varying intensity of the corruption issue, we have simply used a dummy variable equal to one for 1973 IV through 1974 III, the period from the Saturday night massacre until Nixon's resignation.

Both of these choices are consistent with the handling of noneconomic issues by other analysts. In contrast to Hibbs (1982a), however, we have chosen to use only current values of the noneconomic variables. Vietnam and Watergate are qualitatively different from economic performance, and these differences may make it sensible to evaluate past values of each in different ways. The problem of managing the economy recurs in much the same form in every period. In contrast, political problems are more often unique and nonrecurring, so that past performance may not have much predictive power regarding how future problems will be handled. Although we specify only a current effect of political variables in the models developed in the text, an alternative assumption is employed in sensitivity tests to be described in the Appendix.

Several studies have included measures of more idiosyncratic events, usually designated as rally points. We choose not to do so because we expect these events to have varying effects. In the

absence of further a priori information about anticipated effects, we believe that rally events should be viewed as random perturbations which do affect popularity, but in unpredictable ways. Such variables are appropriately left in the equation's error term.

Finally, we include an intercept dummy for each president which is meant to estimate his initial popularity level. These, along with the Vietnam war, Watergate, and honeymoon variables, are included in the vector, X_t , of noneconomic variables.

Empirical Analysis

In this section we describe the results of estimating political support functions of the general form described in the preceding section. Measures of economic performance appropriate for naive and sophisticated voters will be used alternatively, and their relative abilities to explain presidential popularity will be noted. In addition, we summarize the results of sensitivity tests undertaken to see if the major implications of our tests are robust with respect to modifications of the political support function.

The dependent variable, POP_t , is the fraction of Gallup Poll respondents who answered favorably to the approval question quoted above. POP_t is computed as an average of polls taken during the appropriate quarter. In addition to estimating equations to explain aggregate poll data, we have also estimated equations for specific subgroups based on occupational, educational, and partisan classifications. The analysis of these disaggregated groups is intended to provide additional information about the general applicability of the models, as well as to identify different preferences across groups. The time series analyzed for the aggregate poll data covers the 1957 I to 1980 IV period, whereas the group data cover somewhat shorter subperiods.¹¹ Q and QN are measured respectively by real GNP and Gordon's estimates of natural GNP. The inflation series is an annualized rate computed from the GNP deflator.¹² The remaining variables were described in the preceding section.

The equations to be estimated are found by

¹¹The data for partisan and occupational groupings cover the 1961I to 1979IV period; the data for educational groups cover the 1965I to 1980IV period. Each of these series lacks observations for several quarters when polls were not taken.

¹²The data series for QN is from a mimeographed supplement to Gordon (1981). The supplement reflects revisions made in the data series after publication of the book. Other economic data are from the CITIBASE Economic Database.

substituting equation (3) into equation (1) and equation (4) into equation (2). The sophisticated voter model is

$$POP_t = \sqrt{T_t^*} \left[\alpha_0 + \frac{\alpha_1 \sum_{k=0}^{31} |Z_{t-k} - dP_{t-k-1}| D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \right] + X_t \beta + e_t, \quad (5)$$

and the naive voter model is

$$POP_t = \sqrt{T_t^*} \left[\alpha_0 + \frac{\sum_{k=0}^{31} (\gamma_1 P_{t-k} + \gamma_2 Q_{t-k}/QN_{t-k}) D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \right] + X_t \beta + e_t, \quad (6)$$

where $\gamma_1' = \alpha_1 \gamma_1$ and $\gamma_2' = \alpha_1 \gamma_2$. These equations are nonlinear in the parameters, but can be estimated by nonlinear least squares. In estimating the sophisticated voter models, convergence problems forced us to impose values for the weighting parameter g and then estimate the remaining parameters using a nonlinear least squares algorithm. A finely subdivided grid of possible values for g was examined, and results are reported for the value of g which minimized the sum of squared residuals. This procedure provides nonlinear least squares estimates, but does not yield an asymptotic t -statistic for the estimate of g .¹³ Because our results indicated that serial correlation was generally present, in all cases we have reestimated the equations correcting for the presence of first-order serial correlation.¹⁴ For the aggregate data we present both corrected and uncorrected equations, but to conserve space, only

the corrected equation estimates are provided for the group data regressions.¹⁵

Table 1 provides complete regression results for sophisticated and naive voter models using aggregated popularity data; estimates of the key economic parameters and summary statistics are provided in Table 2 for the group regressions.¹⁶ The results show strong support for the sophisticated voter models. In all equations for aggregate and group data the parameter α_1 is negative, indicating that voters punish suboptimality as defined by our sophisticated standard. In addition, the estimates are significantly different from zero at the .05 level or better in most cases. Estimates of d are always negative and large in absolute value, indicating strong aversion to inflation. Indeed in some cases the estimates of d are probably unreasonably large. For example, in the corrected equation for the aggregate data, the value of $d = -.019$ would imply that if last period's inflation rate were 10%, voters would like to see this period's real output be 19% below its natural rate in order to combat the inflation. The estimates of d also tend to be large in the group equations.

Although the excessive magnitude of the d estimates tempers support for the sophisticated voter model, it certainly does not refute it. Although the estimates are large, so are standard errors; d is significantly different from zero at the .10 level in only about half of the equations and is never significantly different from what we would consider plausible values for d (for example $d = -.010$). Hence the results are quite consistent with a priori expectations.¹⁷

¹³Several econometric caveats should be noted. First, the popularity series are missing several observations (for quarters in which no polls were taken), which implies that the correction for serial correlation is only approximately correct. Since there were generally only a few missing observations, this is of minor consequence. Second, the reported standard errors are based on an implicit assumption that values for ρ , the error term correlation coefficient, and g (when g is determined via a grid search) are the true values of those parameters, rather than estimates. The reported t -statistics may therefore be somewhat inflated.

¹⁴Estimates of the remaining parameters have been excluded to conserve space. There were no important differences between group and aggregate regressions relating to these parameters.

¹⁵Several other points could be made regarding the size of the d estimates. First, collinearity problems are likely in a model as complex as this one, and there is evidence that this may partially explain some unusually large values for d . The variance-covariance matrix of the estimated coefficients typically shows that the correlation between estimates of d and α_1 are large and negative, typically on the order of -0.85 , so collinearity is a possible problem. Second, some of the naive voter

¹³In a few regressions it was necessary to search a grid over both g and d . The absence of a t -statistic for d in the tables indicates that this was the case.

¹⁴A two-step procedure was used. First, the residuals of the uncorrected nonlinear least squares estimates were used to obtain an estimate of the error term correlation coefficient. then we reestimated assuming the presence of first order serial correlation with the error term correlation coefficient set equal to the calculated estimate.

Table 1. Aggregate Popularity Functions

	Sophisticated	Naive	Sophisticated (Corrected)	Naive (Corrected)
α_0	-0.024 (-1.492) ^a	-0.314 (-2.774)*	-0.025 (-1.205)	0.044 (.186)
α_1	-0.528 (-2.900)*	— —	-0.387 (-2.127)*	— —
\dot{P}	— —	-5.36×10^{-3} (-2.909)*	— —	-8.12×10^{-3} (-2.446)*
Q/QN	— —	.298 (2.558)*	— —	-0.061 (-.254)
P1	0.078 (2.007)*	0.114 (2.820)*	0.074 (1.649)*	0.075 (1.545)
P2	0.071 (2.000)*	0.113 (3.004)*	0.064 (1.593)	0.072 (1.611)
P3	0.028 (.776)	0.062 (1.616)	0.020 (.526)	0.027 (.637)
P4	0.043 (1.392)	0.066 (2.055)*	0.039 (1.169)	0.039 (1.077)
P5	0.048 (1.615)	0.079 (2.566)*	0.043 (1.406)	0.052 (1.575)
P6	0.002 (.065)	0.035 (1.190)*	-0.003 (-.111)	0.009 (.327)
Watergate	-0.152 (-3.873)*	-0.166 (-4.007)*	-0.137 (-3.110)*	-0.130 (-2.825)*
Killed in Vietnam	-1.56×10^{-5} (-1.732)*	-3.66×10^{-5} (-4.604)*	-1.60×10^{-5} (-1.577)	-2.29×10^{-5} (-2.185)*
Intercept dummies				
Eisenhower	0.823 (11.396)*	0.778 (9.544)*	0.831 (8.823)*	0.792 (7.823)*
Kennedy	0.758 (15.819)*	0.732 (13.713)*	0.753 (12.412)*	0.731 (11.223)*
Johnson	0.804 (10.756)*	0.697 (7.911)*	0.812 (8.371)*	0.764 (7.183)*
Nixon	0.744 (14.306)*	0.678 (12.290)*	0.745 (11.270)*	0.727 (10.072)*
Ford	0.796 (7.706)*	0.783 (7.966)*	0.842 (6.723)*	0.857 (7.018)*
Carter	0.638 (11.344)*	0.615 (11.656)*	0.658 (9.476)*	0.665 (9.496)*
g^b	0.870 —	0.518 (2.617)*	0.850 —	0.835 (1.278)
d	-0.013 (-2.324)*	— —	-0.019 (-1.591)	— —
SSR	0.218	0.231	0.188	0.196
R^2	0.850	0.841	—	—
D.W.	1.210	1.230	—	—
ρ	—	—	0.395	0.385

*Significant at .10 level or better.

^aAsymptotic t -statistics are in parentheses.^bTest of $H_0: g = 1$.

Table 2. Disaggregated Group Popularity Functions

	White Collar	Blue Collar	Non Labor Force	Republican	Democratic	Independent	College	High School	Grade School
Sophisticated voter models									
α_0	-0.031 (-1.103) ^a	-0.033 (-1.286)	-0.015 (-.511)	6.74×10^{-3} (.216)	-0.036 (-1.645)*	-0.025 (-.904)	-8.48×10^{-3} (-.180)	0.013 (.287)	0.037 (.716)
α_1	-0.694 (-3.536)*	-0.672 (-3.635)*	-0.422 (-2.376)*	-0.494 (-3.566)*	-0.644 (-3.714)*	-0.376 (-3.039)*	-0.452 (-1.303)	-0.392 (-1.187)	-0.348 (-.900)
d	-8.69×10^{-3} (-3.150)*	-9.90×10^{-3} (-2.471)*	-0.015 (-1.612)	-0.011 —	-0.010 (-2.338)*	-9.95×10^{-3} (-1.948)*	-0.020 (-1.112)	-0.023 (-1.052)	-0.032 (-.841)
g^b	0.700 —	0.770 —	0.690 —	0.660 —	0.790 —	0.720 —	0.790 —	0.800 —	0.820 —
SSR	0.187	0.152	0.127	0.210	0.127	0.197	0.158	0.140	0.182
ρ	0.305	0.290	0.230	0.335	0.160	0.210	0.450	0.450	0.440
Naive voter models									
α_0	0.708 (2.800)*	0.720 (2.350)*	0.654 (2.142)*	0.815 (3.769)*	0.669 (2.075)*	0.590 (2.163)*	0.290 (.638)	0.335 (.711)	0.354 (.608)
\hat{P}	-6.27×10^{-3} (-2.279)*	-7.64×10^{-3} (-2.357)*	-4.49×10^{-3} (-1.372)	-3.65×10^{-3} (-1.165)	-8.14×10^{-3} (-2.597)*	-7.42×10^{-3} (-2.500)*	-0.011 (-1.779)*	-9.68×10^{-3} (-1.528)	-0.012 (-1.484)
Q/QN	-0.721 (-2.871)*	-0.736 (-2.429)*	-0.665 (-2.219)*	-0.800 (-3.783)*	-0.690 (-2.158)*	-0.593 (-2.193)*	-0.271 (-.631)	-0.310 (-.693)	-0.301 (-.545)
g^b	0.736 (2.390)*	0.828 (2.011)*	0.795 (1.599)	0.740 —	0.854 (1.861)*	0.744 (2.169)*	0.786 (1.295)	0.820 (1.084)	0.851 (.977)
SSR	0.175	0.147	0.124	0.186	0.128	0.181	0.149	0.140	0.183
ρ	0.255	0.270	0.245	0.330	0.130	0.190	0.495	0.455	0.445

*Significant at .10 level or better.

^a Asymptotic t -statistics are in parentheses.^b Test of $H_0: g = 1$.

The predicted consequences of following sub-optimal policies are reasonable in magnitude. Estimates from column 3 in Table 1 imply that if an administration were to deviate consistently from the optimal value of Q/QN by .05 over a four-year period, its rating would be 7.7 points lower than if it had followed optimal policies.

Results of the naive model regressions reported in columns 2 and 4 are somewhat weaker. In the uncorrected equation for the aggregate data in Table 1, signs on the output and inflation variables are properly signed and significant, but the sum of squared residuals is larger than for the corresponding sophisticated voter model. In the equation corrected for serial correlation, the inflation variable is again properly signed and significant, but the output variable has a surprising negative sign and is not significant. Results from the group equations are even more troubling. In those equations inflation generally has the anticipated negative effect on popularity; however, the output variable is always incorrectly signed, often significantly so. Although sums of squared residuals (corrected for the error process) are lower in some naive equations, this is largely attributable to the implausible negative coefficients on the output variable. When output coefficients are constrained to be non-negative, sums of squared residuals are lower for sophisticated voter models than for naive voter models in 9 of 10 comparisons, although differences are not typically large.

Noneconomic variables perform much as expected in all models. In the sophisticated voter models, the parameter α_0 is usually negative, but not significant. The negative sign implies that even if a president followed optimal policies, there would be a downward trend in his popularity over the course of his stay in office. The literature provides several possible explanations for such secular trends,¹⁸ and most empirical investigations have found evidence of them. In the naive voter model, the interpretation of α_0 differs,¹⁹ but for

models in the literature also indicate extreme inflation aversion, so the findings here are consistent with previous ones. See for example Hibbs (1982a), who finds that Republicans have a marginal rate of substitution of 4.3 points of unemployment per point of inflation. This would indicate that Republican voters would be willing to endure 43% unemployment in order to eliminate a 10% inflation. Finally, we find that the overall performance of the model is strong even if we constrain the inflation aversion parameter to assume smaller and more theoretically plausible values.

¹⁸See Mueller (1970, 1973) and Stimson (1976).

¹⁹This is because N_t , as defined in equation (6), does not take on a value of zero at an optimal policy choice. Indeed, the naive model has no well-defined optimal policy choice, and what it regards as good policies might not be sustainable over the course of a term in office.

most plausible values of the economic variables that model too usually implies secularly declining popularity. Table 1 shows that the series of early term dummies pick up honeymoon effects which are often significant, but which appear to be exhausted by the sixth quarter in office. The Vietnam war variable and the Watergate dummy are negatively signed as expected. The honeymoon, Vietnam, and Watergate variables have consistently similar effects in the group regressions.²⁰ The memory parameter g takes on values ranging from .66 to .85 in the various models, which indicates some discounting of the past, and most magnitudes are roughly similar to those found by Hibbs (1982a, b, c).

None of the results suggests striking behavioral differences among diverse groups. Whether this indicates the absence of group-related differences in preferences or simply the inability of the empirical analysis to make such fine distinctions remains questionable.²¹

We have argued that our political support function is a sensible one; however, a number of its features reflect choices for which there exist reasonable alternatives. In order to assess the robustness of our major findings concerning the sophisticated and naive standards of performance, we have estimated additional equations in which specific features of the model are modified, or alternative data sources are employed. Because of the substantial computing requirements, we have used exclusively the aggregate popularity data in these sensitivity tests. The modified models make the adjustments noted below.

1. Alternative formulations are used for $f(T)$.
2. Weighted averages of past values of "killed in Vietnam" are used instead of current values.
3. Partial adjustment in the control of output is assumed to reflect imperfect control of policymakers over the resulting outcomes.
4. Nominal rather than real output is assumed to be the control variable for policymakers.
5. Alternative data replace the estimates of natural rates of output in the model.
6. The models are estimated for subperiods of the 1957-1980 time span.
7. Alternative formulations of the Watergate effect are specified.

²⁰The naive models do consistently indicate a larger effect of killed in Vietnam than do sophisticated voter models. This could reflect collinearity between the war and excessively expansionary economic policies in the sophisticated model.

²¹Our results here differ from those of Hibbs (1982a), who concluded that groups' "objective" economic interests were reflected in his estimates of marginal rates of substitution of inflation for unemployment.

8. A quadratic performance measure is used in the sophisticated voter model.

Complete explanations and detailed results of the sensitivity tests are provided in the Appendix.

Results of the sensitivity tests generally reinforce our earlier conclusion that the sophisticated voter hypothesis performs well. In all of the modified sophisticated voter models, the results indicate that suboptimal performance is punished, and the crucial economic parameter α_1 is usually statistically significant. In some models, estimated inflation aversion is quite strong, but the relevant parameter d is never significantly different from plausible values. None of the modified models substantially outperforms the model presented in the text; all of the sophisticated voter models perform comparably well. The one result that was most damaging to the sophisticated voter hypothesis was that although the sophisticated voter model outperformed the naive for the 1957 to 1968 subperiod, the reverse was the case for 1969 to 1980. Thus, the sensitivity testing does not suggest a single superior model, but does basically confirm the robustness of earlier conclusions.

Conclusions

Two central themes emerge from our analysis. First, given the nature of the constraints on possible macroeconomic outcomes, sensible evaluation of economic performance is a little more complicated than is usually recognized in the study of political support. The way that most support models assume that voters evaluate economic performance implies naivete and vulnerability to manipulation. We have conceptualized and presented an alternative model which assumes that voters are more sophisticated.

Second, in empirical tests, the sophisticated voter model performs well by normal standards. Indeed, with very few exceptions it performs consistently better than the more conventional model, which implicitly assumes naivete.

These two themes are independent. The first set of observations would stand in the absence of empirical support for the sophisticated voter model, and although we find the empirical work remarkably supportive of the hypothesis that voters are sophisticated, that work should be viewed as establishing the credibility of an alternative view, rather than as providing conclusive support for it.

We cannot, of course, say anything directly about the cognitive processes of the voter, but whatever those processes involve, we can say on the basis of our evidence that popular support of incumbent presidents is consistent with a sophisticated standard of evaluation.

The consistently good performance of the

sophisticated voter model does illuminate sometimes puzzling features of the existing literature on politics and macroeconomics. For example, several studies of political support have reported unstable coefficients on unemployment and on income growth, often with the wrong or at least the unexpected sign, given the typical assumption that unemployment is always punished and growth is always rewarded.²²

These findings are not so surprising when viewed with the central insight of the sophisticated voter model, that is, that since economic variables are empirically interdependent, the evaluation of any one of them may not be independent of the others. Specifically, although economic slack is in general undesirable, it may occasionally be accepted in times of high inflation in order to restore price stability.²³

Similarly, several studies have failed to find evidence of the political business cycles one would expect of politicians maximized a naive, retrospective voting function like that which typifies the literature.²⁴ MacRae (1977) found indirect evidence that some politicians might believe or assume that voters are strategic, but we provide direct evidence that voting behavior may be sophisticated. Political business cycles may not exist in part because voters do not offer an incentive for politicians to induce them.

Our findings speak also to the rationales for retrospective voting articulated by Key and Downs, and to related issues of rationality and accountability. Key defended a process in which voters were expected to do little more than vote against incumbents who had displeased them. He articulated no particular standard of rationality and said nothing about the ability to predict the future from past performance. "The vocabulary of the voice of the people consists mainly of the words 'yes' and 'no'; and at times one cannot be certain which word is being uttered" (1964, p. 544).

Downs expected more of voters than Key, including an ability to use the past to predict the

²²See, for example, Arcelus and Meltzer (1975), Keniski (1977), Powell (1982), and Stigler (1977).

²³Ironically, what may have been taken as an indicator of irrationality on the part of voters may be evidence that voters follow a higher standard of rationality than the designers of the studies of voting behavior. This would have delighted E. E. Schattschneider, who asked, "Who are these self-appointed censors who assume that they are in a position to flunk the whole human race?" He also observed that "political research is never better than the theory of politics on which it is based" (1960, pp. 135, 133).

²⁴See, for example, Beck (1982), Golden and Poterba (1980), and Monroe (1983).

future, but he seems not to have expected more than a simple projection of past to future performance. He did not anticipate that such a projection might not be a good predictor.

The political business cycle literature and an understanding of macroeconomic dynamics tends to undermine both Key and Downs by suggesting that retrospective voting can be a poor predictor of the future and can make voters vulnerable to manipulation. On the same grounds, evidence that voters reward and punish incumbents on the basis of simple retrospective measures of recent economic performance should not be taken to indicate the collective rationality of the electorate or the accountability of public officials.²⁵

But models representing voters as more sophisticated and less vulnerable to manipulation have not been rejected; they have not previously been tried. Our evidence that voters may not be naive suggests that Key and Downs are not so much undermined as in need of minor refinement. In some respects, our analysis brings the two perspectives together into a more comprehensive view of retrospective voting.

Whether or not retrospection is meant to predict the future or merely to hold incumbents accountable for the past, the standard of evaluation used is important. Insofar as retrospection is based on reasonable standards, it makes little difference whether the main interest is backward looking or forward looking. The incentives will be the same for the politician; good performance in term one will be compatible with good performance in term two and so on. When we modify Key and Downs to rule out simple standards which are misleading about the future, the difference between the two views is not very great. But without such modification in the context of macroeconomic issues, neither one provides a fully satisfactory basis for a theory of voting that would make public officials accountable for their performance.

Appendix

This appendix provides detailed descriptions of sensitivity tests in which modified versions of our popularity functions have been estimated using aggregate data. For each test we briefly describe the rationale for the modifications made and report the relevant results.

1) We have used alternative formulations for $f(T_i^*)$, which attaches weights to cumulative economic performance at different points in time. The model in the text assumes that $f(T_i^*) = \sqrt{T_i^*}$.

This function increases as T_i^* increases, but does so at a decreasing rate. A number of other functions also exhibit these properties, however. In sensitivity testing we have generalized by letting $f(T_i^*) = (T_i^*)^\omega$ and have used various values of ω between 0 and 1. The major conclusions noted in the text hold up when the model is generalized in this manner. The lowest sum of squared residuals for the sophisticated and naive voter models is obtained when $\omega = 1$, that is, when $f(T_i^*) = T_i^*$, so that the weight attached to performance increases linearly with time in office. Results for these regressions are provided in Table A1. The sophisticated voter model has a lower sum of squared residuals than the naive voter model for all values of ω for which we estimated equations.

2) In our original model, only current values of number killed in Vietnam were assumed to affect popularity. Because voters may also remember and attach importance to past values of this variable, we have also estimated models in which weighted averages of past values affect popularity. The parameter g determines weights used to compute the average for killed in Vietnam, as well as for the economic variables. As Table A2 shows, this change resulted in a smaller coefficient for killed, but slightly increased the sum of squared residuals relative to the original specification of the sophisticated voter model.

3) Like most previous models, our model assumes that policymakers directly control real output. In reality that control is indirect, operating through monetary and fiscal policies, and it is imperfect because of exogenous shocks, lags in

Table A1. $\omega = 1$

	Sophisticated	Naive
α_0	-2.31×10^{-3} (-0.789)	-3.44×10^{-3} (-0.92)
α_1	-0.070 (-2.170)*	-
\dot{P}	-	-1.52×10^{-3} (-2.617)*
Q/QN	-	1.67×10^{-3} (.045)
g^a	0.820 -	0.772 (1.663)*
d	-0.020 (-1.850)*	-
SSR	0.184	0.190
ρ	0.425	0.425

*Significant at .10 level or better.

^aAsymptotic t -statistics are in parentheses.

^bTest of $H_0: g = 1$.

²⁵For examples of such inferences, see Tuft (1975, p. 826) and Powell (1982, pp. 19-26).

Table A2. Weighted Averages of Killed in Vietnam Included

	Sophisticated	Naive
α_0	-0.018 (-0.902) ^a	-0.301 (-2.128)*
α_1	-0.446 (-2.059)*	-
\dot{P}	-	-5.42x10 ⁻³ (-2.377)*
Q/QN	-	0.290 (1.992)*
g^b	0.850 -	0.606 (2.124)*
Killed in Vietnam	-1.85x10 ⁻⁶ (-400)	-7.03x10 ⁻⁶ (-2.922)*
d	-0.017 (-1.787)*	-
SSR	0.194	0.195
ρ	0.385	0.395

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

implementation, and lags in effectiveness. We have investigated a modification of the model in which voters recognize that there is "inertia" to be overcome in adjusting Q , and that there might be substantial costs associated with overcoming that inertia too quickly. Specifically, we build a partial adjustment process into the optimal feedback rule used to define desirable policies.

As before, let Z_t^* refer to that value of Z_t judged best without any consideration of the costs of overcoming inertia. As earlier noted, $Z_t^* = d\dot{P}_{t-1}$. Now, let Z_t^{**} refer to a "second-best" value of Z_t , that is, the policy judged to be best given the existence of inertia in policymaking. We assume that Z_t^{**} is defined by:

$$Z_t^{**} - Z_{t-1} = \phi(Z_t^* - Z_{t-1})$$

where $0 \leq \phi \leq 1$. This equation says that in a given period we should move Z toward its no-inertia optimum Z^* , but because of the costs of changing Z rapidly, we should move it only part of the way. The parameter ϕ indicates how closely the second-best optimum approaches the no-inertia optimum. Measuring performance as the absolute value of the deviation of optimal from actual performance:

$$S = |Z_t - Z_t^{**}|$$

$$= |Z_t - (1 - \phi)Z_{t-1} - \phi\dot{P}_{t-1}|$$

This alternative measure of S_t is substituted into the popularity function. Possibly because of collinearity problems resulting from the added complexity of this model, unconstrained estimates of d and ϕ were not economically meaningful. However, when plausible values for d and ϕ were imposed α_1 was negative and highly significant, indicating punishment for suboptimality. Table A3 provides results assuming $\phi = .8$ and $d = -.010$.

4) As an alternative modification of the assumption that Q is a control variable, we estimate a model where nominal output is assumed to be the policy instrument. In practice, policymakers have considerable ability to control the growth of nominal output, but much less control over how that growth is divided between changes in output and changes in prices.

For this model, we change our measure of policy stance Z to the following:

$$Z_t = (NOMQ_t / EXNOMQN_t) - 1$$

where $NOMQ$ is nominal GNP and $EXNOMQN$ is the value nominal GNP would take if Q were equal to QN and if the inflation rate were to continue to follow current trends.²⁶ If policymakers were to set $NOMQ$ equal to $EXNOMQN$, this would be consistent with achieving real output of QN and not altering the inflation rate. Higher values of $NOMQ$ would be expansionary, whereas lower values of $NOMQ$ would be contractionary.

Estimates of this model are provided in Table A4. All results are consistent with hypotheses and

²⁶Trend rates of inflation are computed as predicted values from a regression of inflation on its last four lagged values.

Table A3. Partial Adjustment Model

α_0	-0.026 (-1.249) ^a
α_1	-0.759 (-3.572)*
ϕ	0.800 -
g^b	0.890 -
d	-0.010 -
SSR	0.194
ρ	0.390

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

are very similar to these obtained with the original model.

Table A4. Nominal Q as a Control Variable

α_0	-0.024 (-1.169) ^a
α_1	-0.379 (-2.190) ^a
g^b	0.850
d	-0.019 (-1.660) ^a
SSR	0.187
ρ	0.400

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

5) We have used alternative data sources to construct measures of policy stance. These alternative runs permit us to determine if the results are sensitive to the use of particular sources of data. In place of Gordon's natural rates of GNP, we have substituted estimates of "high-employment" GNP computed by Tatom (1982). We also estimate models in which the difference between

unemployment and its natural rate²⁷ replaces $Q/QN - 1$ as the measure of policy stance. Using Tatom's measures of high-employment GNP in place of QN , the model provides a slightly better fit than the model in the text, and all results are qualitatively similar. Table A5 shows that the inflation aversion parameter is slightly lower ($d = -.016$) than that in Table 1, and the estimate of α_1 remains negative and is more highly significant than in Table 1.

Using $U - UN$ in place of Z to measure policy stance also slightly improved the overall fit and led to results consistent with hypotheses. (Note that the hypothesized sign for d is positive in this model.) These results are also provided in Table A5 as are estimates of a naive model in which the unemployment rate replaces Q/QN as a measure of stimulus. In that model the unemployment variable was incorrectly signed and the sum of squared residuals remained slightly higher than for the sophisticated voter model.

6) We have estimated sophisticated and naive voter models for sub-samples of the period from 1957 to 1980. This permits us to see if the models perform consistently well in different periods and may also show whether there are perceptible changes in respondent preferences in periods

²⁷The natural rates of unemployment are also obtained from the mimeographed supplement to Gordon (1981).

Table A5. Alternative Data

	Potential GNP replaces QN	$Z = U - UN$	Naive with Unemployment
α_0	-0.022 (-1.128) ^a	-0.029 (-1.477)	-0.032 (-.781)
α_1	-0.605 (-2.737) ^a	-7.80×10^{-3} (-2.068) ^a	-
\dot{P}	-	-	-8.19×10^{-3} (-2.271) ^a
U	-	-	2.85×10^{-3} (.448)
g	0.830	0.810	0.853 (1.111)
d	-0.016 (-3.364) ^a	0.848 (1.897) ^a	-
SSR	0.179	0.186	0.194
ρ	0.365	0.370	0.425

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

Table A6. Subsample Estimates

	Sophisticated			Naive	
	1957-68	1969-80	1969-80 (d constrained)	1957-68	1969-80
α_0	-0.059 (-2.307)**	-0.031 (-.895)	-0.013 (-.318)	4.950 (3.207)*	-0.329 (-1.098)
α_1	-0.711 (-3.719)*	-0.290 (-1.434)	-0.366 (-2.671)*	—	—
g^b	0.850	0.410	0.750	1.089 (5.517)*	0.655 (-1.404)
\dot{P}	—	—	—	0.028 (.758)	-8.51x10 ⁻³ (-1.533)
Q/QN	—	—	—	-5.112 (-3.197)*	0.334 (1.187)
d	-0.010	0.019 (1.222)	-0.010	—	—
SSR	0.064	0.083	0.096	0.059	0.084
ρ	0.435	0.290	0.345	0.565	0.385

*Significant at .10 level or better.

*Asymptotic *t*-statistics are in parentheses.^bTest of $H_0: g = 1$.

where economic conditions differed considerably.

The sophisticated voter model performs as hypothesized for the 1957-1968 sample, but the inflation aversion parameter was incorrectly signed in the 1969-1980 equation. However when d was constrained to take a theoretically plausible value ($d = -.010$) in the latter sample period, then the estimate of α_1 was negative, as hypothesized, and significant. In contrast, the naive model performs poorly for the 1957-1968 period, with both the inflation and unemployment variables incorrectly signed. However, for the latter period the signs are reversed, and the sum of squared residuals is lower than for the sophisticated model. These results are provided in Table A6.

7) We have used alternative measures for the Watergate variable. These measures varied both the length of the effect of Watergate and the possibility that a trend (rather than a dummy) would better capture the cumulative effects of the Watergate revelations. Results are shown in Table A7, in which the Watergate effect is defined as a trend beginning with Nixon's second term and continuing until he left office. All of the substantive results were unchanged regardless of how we defined the Watergate variable, although the results in Table A7 do provide a slightly lower sum of squared residuals.

8) As a performance measure we have used the squared deviations of actual from "optimal"

policies instead of the absolute values of those deviations:

$$S_t = (Z_t - Z_t^*)^2.$$

In an earlier version of this article we used this quadratic performance measure throughout and

Table A7. Alternative Watergate Variable Specification

	Sophisticated	Naive
α_0	-0.015 (-0.681)	0.083 (.324)
α_1	-0.357 (-1.855)*	—
d	-0.019 (-1.525)	—
g^b	0.860	0.852 (1.099)
Watergate trend	-0.037 (-3.655)*	-0.035 (-3.467)*
SSR	0.178	0.187
ρ	0.450	0.380

*Significant at .10 level or better.

*Asymptotic *t*-statistics are in parentheses.^bTest of $H_0: g = 1$.

Table A8. Quadratic Performance Measure

α_0	-0.029 (-1.451) ^a
α_1	-0.838 (-1.837)
g^b	0.810
d	-0.039 (-1.676)*
SSR	0.184
ρ	0.425

Significant at .10 level or better.

^aAsymptotic t-statistics are in parentheses.

^bTest of $H_0: g = 1$.

obtained results quite similar to those presented here. In Table A8 we report estimates for the sophisticated model based on this performance measure. Although the estimate of d is especially large and α_1 is not significant, the parameters are properly signed.

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A Reevaluation of Realignments in American Politics: Evidence from the House of Representatives

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This article reevaluates American realignment literature based on Clubb, Flanigan, and Zingale's (1980) admonition to focus on control of government and political leadership rather than electoral results. I put forward a theory of policy change in the House of Representatives which shows, like Sinclair (1977), that the effect of electoral realignments is to create a strong and unified majority party in the Congress. However, unlike other work that focuses on electoral courses, I show that structural features of elections created the new majority party in both the Civil War and the 1890s realignments. Specifically, I argue that in these two realignments a strong regional seats-to-votes distortion created the Republican majorities that enacted the policy changes associated with these realignments.

The structural arrangements of the American political system were designed to limit any majority's ability to legislate major policy shifts. Federalism, separation of powers, and checks and balances are impediments to decisive majority decisions. Dahl (1956), Orfield (1975), and Huntington (1965), among others, have discussed the effects of these structural arrangements on policy-making in the U.S. Congress. In an attempt to show how these cumbersome governmental structures have at times yielded significant policy changes, a full generation of political scientists and historians has been schooled in the notion that realigning elections are the mechanisms that account for major policy shifts. Those who study realignments have claimed much. V. O. Key, Jr. (1955) wrote that in realignments "more or less profound readjustments occur in the relations of power within the community." Key's student, Walter Burnham (1970), refers to critical elections as "the mainsprings" of American politics. The terminology of the realignment perspective has also been adopted by traditional historians and by journalists such as David Broder to the extent that American political history is divided in terms of realignment eras, which makes realignment theory a basic organizing framework for the study of American history.

One of the major reasons for the popularity of the realignment perspective is that it links electoral behavior to institutions and to major shifts in public policy. As developed by various scholars, the basic realignment scenario holds that societal tensions in the form of cross-cutting issues, such as slavery in the 1850s, emerge to divide the country (Beck, 1979; Burnham, 1970; Ginsberg, 1976; Sundquist, 1973). The result of these issues is that the two major parties take opposing positions on the issues of the realignment (Ginsberg, 1972, 1976; Sundquist, 1973) and in the realigning election or elections, the voters put into place "new and durable electoral groupings" (Key, 1955). The newly elected majority party then enacts the "cluster of policy changes" associated with the realignment (Burnham, 1970; Ginsberg, 1976; Sinclair, 1977). Although the extent to which realignments are the mainsprings of policy changes in the American policy process is still in question (Brady, 1978; Converse, 1974; Rusk, 1974), it seems clear that this perspective serves to link elections, institutions, and policy and to provide greater intellectual order to the varied panorama of American history.

Given that there are three units of analysis in the realignment perspective—elections, institutions, and policy results—it is somewhat surprising that most work has been concentrated on studies of realignment in the electorate and little has been done on the effect of realignment on the governmental leaders who shape and push through the clusters of policy changes associated with them. In fact, Clubb, Flanigan, and Zingale (1980, p. 16) attempted to correct this by seeking "to redirect attention away from the role of electoral change and toward the control of government and the role of political leadership in understanding the phenomenon of partisan realign-

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ment." There are some studies of the effects of critical elections on the House of Representatives and the policy process (Brady, 1978, 1980, 1982; Sinclair, 1977), but much remains to be done before the linkages between realigning elections and policy change through institutions will be well understood. Nevertheless, the central concept of realigning elections is important for scholars concerned about mass and elite behavior in democratic countries.

In this article I examine the three major American realignments—the Civil War, the 1890s, and the New Deal. The general thesis is that in each of the realignments the effect of the realigning elections was to create a unified majority party capable of enacting major policy shifts. However, unlike other studies of realignments, the emphasis is on some important electoral differences between these realignments. Specifically, I shall argue that the Civil War and the 1890s realignments were more the result of structural factors than of massive electoral shifts, in contradistinction to the dominant view that the policy changes are the result of changes in mass voting behavior (Burnham, 1970). I begin by briefly reviewing the major factors that deter the ability of the House to legislate major policy changes. In light of these factors, I put forward a theory of policy change in the House and then test the theory over the Civil War, 1890s, and 1930s realignments.

Major policy changes occur rarely in the U.S. House of Representatives for a number of reasons. The most important of these are: 1) members are normally elected by local interests on local issues (Mann, 1978; Mann & Wolfinger, 1980); 2) once elected, members choose, within limits, committee assignments based on those local interests and issues, thus localizing rather than nationalizing policy alternatives (Cooper, 1970; Shepsle, 1978); 3) the Committee system is powerful in part because it is stable (Anderson, 1979; Fenno, 1966, 1973; Goodwin, 1970; Huitt, 1954, 1957); 4) the congressional parties are normally weak and divided; thus they cannot put together coalitions that can override localism (Orfield, 1975); 5) the organization, rules, and procedures of the House facilitate the interests of those who wish to preserve the status quo (Cooper, 1977; Shepsle, 1979). It should also be mentioned that there are many historical periods where inertia or incrementalism are in accord with both the majority of the public and the majority of the House (Jones, 1970). When this is the case, the congressional system is in harmony with political pressures. However, as Burnham (1970) has argued, political systems must over time adjust to majority pressures for change. It is from this perspective that we look to elections to see how the U.S. House of Representatives has at

times overcome these powerful biases toward incrementalism to produce significant policy changes. How do elections generate changes that allow the House to produce major policy shifts?

A Theory of Policy Change

It is obvious that elections do not generate changes in the basic structures of federalism, separation of powers, checks and balances, and single member plurality elections. These structural characteristics have survived many elections. The answer to the question posed above is that certain critical elections reflect partisan realignments that alter radically the standard policymaking process. To understand how these realigning elections affect major public policy changes, it is necessary to begin with an understanding of the generally localized nature of congressional elections. House elections normally are determined by local factors (local interests, organization, and issues), thereby assuring the dominance of localism in politics in the House. In each American realignment, elections are dominated by national issues. Before realignments, cross-cutting issues arise which do not fit within the framework of the existing two-party system. Ultimately, the parties take positions on the issues that offer clear-cut alternatives to voters. (In the case of the Civil War realignment, the Republicans replaced the Whigs before the choice was clear.) When the realigning election or elections occur, the result is a new congressional majority party elected on its positions on national issues, not local issues. Moreover, the new majority party maintains uninterrupted control of the presidency and both branches of Congress for over a decade (Clubb, Flanigan, & Zingale, 1980).

The effect of these elections in the House is that they reduce the major drawbacks to party strength in the House. Elections that are decided on a localized basis make the congressional parties amalgams of differing interests, for example, southern rural and northern urban Democrats during the 1940s through 1970s. Under normal electoral conditions, congressional parties are not united across a whole series of issues. In recent times, Democrats disagreed over civil rights, social welfare, and Vietnam policies, among others, and as long as elections are decided on the basis of local factors, policy differences are accommodated and incrementalism results. During realignments, representatives are elected more on the basis of party positions on the national issues than on local issues. Thus, the majority party is relatively united on the issues of the realignment. Local factors do not radically compromise policy choices, and a unified majority party votes in major policy changes. The argument is not that

local factors are totally overridden, but rather that the balance between local and national factors shifts toward national forces (Sorauf, 1968).

A second drawback to party strength in the Congress is the committee system. Committees have lives of their own, and the more important the committee, the more stable its membership. The stable leadership passes along committee norms and strategies to new members, and, thus policy in the committee's area does not change greatly. The influx of new members during realignments eliminates committee stability, and new leaders support the party position for major policy innovations. In sum, the influx of new members during realignments reduces the impact of localism and committee stability in the House (Brady, 1978, 1980). The result is increased party structuring of voting, especially on the issues of the realignment.

If this notion of realignments is correct, I should be able to show that in each of the three major realignments—Civil War, 1890s, and 1930s—the following occurred.

- 1) The dominant parties took polarized positions on the cross-cutting issues.
- 2) The election results were more national, less local.
- 3) The "new" majority party controlled the government for at least a decade.
- 4) Committee turnover was high.
- 5) Party structured voting on the realignment issues.
- 6) Significant policy changes occurred during these eras; especially relevant are nonincremental changes in appropriations.

The Theory Tested over Three Realignments

In each of the major realignments, a major cross-cutting issue dominated the election. The second American party system was broken up by the rise of the slavery and free labor issues. The two dominant parties of the 1832-1852 period were the Whigs and the Democrats, both of which had northern and southern wings. The compromise of 1850 plus the Democrats' acceptance of federal activity to expand the economy diminished the differences between the parties such that by 1853, the Whigs were no longer a viable electoral force (Holt, 1978, chaps. 5 and 6). Senator Douglas's introduction of the Kansas-Nebraska Act "permanently severed the Northern and Southern wings of the Whig party and eventually obliterated what remained of it in the South" (Holt, 1978, p. 148). In the elections of 1854 and 1856, the Whigs were replaced by Republicans in the Middle West and Know-Nothings in the East. The nativism issue faded in the East, and by 1858

the Republicans were clearly the Democrats' major competition. In short, by the late 1850s the electorate was offered a clear choice between parties and policy on both slavery and free labor.

The cross-cutting issue of the 1890s realignment was the issue of "what future for America—industrial or agricultural?" The rise of industrialization in the aftermath of the Civil War generated the displacement of farmers and a change from a society of local, self-sufficient communities to a highly interrelated industrial society (Schattschneider, 1960). The result was both the displacement of agricultural interests and the end of a way of life. The overarching issues were specifically focused on the questions of gold, silver, the protective tariff, and American expansionism. Because they were debtors, agricultural interests favored the inflationary coinage of silver at a 16-to-1 ratio to gold, free tariffs and anti-expansionism. Industrial interests were opposed to these policies. The rise of the pro-farmer, Populist party is testimony for the existence of cross-cutting issues in the 1896 realignment. Under William Jennings Bryan, the Democrats adopted the Populist position, while the Republicans adopted a pro-position on gold, protective tariffs, and expansionism.

The cross-cutting issue during the New Deal realignment was the result of a single event—the Great Depression—and the question was whether the government would adopt policies to combat the effects of the Depression. The Republican incumbent, Herbert Hoover, would not adopt policies to aid farmers, workers, cities, and the unemployed. By 1932 the Democratic party had answered by proposing relief funds and programs to aid those most affected by the Depression. Once again voters were offered clear-cut choices between parties and policy.

In order to demonstrate the polarization of opinion associated with realignments, in Table 1 I summarize Ginsberg's (1972, 1976) content analysis of party platforms and show the party differences on these issues in each of the realignments.

Table 1 clearly shows that the Civil War and the 1890s realignment were characterized by deepening party differences in regard to the cross-cutting issues leading up to the critical elections. The pattern for the New Deal realignment is somewhat different. The parties differed in 1924, but it was not until 1932 that the Democrats could convince the electorate to send a new majority party to Congress. In addition, the magnitude of these figures suggests that the parties were less polarized in the positions they took during the New Deal than they were during the Civil War and 1890s realignments (also see Brady & Stewart, 1982). Still, the first part of the theory holds: in each of

Table 1. Partisan Platform Differences on Major Issues in Three Realignment Eras^a

	Slavery	Capitalism	Depression
Civil War			
1848	0		
1852	.10		
1856	.24		
1860	.71		
1890s			
1884		.08	
1888		.04	
1892		.44	
1896		.55	
New Deal			
1920			.02
1924			.30
1928			.19
1932			.26

^aAdapted from Ginsberg (1972, p. 612). Higher values represent greater disagreement.

the realignment eras, the major political parties took opposing positions on the issues of major concern. Thus, when voters went to the polls in the elections of 1860, 1896, and 1932, they were offered "a choice, not an echo."

National, Regional and Local Effects on Voting

If realigning elections are precipitated by cross-cutting national issues, then the congressional election results should demonstrate national or regional rather than local electoral forces at work. The standard technique used to determine national rather than local factors is to calculate the variance across House election results over the relevant time period. The argument first put forward by Butler and Stokes (1969) is that when variance around the mean is low, national factors are at work, and, conversely, when variance is high, local factors are predominant. Butler and Stokes used this argument to show how national factors are more important in Britain than they are in the United States. Their point was that variance around the mean swing in pairs of elections measures the uniformity of change, that is, the extent to which national forces are at work. However, testing for change over time within a single country presents some difficulties.

The first and foremost problem is that as Flanigan and Zingale (1974) have shown, realigning change can be either compensating or across the board. In a compensating realignment, both par-

ties gain some votes while losing others. Of course one party gains more than it loses and vice versa for the other party. In an across-the-board realignment, one party gains votes while the other party loses votes. In both cases, as one moves to the realigning election or elections, the variance around the mean is affected because vote totals in districts are shifting, and measures of variance are squared differences; thus, the direction of change is irrelevant. Determining whether a realignment is compensating or across the board is particularly important in the American case because of the regional and cultural variation across the American states (Elazar, 1972).

The strategy for analyzing electoral change in each of the three American realignments is first to show the mean swing in the vote and standard deviation for the following time periods: 1852-1876, 1884-1900, and 1924-1940. The measure of change to be used is the coefficient of variation, which is $V = S/X$, where S is the standard deviation and X is the mean. I want to take into account both the mean swing for or against a party and the variance around that mean. The argument is that as the standard deviation increases relative to the mean, local variation is greater than the national trend, whereas as the mean swing increases relative to the standard deviation, then national trends eclipse local factors. In short, the lower the value of the coefficient of variation, the greater the likelihood of an election reflecting national rather than local factors.

Upon completion of this analysis, I focus on regional variation in each of these realignments, and finally, I turn to the Flanigan and Zingale (1974) analysis-of-variance technique to determine the nature of the realignments.

Testing for national effects during the Civil War realignment era constitutes a problem in that the demise of the Whigs, the rise of the Republicans, the secession, and the later readmission of 11 southern states to the Union during this time period complicates analysis of electoral results. Ideally such a time series should be run on both major parties for all points in the set (Clubb, Flanigan, & Zingale, 1980). Obviously, this is not possible for the 1848-1876 period; thus the analysis for the Civil War period focuses on the Democrats' percentage of the vote in each national election. The Democrats contest elections throughout the era including the period of secession. Silbey's (1977) work demonstrates the party's strength throughout the era. During the Civil War proper, the Democrats almost control the House in the 1861-1862 election, and they only narrowly lost the presidency in 1864. Testing for national versus local factors is possible because the Democrats were both stable and competitive over the entire

time period.¹ The analysis of electoral changes for the other two realignments is straightforward, given that the Democrats and Republicans are stable in both eras. The data in Table 2 show the average swing in the vote and the average of the V coefficients for the pre-realignment, realignment, and post-realignment periods. The realignment elections for each period are: Civil War realignment, 1854-1860; 1890s realignment, 1894-1896; New Deal realignment, 1932-1936.² Table 2 also shows the mean swing and V coefficient for the single election that gave the new majority party control of the presidency, the House, and Senate—1860, 1896, 1932.

During the pre-Civil War realignment (1846 through 1852), the average swing was 3.26 and the average V coefficient was 8.09, which indicates a relatively low swing and considerable variation in voting patterns. In contrast, the realignment period (1854-1860) has a mean swing of -5.31 away from the Democrats and an average V coefficient of -4.34, indicating national electoral factors at work. The post-realignment period (1862-1876) has a mean swing of only 2.87 and a mean V coefficient of 8.15. These data support the hypothesis of increased nationalization of elec-

toral results during the Civil War realignment. The data for the 1860 election are even more dramatic, a swing vote of 6.07 away from the Democrats and a low V of 2.8.

Unlike either of the two other realignments, the Civil War era has major structural changes which affect the electoral results. The replacement of the Whig Party by the Republicans and the secession of the southern states and their readmission are the major structural changes. The elections of 1853-1854 and 1855-1856 are the two decisive elections that replace the Whigs as a major party. The analysis of these elections shows a dramatic increase in the mean swing relative to the variance—scores of -2.8 and 2.9, respectively. The Republicans actually won a plurality of House seats in the 34th House. The Democrats lost 7.41% of the vote, while variance dropped to 20.7. In the 35th House the Democrats gained from the mean swing of 6.34%, while variance dropped to 18.4. The period of the 33rd through 35th Houses realigned party preferences from Whig-Democrat to Republican-Democrat.

The election of 1859-1860, which brings the Republican party full control of the government (president, Senate, and House) also has a coefficient of variance of less than 3.0. In this election, the Democrats lost 6.07% of the vote, and the standard deviation is 17.6. The only other election pair with a coefficient of variance under 3.0 is the 40th-41st (1866-1868) pair. In this pair, the Democrats gained 3.8%, while the variance was only 8.4. The Democratic gain was, in part, a repudiation of radical Republicanism plus the strong Democratic returns from border states. In sum, the elections with low variation coefficients correspond to the structural changes mentioned above. Or as Clubb, Flanigan, and Zingale wrote:

¹The addition of the Republicans beginning in the 36th House does not change the substantive results.

²The following analyses include all election pairs in districts. I have not excluded races where little or no competition exists; thus when a candidate is unopposed in one race and an opponent garners 30% of the vote in the next, a major swing is recorded. Some scholars limit these calculations to competitive districts, thereby causing lower standard deviations. My approach was that in periods of major change, such data should not be thrown out.

Table 2. Aggregate Vote Swings and V Coefficients during the Civil War, 1890s and New Deal Realignments

	Pre-realignment	Realignment	Post-realignment	Controlling Elections
Civil war realignment	1846-1852	1854-1860	1862-1876	1860
X Dem	3.26	-5.31	2.87	-6.07
V	8.09	-4.34	8.15	-2.81
1890s realignment	1884-1892	1894-1896	1898-1902	1896
X Dem	-1.79	-5.45	-3.13	
V	-15.41	-3.05	6.27	
X Rep	2.62	4.39	-2.13	3.67
V	-8.21	3.20	5.46	3.90
New Deal Realignment	1924-1930	1932-1936	1938-1944	1932
X Dem	1.40	9.12	-3.79	7.20
V	8.32	3.04	-9.64	2.14
X Rep	-1.56	-9.60	4.01	
V	-16.60	-4.15	13.47	

"As at the national level, electoral change during these years seems to be largely interpretable in terms of southern secession, occupation and Reconstruction" (1980, p. 110).

The late 1890s realignment shows a clear national trend at work. In the 1894 and 1896 elections, the Democrats lost 5.45%, while the Republicans gained 4.39% and the corresponding V s were below ± 3.20 in each case. This contrasts sharply with the pre-realignment period (1884-1892), where the Democrats lost 1.79% swing and the Republicans lost 2.62% swing with V s of -15.41 and -8.21, respectively. The post-realignment period is characterized by smaller swing votes and V s approximately twice as high as during the realignment. The election that shows the most dramatic effect was the 1894 election. This election gave the Republicans a large majority in the House as voters swung away from the Democrats (-7.6%) and to the Republicans (5.5%). The election of 1896, normally considered the realigning election, was at the congressional level an increase for the Democrats as well as the Republicans (3.3% and 3.6% swing, respectively). The fact that both major parties gained in the 55th House elections (1896) over their totals for the 54th House is clearly the result of the Democratic merger with the Populist Party, just as the loss for both parties in the 53rd House reflected Populist party gains in the 1892 election. Thus, the election pair of 1892 and 1894 most clearly meets the criterion for a shift toward national electoral factors over local factors. The most likely explanation for these data lies in an analysis of vote shifts by region, and I shall turn to that analysis in a moment. However, it should be noted that these results support the assertion of Clubb, Flanigan, and Zingale that "the realignment of 1896 appears substantially less impressive than might have been expected" (1980, p. 111).

The results for the New Deal realignment clearly point to the nationalization of electoral factors in the 1932-1936 (73rd through 75th Houses) period. The coefficient of variation is 3.0 for the Democrats and -4.2 for the Republicans. The Democrats gained 9.1% over their pre-realignment vote totals, whereas the Republicans lost 9.6% from theirs. The largest portion of the Democratic gain came in the shift from 1930 to 1932 (7.2%) and the concomitant Republican loss of 8.0%. The mean swings and the V coefficients in the pre- and post-realignment periods support the nationalization hypothesis. In the pre-realignment both parties' mean swing was less than 2.0% and the V s were at least twice as high as during the realignment. In the post-realignment period the mean swings for both parties were approximately 4%, and the V s were between three and four times higher than during the realignment period. These

results for the New Deal are so clear that the only possible explanation is that the New Deal was an across-the-board realignment to Roosevelt and the Democrats. The realignments of the Civil War and 1890s present a more complicated pattern. Thus I turn to an analysis of all three realignments, controlling for regional effects.

A Regional Analysis of Electoral Results

Tables 3-5 show the mean change and variance in pairs of elections for all three realignment eras, controlling for region. If the shift in the vote (swing) was less than 5%, no entry is made in the tables, in order to enhance their readability.³ A coefficient of variation is given for each election pair where the swing was 5 or greater. The expectation is that the Civil War era and the 1890s realignments will be compensating realignments, whereas the New Deal will be an across-the-board realignment. In short, the pattern for the first two realignments will show greater regional variation than will the pattern for the New Deal era.

The results for the Civil War period (Table 3) show both regional variation and compensation. In the New England states, six of the 14 election pairs have mean swings of 5% or greater. Of these, the largest and most significant were the Democrats' loss of 13.5% in the 1853-1854 election owing to the increased vote for the American (Know-Nothing) party and some support for Republican candidates. The election to the 36th House (1857-1858) resulted in another steep drop for the Democrats (-11.3%) as the Republicans became the dominant party. The coefficients for both these elections were below 1.7, which indicates a strong regional over local trend.

The Union Democrats gained 7.8% in the 1862 elections ($V=2.9$), which reflected displeasure with the Union war effort. The Democrats, however, lost most of this gain in the 1864 election, as Union forces were more successful. The only other election pair with greater than 5% swing was the 1872-1874 election pair, in which the Democrats gained 7.2% ($V=1.4$) and brought a Democratic majority to the House for the first time in 14 years.

The pattern for the mid-Atlantic states shows the election for the 35th House (1855-1856) as a realignment away from the Democrats. The Democrats decline from the election of 1853-1854 in the mid-Atlantic states is impressive when one considers that the Know-Nothings made substantial gains in the 1853-1854 elections. In New York,

³The author will provide upon request complete tables showing the results for each election pair.

Table 3. Regional Vote Swings away from Democrats in the Civil War Realignment^a

Congress	Years	New England		Mid-Atlantic		East West Central		South		Border	
		$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y
31-32	1848-1850	5.5	2.9	13.5	1.1						
32-33	1850-1852							14.3	2.8	6.2	5.8
33-34	1852-1854	-13.5	1.4			-9.7	1.0			11.5	2.3
34-35	1854-1856			-7.6	-2.6			7.9	2.9		
35-36	1856-1858	-11.3	1.6								
36-37	1858-1860										
37-38	1860-1862	7.8	2.9								
39-40	1864-1866									9.7	1.5
40-41	1866-1868									8.5	3.7
41-42	1868-1870							14.7	1.9	6.2	3.1
42-43	1870-1872							12.6	1.4	7.8	2.7
43-44	1872-1874	7.2	1.4								
44-45	1874-1876										

^aOnly average changes of more than 5% are included.Table 4. Regional Vote Swings to the Republicans in the 1890s Realignment^a

Congress	Years	New England		Northeast		East North Central		West North Central		South		Border	
		$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y
49-50	1884-1886							10.4	2.6	-13.7	1.4		
50-51	1886-1888									11.4	1.6	5.9	2.4
51-52	1888-1890							-8.6	0.7	-9.8	1.9	-6.8	1.9
52-53	1890-1892												
53-54	1892-1894	8.5	0.3	9.1	1.0	6.7	0.8	5.0	0.7			6.1	2.4
54-55	1894-1896	7.1	0.9							10.3	2.1		
55-56	1896-1898	-8.0	0.6	-7.7	2.0					-8.7	-2.2		
56-57	1898-1900												

^aOnly average changes of more than 5% are included.

Table 5. Regional Vote Swings to the Democrats in the New Deal Realignment^a

Congress	Years	New England		Northeast		East North Central		West North Central		South		Border		West	
		$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V
69-70	1924-1926														
70-71	1926-1928														
71-72	1928-1930														
72-73	1930-1932			8.6	2.0	12.7	1.3	10.4	1.5			-10.8	1.6	23.6	1.0
73-74	1932-1934											11.6	1.5		
74-75	1934-1936													6.1	3.3
75-76	1936-1938			-7.9	-0.7	-6.0	1.0							-5.6	2.2
76-77	1938-1940														

^aOnly average changes of more than 5% are included.

New Jersey, and Pennsylvania the Know-Nothings made strong showings in congressional elections on the nativism and anti-Catholic issues. However, by late 1856 the Republicans had become the major anti-Democratic party as the nativism issue faded (Holt, 1978, pp. 160-164).

The election to the 32nd House (1849-1850) resulted in a solid Democratic gain (13.5%) as the Whigs fell apart over the slavery issue and the compromise of 1850. Although no other elections meet the 5% swing criteria, the election to the 39th House (1863-1864) reflected the positive achievements of the Union armed forces, just as the elections of 1861-1862 (38th House) reflected the poor northern showing in the Civil War (Democratic loss of 4.0% and a gain of 3.7%, respectively).

The midwestern states show the election to the 34th House (1853-1854) as realigning. In the elections to the 34th House, the Democrats lost 9.7% from their 33rd House vote totals, and in the elections to the 35th House, the Democrats lost another 4.7%. The pattern reflects first the Republicans replacing the Whigs as the second party in 1853-1854, and then, in the 35th House elections, the Republicans became the dominant party. The Democrats gained votes in the 1861-1862 elections, again as a result of the North's poor showing in the war. When in 1863 and 1864 the North began winning the war, the Republican vote in congressional elections picked up considerably.

The southern states moved solidly into the Democratic column in the elections to the 33rd House (1851-1852) with a gain of 14.3% over their 1849-1850 vote totals. This gain and Democratic dominance in the region were increased in the elections of 1855-1856 (35th House), when the Democrats gained another 7.9% over their 1853-1854 totals. The two lowest coefficients of variation in the pre-Civil War period, 2.8 and 2.9, occur in these two elections. The border states followed the deep southern states into the Democratic camp in the elections to the 34th and 35th Houses. In these two elections, the Democrats gained more than 38% from their 33rd (1851-1852) House totals. The decisive presidential elections of 1860 resulted in another huge gain for the Democrats in the border states. In each of these elections, the V coefficients are below 2.5.

In the post-Civil War period, the readmission of the southern states resulted in an increased Democratic party vote in the period from 1871 to 1874 (43rd and 44th Houses). In these election pairs, 42nd-43rd and 43rd-44th, the Democrats did well. From the 42nd to the 43rd, the Democrats gained in the mid-Atlantic and the southern states—mean change of 8.6% and 10.2%, respectively, and in the 44th House elections, the Demo-

crats gained decisively in the New England (9.8%) and border (7.8%) states.

These results point to a regional and structural explanation of the Civil War realignment. In a varying pattern, regions first abandoned the Whigs for the Republicans, and then the Republicans came, over time, to dominate the northern state elections, while the South turned strongly to the Democrats. The end of the Civil War and the readmission of the southern states brought about solid Democratic gains throughout the country, such that by 1874 (44th House) the Democrats were the majority party in the U.S. House of Representatives. This pattern varied across regions and states as well as across time. Given these regional results, it is obvious why the results for the whole nation do not fit a classic across-the-board realignment pattern.

Regional Results in the 1890s Realignment

The national results showed the 1892-1896 (53rd-55th Houses) election pairs as realigning. The regional results show that the pattern varies by region (Table 4). In the New England states, the elections of 1892-1894, 1894-1896, and 1896-1898 all have low coefficients of variation. The 1894 election has Democrats losing almost 10% from their 1892 vote total, whereas the Republicans gained 8.5% ($V = .3$). In the election to the 55th House, the Democrats lost another 5.5% and the Republicans gained 7.1% ($V = .9$). However, in the election to the 56th House (1898), the Democrats gained back more than 8% of the vote while the Republicans lost 8% from their 1896 totals. The northeastern states follow essentially the same pattern, although swings of more than 5% are not as frequent. In the elections to the 54th House (1894), the Democrats lost 7.6%, while the Republicans gained 9.1%, with V equal to 1.0. Unlike the New England states, northeastern congressional elections did not continue a major swing away from the Democrats in the 1896 elections—a loss of less than 3% for Democratic candidates. In the election to the 56th House, the Democrats gained 7.4% while the Republicans lost 7.7%, with V s of 2.0 or under. Thus, for the eastern states, the critical elections at the congressional level occur in the 1894 to 1898 period, with some differences between the northeastern and New England states. Most of this difference can be accounted for by the resurgence of the Democratic machine in New York City in the 1896 election; that is, while Democrats continued to lose votes in New England in the 1896 elections, the New York City Democrats recaptured votes lost to Republicans in 1894 (Brady, 1973).

Congressional elections in the East North Central states show a separate pattern. In the 1894

elections, the Democrats lost over 10% from their 1892 totals, while the Republicans gained 6.7%. This, of course, follows the national trend. The elections of 1896 show a different pattern in that the Democrats gained votes while the Republicans gained only .3%. The Democratic gain was the result of their adoption of Populist principles and of combining candidates; thus, there was no Republican loss in the vote.

The Populist effect on congressional elections is most evident in the largely agricultural West North Central states. For the Republicans there are three elections where the swing is more than 5% and the V s are under 3.0. Clearly the mean vote swings are high relative to the variance around the swing in these elections. The Republicans gained 10.4% in the 1886 election, lost 8.6% in the 1890 election, and gained 5% in the 1894 election. Most of the vote pattern for this region can be explained in terms of the rise and fall of Populist strength relative to Democratic strength. In terms of a national pattern, the Republican gains in the 1894 House election contributed to the Republicans victory in 1894, but there are no other elections with a mean swing of more than 5%.

The pattern for the southern states parallels that found in the agricultural Midwest. In five of eight cases, the coefficient of variation is below 2.5; in three of these, it is below 2.0. Again, the explanation lies in the rise and fall of Populist strength relative to Democratic strength. Five of the election pairs show swings of more than 8% to or from the Democrats and Republicans. The different pattern observed here for the Republicans, that is, gaining and losing relatively high percentages of the vote, is a result of the fact that the Republicans combined with Populists in a number of southern states to oppose Democrats. After the 1896 election, Democrats met in Jackson, Mississippi, to determine how coalitions of blacks and poor whites could be prohibited from challenging the regular Democratic hegemony in the region. The result was the Jim Crow legislation which caused an immediate sharp drop in voter turnout throughout the deep South and firmly established Democratic control. Evidence for this can be seen in the increase of more than 13% for the Democrats in 1898 and corresponding loss of 8.7% for the Republicans.

The pattern for border-state elections shows relatively small swings in the mean vote except for the 1888-1890 and 1894-1896 election pairs. In the 1888 election, Republicans gained 5.9% ($V=2.9$); however, these gains were lost in the 1890 elections as the Democrats swept to victory. The major change occurred in the elections for the 54th House, when the Republicans gained more than 6% and the Democrats lost more than 9%.

Thus, the border states' electoral voting pattern is more in line with national pattern found in Table 2 than was the case for the southern and West North Central states.

The regional electoral results yield a complicated conclusion at best. The realignment of the 1890s was compensatory in that the north became more Republican while the southern and West North-Central states became more Democratic. However, this pattern varies by region and by election. The New England states swung more solidly Republican than did the northeastern and East North-Central states. The rise and fall of Populist party candidates affected results in the rural East North-Central, West North-Central, and southern states. The brunt of lasting electoral change occurs from 1892 to 1898 with the 54th and 55th House elections benefiting Republicans, and the elections to the 56th House reversing a good portion of the Republican gains. In short, there is no question of a clear across-the-board realignment to the Republicans in the 1890s.

Regional Results in the New Deal Realignment

Unlike the Civil War and 1890s realignments, the New Deal realignment fits the classic pattern. As Table 2 showed, the election pair 1930-1932 was clearly the period where the Democrats became the dominant party. The same analysis run over regions (Table 5) shows some regional variation, but much less than was the case in either of the two previous realignments. The New England states remained Republican even during the New Deal era. The Democrats gained slightly in each election from 1926 through 1932; however, they never gained as much as 3%, and the base from which they started (1924) was very low. Moreover, only one coefficient of variation approaches 2, that for the Republicans in the 1936-1938 election pair. Thus there is little in the data to show a major switch to the Democrats.

The data for the northeastern and East and West North-Central states show a clear switch to the Democrats in the 1932 election. The Democrats gained from 8.6 to 12.7% over their 1930 totals, whereas the Republicans lost between 8.1 and 11.8%. The coefficients of variation in each of these regions for both parties over the 1930-1932 elections are ± 2 or less. No other pre-1932 election pair has a coefficient of variation close to 2. In each of these regions, the Democrats gained votes in each pair of elections from 1924-1926 through 1932. In the Northeast, the Democrats also gained votes in the 1934 and 1936 elections. The result was to transform the northern states from a bastion of Republican strength into a Democratic stronghold from 1932 until 1938.

The congressional elections of 1938 were a modified across-the-board readjustment in the North back toward the Republicans. The coefficients of variation for these states are below ± 2 , and the Republicans gained more than 5% in each region. However, this readjustment did not return the Republicans to their former majority status. Rather, after 1938 the northern states were two-party competitive—a distinct improvement in Democratic fortunes. During the pre-1932 period, the average Democratic vote in these Northern states was 40.3%, and the Republicans averaged 56.8%. The New Deal elections of 1932 through 1938 gave the Democrats an average of 53%, while the Republicans fell to approximately 44%. In the post-1938 electoral period, the parties became very competitive, with the Democrats holding a 48.9 to 47.8% edge through the 1944 elections. In sum, the northern states moved from solidly Republican to two-party competitive as a result of the New Deal realignment.

The southern states began the pre-New Deal period heavily Democratic, and over the New Deal era, there was a slight increase in the Democrats' share of the vote. The only elections with coefficients of variation below 3 are the 1924-1926, 1926-1928, and 1928-1930 pairs. In the 1930 elections, the Democrats gained back the approximately 3.5% they had lost to the Republicans in the disastrous 1928 presidential election. However, owing to Al Smith's candidacy in 1928, Democratic congressional candidates lost almost 4% of their 1926 vote. In 1930, when the party ticket was not led by the antiprohibitionist and Catholic Smith, the Democrats gained 4.4% while the Republicans lost 4.5%. In the 1932 and 1934 elections, Democrats gained votes, but their total percentage was so high that there were no significant coefficients of variation. Thus, the New Deal realignment resulted in a heavily Democratic region becoming slightly more Democratic.

The pattern for the border states shows only two significant pairs of elections, 1926-1928 and 1928-1930, and these results are explainable by the Smith candidacy in 1928: The Democrats lost more than 10% of their vote from 1926 as a result of a backlash against Al Smith. With Smith gone in 1930, the Democrats gained back all they had lost plus approximately one percentage point. In the critical election of 1932, the Democrats gained 3.4%, and the Republicans lost more than 5%. Thus, as in the South, the border states began the period solidly Democratic and ended the period slightly more Democratic.

The western states are more difficult to analyze because the number of representatives in the region is limited and the vote fluctuates wildly, but the 1930-1932 election pair stands out as significant; the Democrats gained more than 23%,

while the Republicans lost over 25%! The Republicans gain back some of this loss in the 1938 elections, but the Democrats end the era in much better position than they began it.

The regional analysis of New Deal elections demonstrates some differences between the extent to which various areas shifted to the Democrats and to a lesser extent the times at which they shifted. However, the major finding was that across all regions there was a shift to Democratic congressional candidates in the 1932 general election, and that in four of the six regions the shift was greater than 8% and the corresponding V s were less than 2.1. The shift was greatest in the non-New England, northern states, and less in the predominantly Democratic southern and border states. Clearly, in comparison to the Civil War and 1890s realignment, the New Deal realignment meets the test of an across-the-board realignment.

Analysis of Variance of Electoral Change

The most thorough test for analyzing realigning elections is the Flanigan and Zingale (1974) technique which sorts out across-the-board and compensating *realignment* change from non-realigning temporary surges and interactive change. Their technique is essentially a two-way analysis of variance (election by subunit) with interaction and one observation per cell (see also Clubb, Flanigan, & Zingale, 1980, pp. 116-117). I duplicated their analysis with the congressional district as the unit rather than the county or the state. This technique allows one to distinguish between temporary and permanent or realigning, across-the-board change and to distinguish between temporary and permanent compensating change. Because I am only interested in realigning change (either across-the-board or compensating), Table 6 shows only results that can be interpreted as realigning. The figures for across-the-board change can be interpreted straightforwardly as percentage-points change. For example, the across-the-board change of 9.6 for the Democrats in 1932 means that the subunits moved an average of 9.6 units toward the Democrats, and the change was not temporary. The interactive-change figures mean that congressional districts moved in different directions from their own expected vote and from the nation as a whole. Because the figures for compensating change represent increases in some districts and decreases in others, no sign is attached to the figure. Following Flanigan and Zingale, I use moving cross sections of four elections.

Table 6 shows that the Civil War era and 1890s realignments are not as clearly realignments as was the New Deal. During the Civil War period, the interpretation is structural. The 3.6 shift away

Table 6. A Flanigan-Zingale Analysis of Variance for Three Realignment

	Democrats		Republicans	
	Across the Board	Compensating	Across the Board	Compensating
Civil War				
1850				
1852				
1854	-3.6			
1856	1.9	2.7		
1858				
1860	-2.8	1.8		
1862				
1864				
1866				
1890s				
1884				
1886				
1888				
1890				
1892				
1894	-3.9		1.9	2.4
1896	1.0			
1898		1.5		
1900				
New Deal				
1924				
1926				
1928				
1930				
1932	9.6	2.1	-4.2	2.3
1934				
1936				
1938	-3.4			
1940				

from the Democrats in the 1853-1854 elections was a result of the Whigs' being replaced in many northern congressional districts. The 1856 elections showed a slight shift (1.9) back to the Democrats and a compensating change of 2.7. The election of 1860, which brought to Republicans control of the government, resulted in a 2.8 loss to the Democrats and a 1.8% compensating shift.

The 1890s realignment showed the Democrats losing 3.9%, while Republicans gained 1.9% across the board in the 1894 elections. In addition, there was a compensating change of 2.4% on the Republican side of the ledger in 1894. The 1896 and 1898 elections showed first a slight Democratic across-the-board shift (1%) and a 1.5% compensating shift in the 1898 elections. In both the Civil War era and the 1890s realignments, there are indications of lasting electoral change;

however, with Clubb, Flanigan, and Zingale (1980), one can conclude: "Electoral change during these years [Civil War era] seems to be interpretable in terms of Southern secession, occupation and Reconstruction. . . . The realignment of 1896 appears substantially less impressive than might have been expected" (pp. 110, 111).

In contrast, the results for the New Deal realignment indicate a clear-cut across-the-board shift toward the Democratic party in the 1932 election. The across-the-board 9.6% Democratic gain and the 6.2% Republican loss clearly outweigh the 2.1 and 2.3% compensating losses. The Republican resurgence in the 1938 elections is documented by the across-the-board, 3.4% loss suffered by the Democrats in the 1938 elections.

The major point of this extended analysis of the three major American realignments is that at least two of the three realignments were not the result

of massive shifts of voters toward the new majority party. I do not deny that each of these were realignments; rather the point is to show that what is important about realignments is how the electoral results affect the composition of the U.S. House of Representatives. The most important effect is that in each realignment the result was undisputed control of the House of Representatives, the Senate, and the presidency for at least 14 years. The massive shift of voters to the Democratic Party in the 1932 elections is easily translated into Democratic control of the House. The Democrats added to their solid southern and border-state base a bloc of urban, northern ethnic districts (Brady & Stewart, 1982). It is more difficult to understand how the less dramatic electoral shifts in the Civil War era and the 1890s resulted in Republican control for an equal period. The answer to this question entails analysis of the votes-to-seats ratio and the use of the so-called cube law (Tuftes, 1973).

The Votes-to-Seats Ratio

In the American electoral system, representatives are elected through a single-member, plurality winner arrangement. This "first past the post" rule often distorts the ratio of popular votes to legislative seats. In attempting to assess this distortion, scholars in the United States and Great Britain (which have the same electoral arrangement) have tried to apply the cube law, which states that the votes-to-seats ratio will take the following form:

$$\frac{\text{seats Party A}}{\text{seats Party B}} = \frac{\text{Votes Party A}^3}{\text{Votes Party B}^3}.$$

Although the cube law has not attained the status of a precise empirical law, some, for example, argue it should be a $2\frac{1}{2}$ rule. It does contain much validity in that for some periods, it expresses the votes-to-seats ratio accurately. The argument in this section is that during the major electoral realignments in the United States, there is a high votes-to-seats distortion. I shall use the cube law, which represents a significant distortion, as a base line. The hypothesis is that during realignments, the votes-to-seats ratio will be greater than that predicted by the cube law. This is particularly important for the Civil War and 1890s realignments, because as I have shown, there is not major shifting of votes to Republicans during either of these realignments. In short, the long periods of Republican control brought about during the Civil War and 1890s realignments were more the results of a vote-to-seat distortion than of a massive voter shift, as was the case in the New Deal realignment.

The regionalism of both the Civil War and 1890s realignments was the result of an overall shift of approximately 5% of the voters to the Republicans in the northern states. This shift causes the competitive seats to become Republican and translates this vote shift into sizable seat gains for the Republicans. In the New Deal realignment, the vote shift in the north to the Democrats should result in a significant vote-seat distortion which should be masked when the solidly Democratic South is calculated into the equation. In the Civil War realignment, the period immediately before the realignment (the Second Party system) was characterized by a nationally competitive two-party system, and from 1860 to 1868-1870, only the northern states are in the Union; thus, during this period, calculations are not divided regionally. In the 1890s and New Deal realignment, the calculations are done both with and without the South. Table 7 shows the results for all three realignments.

The results for the Civil War realignment corroborate the hypothesis. In the elections to the 34th House where the Republicans become the plurality in the House, they enjoyed a +.50 differential above what the cube law predicted. The 1855-1856 elections were accurately predicted (-.05), whereas in the 1857-1858 elections, the Republicans were again benefited (.53). The major distortion in the series occurs in the presidential election year of 1860. The cube law predicts a vote-to-seat ratio of 1.07 for the Republicans, and the actual seat ratio was 2.52, which favors the Republicans by an impressive 1.45 over the cube law prediction. Thus, the Republican victory in 1860 grossly distorted the seats in the House toward the new majority party. After the southern secession, the cube law becomes a more accurate predictor, with no post-secession House having an advantage as great as .50. The swing in votes to Republicans in the North during the 1859-1860 elections clearly results in an overwhelming number of competitive seats going Republican. In the elections that follow, the Republicans retain their majority, but by narrower margins. In fact, during the election to the 38th House (1861-1862), the Union Democrats almost regain control of the House of Representatives.

The national elections of 1886 and 1888 (50th and 51st Houses) favored the Republicans by a .46 and .36 margin over the cube law prediction. The largest difference between the cube law prediction and actual results appears in the 1894 elections (54th House), when the Republicans have a full 1.0 difference between the predicted seat ratio and the actual results. The elections to the 55th, 56th, and 57th Houses (1896 to 1900) also gave the Republicans a sizable advantage (.46, .46, and

Table 7. The Votes-to-Seats Ratio in the House of Representatives during Three Realignment Eras^a

Realignment Year	Cube Law Prediction		Actual Results		Difference	
	With South	Without South	With South	Without South	With South	Without South
Civil War (R to D)						
1854	.80		1.30		+ .50	
1856	.77		.72		- .05	
1858	.59		1.12		+ .53	
1860	1.07		2.52		+1.45	
1862		1.18		1.29		+ .11
1864		3.21		3.15		- .06
1866		2.09		2.92		+ .37
1890s (R to D)						
1884	.54	.89	.76	1.20	+ .22	+ .31
1886	.42	1.05	.88	1.38	+ .46	+ .33
1888	.66	1.22	1.02	1.73	+ .36	+ .51
1890	.35	.76	.36	.51	+ .01	- .25
1892	.42	.89	.55	.83	+ .13	- .06
1894	1.05	2.66	2.05	5.70	+1.00	+3.04
1896	1.05	2.20	1.51	2.93	+ .46	+1.73
1898	.58	1.52	1.04	1.83	+ .46	+ .31
1900	.76	1.95	1.19	2.36	+ .43	+ .41
New Deal (D to R)						
1924	1.18	.33	.76	.38	- .42	+ .05
1926	1.27	.34	.81	.42	- .46	+ .08
1928	1.08	.31	.61	.28	- .47	- .03
1930	1.68	.48	.99	.56	- .69	+ .08
1932	4.78	1.58	2.38	1.63	-2.40	+ .05
1934	5.41	1.84	2.88	2.07	-2.53	+ .23
1936	5.60	2.15	3.38	2.47	-2.23	+ .32
1938	3.52	1.08	1.49	.96	-2.03	- .12
1940	3.35	1.08	1.63	1.05	-1.72	- .03

^aTheory dictates use of all elections before 1862.

.43). Excluding the southern states from the analysis clearly shows that the pro-Republican seat distortion comes from the northern states. In the 1894 elections in northern states, the cube law predicts a Republican seat advantage of 2.66 to 1, whereas the actual seat ratio is 5.7 to 1, a difference of 3.04 favoring the Republicans. The election of 1896 in the northern states also heavily favored the Republicans, a 1.73 advantage over the cube prediction. Thus, in the two critical elections of 1894 and 1896, the Republican victories in the House can be attributed to the vote-to-seat advantage they enjoyed in the northern states. It is worthwhile to point out that the cube law predictions are in fact sizable distortions themselves; thus, when a party has advantages over the prediction, the electoral system is creating a majority party. In sum, the realignment to the Republicans in 1894-1896 can best be understood not in terms of massive vote shifts, but rather in terms of relatively minor across-the-board shifts to the

Republicans in the northern states which translated into major seat shifts.

The results for the New Deal realignment show the Republicans with a clear national advantage throughout the period. In every election from 1924 to 1940, the Democrats received fewer seats than the cube law predicted. From 1924 to 1930, when the Democrats were generally not competitive in the North, the Democrats were from -.42 to -.69 from their predicted number of seats. Beginning with the 1932 realignment, when the Democrats clearly became the majority party, they were considerably below the cube law prediction. In fact, from 1932 until 1940, the Democrats were from 2.53 to 1.72 below the cube prediction. This is understandable in that the Democrats control the South so heavily that the addition of northern voters leaves them below what the cube law predicts.

Excluding the South demonstrates these points clearly. In the northern states, the cube law is ac-

tually a good predictor of the vote-to-seat ratio. In the 1932 elections, the Democrats are .05 over the cube law prediction; however, in both the 1934 and 1936 elections, the Democrats receive a .25 and .32 average over the cube law prediction. In short, the across-the-board 1932 realignment to the Democrats was so massive in proportion that the Democrats dominated without a votes-to-seat distortion equal to the cube prediction. Since the Democrats gained in every region, the cube law actually overpredicts the actual vote-to-seat ratio.

These results point to differences between the nineteenth-century realignments and the New Deal realignment. This difference has been noted by other scholars (Burnham, 1970, 1974; Converse, 1974; Rusk, 1970, 1974). Burnham's (1970) thesis attempts to account for these differences by claiming that the nineteenth century was a different political universe in which voter interest and participation were high. Moreover, the party systems were characterized by strong, highly divided partisan blocs with relatively little give in the electorate and no large bloc of uncommitted voters. The break-up of the nineteenth-century system resulted in a steady decline in voter participation, partisanship, and ultimately policy consensus in government (Burnham, 1965, 1970). Thus, the New Deal realignment was just a temporary break in the trends of declining partisanship and participation. The argument offered in this article is simpler and does not carry with it any ultimately untestable baggage such as the differing effects of education on politics in the nineteenth and twentieth centuries. Rather, the argument offered herein has been that in a competitive party system, relatively minor shifts of votes in the 1850s and 1890s resulted in clear majorities for the new majority party. This argument has the advantage of cross-time and cross-national comparability; that is, it is an argument about certain measurable kinds of party systems yielding certain results. Thus, the difference between nineteenth- and twentieth-century realignments are accounted for by the following two factors. First, the nineteenth-century realignments were compensating in that the Republicans gained in the North and lost in the southern and border states. Compensating realignments would be less likely to show as dramatic electoral effects as would across-the-board realignments. Second, in both the Civil War and 1890s realignments, the average margin of victory before the realignment was relatively small; thus minor swings in the vote could result in major seat swings in the seat ratios. Although one cannot conclude from this much about the changing effect of education on participation or on the role of politics as entertainment, one can draw generalizations about the effects of small swings of the vote in a competitive party system.

Namely, competitive party systems can have significant seat swings given only minor vote switches if the vote swing cuts across a large number of districts.

A partial proof for this generalization can be derived by looking at the ratio of vote shifts to seat shifts in British general elections; that is, the British party system before 1970 was similar to the nineteenth-century American system in that there were strong partisan blocs, little relative give in the electorate, and no large bloc of uncommitted voters, plus both systems have a single-member, plurality-winner electoral arrangement. Thus we would expect that under these conditions small shifts in votes would often result in major seat switches. In 8 of 18 elections from 1900 to 1970, vote swings of less than 7% resulted in seat swings of more than 20%. Moreover, the elections of 1910, 1924, 1929, and 1970 had vote swings of less than 5% with seat swings ranging from 15 to 32%. (See Brady & Stephens, 1983, for a more complete analysis.) In short, the party system factor is related to the electoral results. The Civil War and 1890s realignments occurred during a period in which there existed relatively strong party systems; thus small shifts to the Republicans in the North resulted in seat changes in the House which kept Republicans in control for over a decade. The New Deal realignment was a major electoral shift to the Democrats in the North which, when added to their solid southern and border-state base, gave Democrats control of the House for over a decade. The important point is that however the House majority was created, by major vote switches or a vote-seat distortion, the result was that rare condition in American politics, unified control of government for over a decade.

Party Structure of Voting across Realignments

The electoral effects in all three realignments resulted in unified majority parties capable of enacting the clusters of policy changes associated with these realignments (Brady, 1978, 1982; Clausen, 1975; Ginsberg, 1972, 1976; Sinclair, 1977, 1978). One of the major reasons for the high levels of party voting associated with the clusters of policy changes is that the "new" congressional majority was elected on relatively "national" issues. Even in the compensating elections of the Civil War and 1890s era, the Republican congressional party was elected to majority by standard swings in the northern states. Under such conditions, constituent-party cross pressuring is reduced because the election was decided on the national issues of the realignment (Brady, 1978, 1982; Huitt, 1961). This argument has been elab-

orated in detail elsewhere; thus I simply present here three tables showing the product moment correlation between party identification and the issue dimensions associated with each of the three realignments. Details regarding the construction and measurement of the issue dimensions can be found in Brady (1982). Tables 8 through 10 show the results.

The findings show that during each realignment, party structures voting patterns, especially in regard to the dominant issues of the realignment. This result was expected, given the national nature of the realigning elections; however, an important aspect of how realigning elections are converted into policy changes has been neglected. Namely, how were committees affected by three elections such that they legislate clusters of policy changes?

Committees and Policy Changes^a

It has been shown that during realigning eras, turnover on congressional committees was extremely high (Brady, 1978). When turnover on committees is high, committee stability and its ability to pass along the norms that affect decisions is sharply altered. Fenno (1966), for example, has shown that high turnover on the Appropriations Committee affected committee behavior. Specifically, he argued that when the Republicans became the majority in the House

^aI am indebted to Joseph Cooper for help with this section. He and I have written a forthcoming article that covers the appropriations process more thoroughly, including an Arima analysis.

Table 8. Product Moment Correlations between Party Identification and Issue Voting over the 33rd (1853) to the 42nd (1871) Congresses

	33rd	34th	35th ^a	36th	37th	38th	39th	40th	41st	42nd
Slavery secession and civil rights	.51	.41	.89	.87	.88	.92	.91	.96	.98	.97
Public works	.65	.59	.89	.78	—	.67	.78	.64	.91	.91
Railroad and telegraph construction	.14	.22	.89	.67	.32	.58	.52	.15	.38	.47
Housekeeping	.27	.47	.96	.90	.64	.92	.79	.94	.97	.94
Money and banking	—	—	—	.86	.79	.94	.41	.78	.83	.94
Tariff	.55	.02	.71	.85	.74	.92	.71	.58	.79	.59

^aA rise in correlation of party identification with issue dimensions is expected at this point in time.

Table 9. Product Moment Correlations between Party Identification and Issue Voting from the 52nd (1890) to 56th (1900) Congresses

Issue Dimensions	Congress				
	52nd	53rd	54th	55th	56th
Currency	.02	.42	.71	.96	.96
Military Appropriations	.69	.19	.16	.89	.93
Tariff	.97	.89	.20	.98	—
Business	.92	.78	.41	.92	.98
Public Works	.31	.70	.26	.31	.31
Housekeeping	.73	.93	.94	.88	.99
Immigration	.26	—	.47	.54	.81
Taxation	—	.97	—	.92	.88

Table 10. Product Moment Correlations between Party Identification and Issue Voting from the 69th (1924) to 76th (1940) Congresses

Issue Dimensions	Congress							
	69	70	71	72	73	74	75	76
Government management	.84	.88	.91	.94	.97	.91	.94	.89
Social welfare	—	—	—	.72	.89	.94	.86	.69
Agriculture assistance	.08	.24	.94	.77	.90	.91	.77	.89
Civil rights	.17	—	.20	.46	—	.22	.18	.22

after the 1946 elections, they attempted to cut back many of the New Deal programs. In the process, consensual norms that had governed committee behavior were threatened. Thus, there is some empirical evidence to justify using turnover as a variable that affects policy outputs.

A second variable that affects committee behavior is partisanship. The Appropriations Committee integrates partisan and ideological differences by attempting to build a consensus (Fenno, 1973). The committee is successful—wins the floor—when the appropriations bill is viewed as a committee bill rather than a Democratic or Republican bill (Fenno, 1973). In contrast, the Education and Labor Committee, among others, has a very partisan decision style (Fenno, 1973; Murphy, 1974). The result is that the Education and Labor Committee reports partisan and ideological bills which are not as easily passed on the floor of the House. In this section I examine the effects of realigning elections on the House Appropriations Committee.

The Appropriations Committee is at the heart of the congressional process and is thus a natural choice. In the post-World War II era, the Appropriations Committee has operated in a consensual mode wherein differences are compromised within the committee. This process of consensus building results in incremental public policy changes (Fenno, 1966, 1973). Moreover, after the formation of the Appropriations Committee in 1865, the major changes in the House's appropriations process resulted from disputes over the committee's funding of programs and agencies. When the House in the 1880s gave seven committees besides Appropriations the right to appropriate funds, it was because the Appropriations Committee was not funding agencies such as agriculture at a high enough level. When the House voted in 1919 to return all appropriations powers to the Appropriations Committee, it was because the seven clientelist committees were funding their agencies and programs at too high a level. Thus there exists historical evidence that the committee's strategy to report consensual bills that balance the need to guard the Treasury and to fund programs incrementally is a long-standing strategy.

During realigning eras, old policy patterns are broken and clusters of policy changes are enacted. If this is the case for policy, in general such results should also be observable at the level of House appropriations. The argument is that if the decision process on the committee is partisan rather than consensual, the chances of nonincremental appropriations is increased. In addition, I argue that during realignments, committee turnover is high and the new members of the Appropriations Committee will behave in a partisan fashion, and

the result will be appropriations which are non-incremental. In order to demonstrate this, it is necessary to show that:

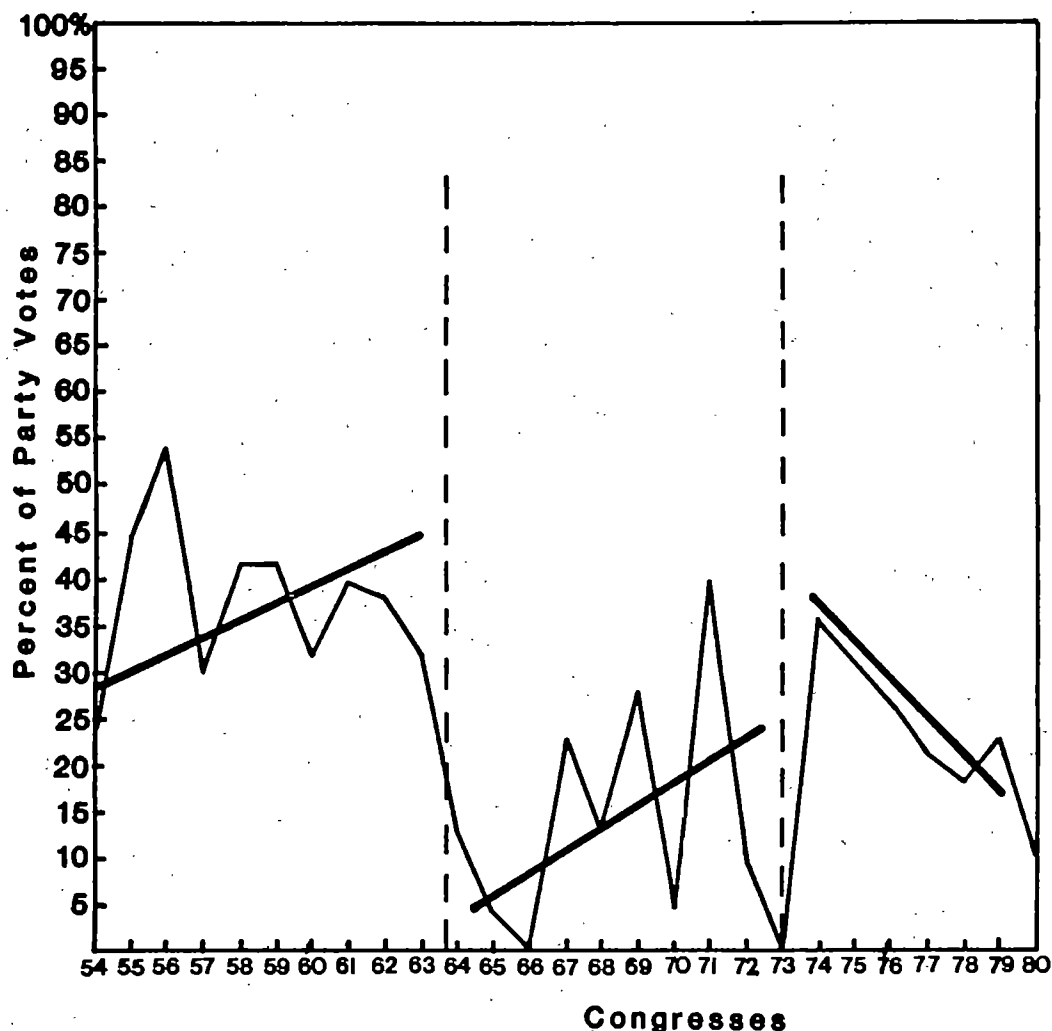
- 1) During the period from 1890 to 1950, the Appropriations Committee's behavior was at times partisan as well as consensual.
- 2) The absolute change in appropriations from year to year is associated with levels of partisan voting. Specifically, partisan committee behavior will be related to nonincremental appropriations.
- 3) During the realignment periods 1895-1899 and 1933-1939, the levels of partisanship and non-incrementalism will be highest.
- 4) Committee turnover will be high during the realignment periods and thus associated with both partisanship and nonincrementalism.

In sum the argument is that during a realignment period, electoral results cause high turnover, and the new Appropriations Committee behaves in a partisan fashion which results in nonincremental appropriations.

The 1890-1910 era in congressional history was the heyday of the Speaker's power. The committee and party systems were molded together by the practice that party leaders chaired the important committees. The result was a more hierarchical, centralized House of Representatives (Brady, 1973; Brady & Althoff, 1974; Cooper & Brady, 1980; Jones, Cannon, & Smith, 1968; Polsby, 1960; Polsby, Gallaher, & Rundquist, 1969). During this era, party voting in the House of Representatives peaked; thus one expects to find that during this period the House Appropriations Committee voted in a more partisan fashion. In order to test this hypothesis, Appropriations Committee members' votes on the following kinds of roll calls were analyzed: amendments to appropriations bills, recommittal motions on appropriations bills, and the single vote on final passage. An average Index of Party Likeness score was computed for the entire committee, and the percentage of all such votes where 90% of one party opposed 90% of the other party was calculated. These scores were calculated for all Houses from the 54th through the 81st House (1894-1950). Figure 1 shows the percentage of 90 versus 90 votes on the Committee over time.

Figure 1 clearly shows that over the entire time series, there have been periods of partisan committee-member behavior. The period from the 1894 realignment to the stripping of the Speaker's power in the 61st House is a period of consistently partisan behavior. The lowest figure occurs in the 54th House (22%), whereas in the 55th and 56th Houses, the average is 50% of all votes being heavily partisan. During the second period, the

Figure 1. Percentage of Party Votes (90 vs. 90%) on House Appropriations Committee, 1894-1950



average is much lower, and only two Houses attain levels corresponding to the 1894-1911 levels. The New Deal realignment results in an increased percentage of party votes during the 1934 to 1938 period (74th through 76th Houses), when Roosevelt made his turn to the left (Leuchtenberg, 1963). After these Houses, the figures fall back toward the levels Fenno (1966) reports. In addition, I calculated an average Index of Party Likeness for each House, which was 27.2 for the 1894-1915 period and 51.6 for the post-1916 period. Thus, by both measures it is clear that the Appropriations Committee members have behaved at various points in a partisan as well as a consensual manner.

The partisan behavior of the committee occurred during two periods: the 1894-1911 period, when party strength was greatest in the entire House as well, and during the New Deal realignment era. The 1894-1915 period contains both a realignment and a post-realignment period when committee partisanship was high, which suggests two important points about partisanship and non-incremental appropriations decisions. I expect that during the realignment period 1895-1899 changes in appropriations would be greater than they were in the post-1900 period, even though partisanship remains high. Essentially this means that partisan behavior can yield both nonincremental and incremental decisions. The nonincre-

mental decision will occur in realignment periods when committee turnover is greatest. If I can show that the 1895-1899 and 1933-1939 periods were the most partisan and also the least incremental and that turnover in these periods was greatest, the results for the committee study would be similar to those found for the entire House.

Estimating the amounts of money appropriated is problematic during the First and Second World Wars, and to a lesser extent during the Spanish-American War. In order to reduce as much as possible the effects of war on appropriations decisions, I do not include defense appropriations in any of the 58 appropriations bills in the time

series. In addition, I calculated changes in appropriations for agriculture, rivers and harbors, commerce, and labor as a check against changes in total appropriations. Because these checks did not show significant differences from the overall appropriations, only the overall figures (minus defense) are given. Figure 2 shows the absolute changes from appropriations by previous Houses for the entire time series excluding World Wars I and II. The figure for each Congress is the average of the two appropriations bills passed by the House.

The results show that the highest absolute changes in appropriations occur during the realignments of the 1890s and 1930s. The only

Figure 2. Percentage Changes in Appropriations Committee Decisions, 1894-1950

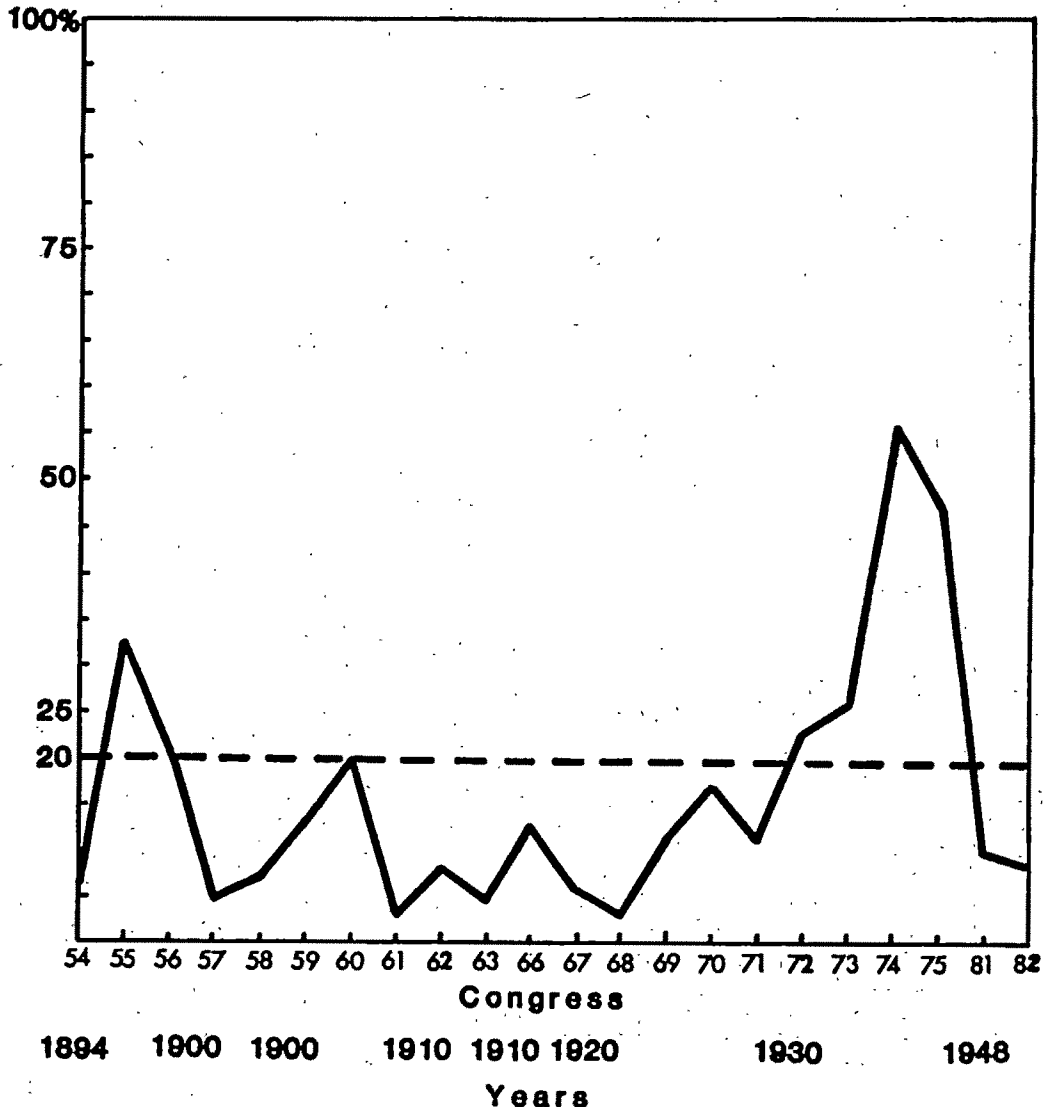


Table 11. Appropriations Committee Turnover, Party Voting and Changes in Appropriations, 1895-1949

	Committee Turnover		Average Index of Likeness		Average % Increment	
	\bar{x}	σ	\bar{x}	σ	\bar{x}	σ
1895-1899	46.6	11.5	20.5	0.7	27.0	8.5
1900-1915	35.9	18.1	34.3	4.2	7.6	7.1
1916-1932	26.0	13.6	57.6	12.4	4.5	18.9
1933-1939	34.3	10.9	29.7	1.2	34.1	19.4
1940-1949	24.4	8.0	54.0	9.3	9.6	14.3

changes in excess of 20% obtain in the realignment eras. The single exception is the change of over 20% in the 72nd House (1930), and that change was a decrease of more than 20% in President Hoover's attempt to balance the budget. The only positive changes above 20% are associated with the two major post-Civil War realignments.

At this point, I have shown that the Appropriations Committee has behaved in a partisan fashion during both realignments and during the period from 1900 to 1915 and that absolute changes in appropriations are greatest during realignments. What remains is to tie together committee turnover, partisanship, and major changes in appropriations. Table 11 shows the average turnover on the Appropriations Committee, the average Index of Party Likeness for committee members on relevant votes, and the average positive increases in appropriations. The hypothesis is that during the 1895-1899 and 1933-1939 realignment periods committee turnover, party voting, and appropriations will be high, and that high partisanship during the 1900-1915 period will not result in abnormally high change in appropriations. In addition it is expected that the 1916-to-1932 and 1940-to-1949 (excluding war expenditures) periods will follow the Fenno model of lower partisanship and incremental changes in appropriations.

The results support the argument. During the 1890s realignment partisanship was high with an average Index of Likeness of 20.5, which means that the average vote was about 80% of Republicans opposing about 80% of Democrats. The average increase in expenditures during these years was 27.0%, and the turnover on the Appropriations Committee averaged 46.6%. The New Deal realignment was also characterized by high turnover—34.3% party voting—Index of Likeness of 29.7 (about 75% of one party versus 75% of the other), and an average increase in expenditures of 34.1%. Thus the highest partisanship and highest change in appropriations occurred during realignment periods. The turnover on the Appropriations Committee during the 1900-1915 period was slightly higher than that found during the

New Deal realignment. However, it must be remembered that during more than two-thirds of this period the Speaker had the power to appoint committees. The 1895-1899 and 1933-1939 periods were highest in terms of electoral turnover; that is, incumbent members defeated in an election. Thus the slight difference observed in turnover between the 1900-1915 and 1933-1939 periods can readily be accounted for by internal structural factors. Turnover on the committee brought about by electoral results appears to bring about the most dramatic changes in partisanship and changes in expenditures.

The 1900-1915 period is characterized by relatively high partisan voting, but the average increase in appropriations is only 7.6%. The 1916-to-1932 and 1940-to-1949 periods have the lowest turnover, the lowest levels of partisan voting, and average increases in appropriations below 10%, a figure commonly used to define incrementalism.

Conclusions

There are two sets of findings in this article. The first is that there are similar linkages present in all three major American realignments. Realigning elections at the congressional level are determined more by national issues than by local factors. The electoral turnover generated by the realigning election(s) results in a congressional majority party that behaves in a partisan fashion to achieve major policy changes. The article shows that whether the unit of analysis is congressional voting on roll calls or Appropriations Committee money decisions, the electoral turnover generated by the realignment results in an American form of party government. The reduction of constituent-party cross pressuring and the sense of mandate from the election results creates a new majority party capable of legislating major shifts in American public policy.

The second set of findings shows that at least two of the major American realignments do not meet a number of the criteria normally associated with electoral realignments. Neither the Civil War nor 1890s realignments were of the magnitude or

nature of the New Deal realignment. In both these realignments relatively minor shifts of voters to the Republicans in the North resulted in long periods of Republican dominance of government. In large part, both the original electoral success and subsequent control were the result of a vote-seat distribution that favored Republicans. These vote-seat distortions especially favored the Republicans in the realigning election or elections. Thus, although there appears to be structural similarities across all three realignments, it should also be clear that interpretations of American realignments that automatically ascribe policy results to massive voter shifts are facile.

Like almost all research on realignments, this article has focused on electoral and structural change. Very little attention has been paid to the role of congressional leadership in obtaining these policy shifts. There have been other periods in American political history where the electoral and structural shifts associated with policy change have been present (Clubb, Flanigan, & Zingale, 1980). The Republicans' victory in 1920 and their domination of government for a decade comes to mind, yet we know that there are no major shifts in policy associated with this period. Why some periods result in major policy changes while others do not remains an unanswered, even an unasked question. Certainly the way congressional leaders interpret election results and how they translate their perceptions into policy greatly affects policy outputs. Future research will have to focus on the role of congressional leadership in formulating and adopting policy responses to critical elections. Franklin Roosevelt is unanimously viewed as the architect of the New Deal; yet we know that Congress forced some policies, the Wagner Act for example, on a reluctant Roosevelt. Until we know how Congress itself affected the policy process during such eras, we will continue to have a view of realignments that overemphasizes the effects of both elections and presidents.

Reducing the explanatory strength of electoral and presidential variables allows us to concentrate on how electoral results change congressional structure and agendas. Focusing on structural and agenda variables begins to tie realignment studies to the social choice results generated by Shepsle (1979) and Ferejohn (1974).

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Perceived Representativeness and Voting: An Assessment of the Impact of "Choices" vs. "Echoes"

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The persistent decline in voting in presidential elections since 1960 has resulted in serious scholarly attention being given to nonvoting. Despite the quality of these studies, however, the ratio of what we know about nonvoting to what we do not know is rather low. In the hopes of improving this situation, I advance the hypothesis that one reason that individuals do not vote is that their interests are not represented by any of the major candidates. To test this hypothesis, I used the SRC election studies (1968-1980) to construct measures of individuals' perceived distance from the major candidates on a variety of issues. Net of an extensive set of factors usually invoked to explain participation, increased distance from candidates significantly decreases the probability of voting. The implications of these results are discussed.

The persistent decline in voter turnout in presidential elections since 1960 has resulted in serious scholarly attention being given to nonvoting. There is now a growing number of studies that attempt primarily to explain this trend and secondarily to explain nonvoting in general (e.g., Abramson & Aldrich, 1982; Boyd, 1981; Cassel & Hill, 1981; Cavanagh, 1981; Hadley, 1978; Katosh & Traugott, 1982; Ladd, 1978; Reiter, 1979; Rosenstone & Wolfinger, 1978; Schaffer, 1981; Tarrance, 1978; Wolfinger & Rosenstone, 1980). Despite the quality of these studies, serious gaps in our knowledge of nonvoting still exist, and I hope that this study will begin to fill some of them.

Recent nonvoting studies have emphasized three principal kinds of factors: 1) socioeconomic and demographic (e.g., income, education, age, and sex); 2) social psychological (e.g., party identification, citizen duty); and 3) rational choice variables (e.g., the perceived closeness of the election) (Cassel & Hill, 1981; Cavanagh, 1981; Reiter, 1979; Shaffer, 1981). The procedure for testing these relationships is generally the same: through some extension of the general linear model, a dichotomous dependent variable of voting-nonvoting is related to some subset of known or suspected correlates. These studies suggest that the

most important factors contributing to the decline in turnout have been the changing age structure (see also, Boyd, 1981), a decrease in participation among lower status citizens with no counterbalancing increase, and declines in both political partisanship and feelings of political efficacy.

Despite their value, however, these analyses have a major shortcoming. In line with the dominant emphasis in electoral research in the United States, they focus primarily on individual factors and do not consider the importance of the options available to individuals. Turnout among any segment of the electorate is known to be decreased by the absence of a realistic party or candidate choice expressing the interests of that segment (see Milbrath & Goel, 1977; Zipp & Smith, 1982), but this finding has not been incorporated into studies of recent voting declines. In this article, I present evidence of how consideration of the options available to individuals helps to explain nonvoting.

Background

Converse (1971) listed three broad factors that keep people from voting: accidental, legal, and motivational. Accidental factors do not concern us because there is little that can be done about them. Legal factors, on the other hand, clearly explain some portion of nonvoting, as it is harder both to register and to vote in the United States than in virtually any western democracy. However, legal factors cannot explain the decrease in turnout since 1960 because since that time legal restrictions on registration and voting have been relaxed (e.g., Rosenstone & Wolfinger, 1978). The third set of factors, motivational ones, is generally considered to be the most important. Converse surmised that typically motivational factors in-

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volve some indifference or apathy to voting. He further distinguished two types of motivational factors: internalized motivation (the learned need for citizen participation), and external stimulation (the mobilizing effects of the excitement surrounding a presidential campaign).

Although attention has been given to both of these aspects of motivation, perhaps much more has been given to internalized motivation. Brody (1978), Cassel and Hill (1981), Cavanagh (1981), Reiter (1979), and Schaffer (1981) all cite the expectation that increased levels of education during this period should have resulted in increased turnout. In other words, because more people became aware of the need for and the benefits of participation, more people should have voted. The effects of education, however, may have been offset by external factors: the enfranchisement of 18-year-olds and the increased distrust of and cynicism toward the government in the wake of the Vietnam war and Watergate. Lowering the voting age increased the size of the electorate and artificially lowered turnout rates (when compared to earlier years), because 18-20 year olds have low rates of turnout (e.g., Boyd, 1981). The same cannot be said for political trust, as Schaffer (1981) found no significant independent effect of trust on turnout.

There is one major external factor which generally has been ignored. After finding that lower status whites had higher rates of nonvoting than others, Reiter (1979) drew on Burnham (1967) and mentioned that a reason for this failure to vote might be the lack of a viable socialist party for them to support. In comparative terms, the socioeconomic gap in participation is greater in the United States than in most other western democracies (Verba, Nie, & Kim, 1978; Zipp, Landerman, & Luebke, 1982). Although this is a provocative idea, there has been no direct test of it in the United States and, indeed, Reiter (1979) went so far as to speculate that such a test might not be possible with current data.

Although it is not my purpose to determine whether or not lower status citizens want a socialist party, I do contend that turnout among various groups in the electorate is affected by the presence or absence of a party that represents the interests of those groups. In their study of voting behavior in Canada, Zipp and Smith (1982) found that nonvoting among members of the working class decreased in constituencies in which there was a viable candidate of a mildly socialist party (the New Democratic Party), the party that they presumed to represent the interests of the working class. Furthermore, research done by Campbell and his colleagues, along with others, has found that turnout increased when people perceived clear and attractive alternatives in a particular

election (Campbell, 1960, 1962; Campbell et al., 1960; Rokken & Valen, 1962). Based on this work, I have tested the hypothesis that accounting for whether or not one's interests are represented among the various candidates increases our ability to explain turnout.

Data and Methods

The data for this article are drawn from the Survey Research Center's American National Election Studies. These data are available from the Survey Research Center at the University of Michigan and are a standard source for studies of voting in presidential elections.

Because my hypothesis concerns the idea that the options available to individuals affect voting, a central task is to obtain measures that indicate whether or not the respondents felt that their interests were represented by any candidate in the relevant election. Although there is no direct information on this, I was able to construct measures that reflect on a 7-point scale the distance between the respondent's position on a particular issue and the respondent's perception of each candidate's position on that same issue. The closer a respondent is to a candidate, the more that respondent feels his or her interests on that issue are represented by that candidate. Perhaps the situation most conducive to high levels of voting would occur when all citizens are close to one candidate but a considerable distance from the others, so that there would be clear, representative choices for everyone. The least conducive situation, on the other hand, would occur when all individuals are equidistant from all candidates. In the latter case, individuals would be indifferent about who wins the election, whereas in the former instance, who wins would make a difference to the voters. What I have outlined, in fact, is very similar to Brody's and Page's (1973) notion of indifference. Drawing on the idea of rational abstention (Downs, 1957), Brody and Page demonstrated that those who felt equally about the candidates were less likely to vote than those who clearly favored one candidate.¹ Thus, *indifference* is one way in which the lack of representation can lead to nonvoting.

As Brody and Page also noted, It is possible to have a relatively clear choice but yet, at the same time, not to feel favorably disposed toward any of the candidates. Brody and Page termed this notion, which draws most closely from Con-

¹Brody and Page (1973) did not use these perceived distance measures, but rather used a summary evaluation of how favorably the respondent felt toward each candidate.

verse's (1966, p. 24) idea of "dynamic nonvoting," *alienation*. Brody and Page found that even though alienated citizens might not be indifferent, they were also less given to voting. Thus, it is through both indifference and alienation that the absence of preferred options can lead to nonvoting.²

The questions on which the measures of both indifference and alienation are based are listed in the Appendix. Because these questions were included only in the SRC surveys from 1968 through 1980, my analysis must be confined to those years. In each year I use the absolute value of the difference between a respondent's position and the respondent's perception of the candidate's position. Absolute values are used because I am concerned not with the direction of the distance between the respondent and the candidates, but with the gap itself. In each of the four elections, I measured the gap between the respondent's positions and the perceived positions of the major candidates: Nixon, Humphrey, and Wallace in 1968; Nixon and McGovern in 1972; Ford and Carter in 1976; and Reagan, Carter, and Anderson in 1980.

Indifference. I expect the situation most conducive to voting to occur when one candidate's positions are close to one's own position and all other candidates' are distant from it. To operationalize this notion, I took the absolute value of the difference between the respondent's position on a particular issue and the respondent's perception of each particular candidate's position on that same issue (*pdD*, *pdR*, and *pdM* for the absolute value of the perceived distance from the Democratic, Republican, and minor party candidates). Thus, in 1968 there were six measures: the absolute distance between the respondent and 1) Nixon on the Vietnam War and on urban unrest; 2) Humphrey on the Vietnam War and on urban unrest; and 3) Wallace on the Vietnam War and on urban unrest. In 1972, there were eight measures; that is, the absolute distance from the respondent and both Nixon and McGovern on each of four issues: liberalism, jobs, minority

rights, and equality for women. Similar measures were obtained for the same four issues in 1976 and 1980, with the latter including distance from Anderson's position.

To construct a measure indicating whether one was close to or distant from any or all candidates, I proceeded as follows. In 1972 and 1976—when there were only two major candidates—I took the absolute value of the difference between the respondent's and Democratic candidate's positions (*pdD*) and the respondent's and Republican candidate's positions (*pdR*) on each issue and reversed the scoring ($(|pdD - pdR| \times -1) + 7$). Thus, if the respondent were close to one candidate and distant from the other, the respondent would get a low score on this measure for each particular issue. Similarly, if the respondent was close to or distant from both candidates, he would receive a high score. Thus, we would expect a negative relationship between scores on these indexes and voting: as the choice between candidates becomes less clear (i.e., as indifference increases), individuals should be less likely to vote.

I wanted to retain the same scoring and scale for the 1968 and 1980 elections, and to do so I had to adjust for the presence of a third candidate. In each election I made contrasts for each issue similar to the ones described immediately above. To illustrate this process for 1968, I created three new terms: *RD* = the absolute value difference between the respondent and Nixon (*pdR*) and the respondent and Humphrey (*pdM*); *RM* = the respondent and Nixon (*pdR*) vs. the respondent and Wallace (*pdM*); and *DM* = the respondent and Humphrey (*pdD*) vs. respondent and Wallace (*pdM*). I then subtracted the smallest of these new terms from the largest and reversed the scoring (as above). This allowed me to capture how clear one's choice was: if there is little difference between the largest and smallest of these measures (*RD*, *RM*, *DM*), then the respondent is indifferent. If, on the other hand, there is a big gap, the respondent is not indifferent. For example, assume that a respondent is 0 units from Humphrey, 5 units from Nixon, and 6 units from Wallace. From these scores it is obvious that the respondent has a clear choice (Humphrey) and is therefore not indifferent, and my measure of indifference must apprehend this. In this case, *RD* is $|5 - 0| = 5$; *RM* is $|5 - 6| = 1$; and *DM* is $|6 - 0| = 6$. Indifference would be measured as the largest of these (*DM*) minus the smallest (*RM*), with the scoring reversed: $((6 - 1) \times -1) + 7 = 2$. Thus, the respondent would have a very clear choice, as the only clearer choice would be indicated by a score of 1, and my measure of indifference reflects accurately the respondent's perceived choices. To reiterate: voting should be negatively related to indifference.

²In a somewhat similar vein, Kelley and Mirer (1974) used both partisanship and affect toward candidates and parties to predict (primarily) the direction of voting. Although they also looked briefly at nonvoting, they acknowledged that there was an important difference between studying decisions of whether to vote and the direction of the vote. "A comparison of candidates resulting in a choice among them should be one consideration—but not the only one—in the decision about whether to vote" (Kelley & Mirer, 1974, p. 574). I agree with them and include some of these other factors in my model (e.g., political efficacy, political interest, whether one was mobilized to vote).

Alienation

The measure of alienation is more straightforward. The concept of alienation is similar to the situation described above: respondents are distant from all the candidates on the perceived distance measures (*pdD*, *pdR*, *pdM*). I have operationalized alienation as the minimum of the absolute values of the perceived distance measures on each of the items.³ A low score on these minimum distance variables—or alienation—indicates that the respondent is close to at least one candidate (i.e., the respondent is not alienated), whereas a high score means that one is not close to any candidate (i.e., alienated). Thus, the alienation variables also should be negatively related to voting: as alienation increases, the likelihood of voting should decrease.

In sum, the lack of representation can lead to nonvoting through the absence of a clear choice (indifference, as measured by the joint distance variables), through the lack of any candidate close to one's positions (alienation, as indicated by the minimum distance measures), or through some combination of both.

Unlike earlier researchers (e.g., Brody & Page, 1973), I developed an extensive model of factors thought to explain voting and only considered the impact of indifference and alienation net of these other factors. This approach should help to guard against reaching a spurious interpretation of the relationship between interests and voting. All of these other independent variables and their coding are listed in Table 1. For each of them, missing data were recoded to the mean. Because I am only assessing the impact of the distance variables after controlling for age, sex, race, region of the country, education, occupation, income, whether one was contacted by the parties, political efficacy,⁴ political interest,⁵ and party identification, these will be conservative tests of my hypothesis that having one's interests represented increases the probability that one will vote.

Before continuing, it is important to note the

³The measure of indifference does not distinguish between two different reasons for nonvoting: being indifferent because one is equally close to all candidates and being indifferent because one is equally distant from them all. Alienation addresses this in part, as it captures whether one is close to any candidate. I thank an anonymous reviewer for bringing this to my attention.

⁴I used two items to measure external political efficacy (e.g., Abramson & Aldrich, 1982): 1) "People like me don't have any say about what the government does," and 2) "I don't think that public officials care much about what people like me think."

⁵Political interest is a measure of the respondent's interest in the political campaign.

limitations of these measures. First and most serious is the direction of causal flow between the perceived distance measures and voting. I have assumed that the perceived distance variables precede voting causally, but it is plausible that the reverse is true. Justification for this reasoning can be found in cognitive dissonance theory under the rubric of "post-decisional conflict reduction" (e.g., Brehm, 1956; Crawford, 1976). Basically this term describes a process in which once a choice is made among various options, the subjects tend to evaluate their chosen option much more positively than they did before making the choice, with the reverse being true for the options that were not chosen. Thus, in a postelection interview—in which voting decisions already have been made—it is possible that respondents first decided whether or not to vote and then rationalized whether they were close to any of the candidates.

Although this is a realistic possibility, I feel that the predominant causal flow is from attitudes to behavior. I base this judgment on the work of Brody and Page (1973). In their study of the 1968 election, they tested this notion by comparing pre-election and postelection candidate evaluations. Even though they found some evidence of post-decisional rationalization, they concluded that it "was not sufficiently prevalent" to warrant a reversal of causal attribution.⁶ However, because some rationalization does occur, the conclusions need to be tempered by this fact, and the reader should bear this in mind.

Second, there is no guarantee that the issues that have been measured are salient and ones upon which voting decisions are made.⁷ Given these and the fact that the major party candidates often do not differ very much from each other (e.g., Page, 1978), it may be better to view our analyses as suggestive.⁸

⁶What ideally would be needed is a comparison of the pre- and post-election evaluations of those who were undecided immediately before making the decision to vote or not to vote. According to my approach, these voters with no clear choice should not vote and should still have no clear choice after the election. Should they vote and have a clear choice after the election, this would indicate the presence of post-decisional conflict reduction.

⁷In 1968, questions were asked concerning the importance of these particular issues in the respondent's voting decision. These results indicate that these two issues were of substantial importance to the respondents.

⁸Fiorina (1981, pp. 139-143) pointed out that measures such as those which ask one to compare someone else's position with one's own position are subject to a psychological process in which one rationalizes the other's position as being near to one's own. Although

Table 1. Independent Variables Used in the Analyses

Variable	Description	Coding
Middle aged	R's age	1 = 35-54, 0 = All else ^a
Older aged	R's age	1 = 55+, 0 = All else
Sex	R's sex	1 = Male, 0 = Female
Race	R's race	1 = White, 0 = Nonwhite
South	Region where R lives	1 = South, 0 = Nonsouth
Some high school education	R's education level	1 = 9-11 yrs., 0 = All else ^b
High school graduate	R's educational level	1 = 12 yrs., 0 = All else
Some college education	R's educational level	1 = 13-15 yrs., 0 = All else
College graduate	R's educational level	1 = 16+ yrs., 0 = All else
Professional-technical	Occupation of head of household	1 = Professional, technical jobs, 0 = All else ^c
Clerical-sales	Occupation of head of household	1 = Clerical, sales job, 0 = All else
Blue collar	Occupation of head of household	1 = Blue collar job, 0 = All else
Family income	R's family income	In dollars
Contact	Whether R was contacted	1 = Contacted, 0 = Not contacted
Political efficacy	R's feelings of political efficacy	0-2; lowest to highest
Political interest	R's interest in politics	1-5; Not interested to Very interested
Democratic ID	R's party identification	1 = Democrat, 0 = All else ^d
Republican ID	R's party identification	1 = Republican, 0 = All else
Indifference: Liberalism	R's joint distance from all major candidates on the liberalism scale.	1-7, with 1=R close to one and distant from all other candidates.
Indifference: Jobs	R's joint distance from all major candidates on the jobs question.	Same as above
Indifference: Minority rights	R's joint distance from all major candidates on the minority rights question.	Same as above
Indifference: Women	R's joint distance from all major candidates on the women question.	Same as above
Indifference: Urban	R's joint distance from all major candidates on the urban question.	Same as above
Indifference: Vietnam	R's joint distance from all major candidates on the Vietnam question.	Same as above
Alienation: Liberalism	Minimum of the absolute value of R's perceived distance from any of the candidates on the liberalism scale.	1-7, with 1=R close to at least one candidate and 7=R distant from all candidates.
Alienation: Jobs	Minimum of the absolute value of R's perceived distance from any of the candidates on the jobs question.	Same as above
Alienation: Minority rights	Minimum of the absolute value of R's perceived distance from any of the candidates on the minority rights question.	Same as above
Alienation: Women	Minimum of the absolute value of R's perceived distance from any of the candidates on the women question.	Same as above
Alienation: Urban	Minimum of the absolute value of R's perceived distance from any of the candidates on the urban question.	Same as above
Alienation: Vietnam	Minimum of the absolute value of R's perceived distance from any of the candidates on the Vietnam question.	Same as above

^aOmitted category is young-age 21-34.^bOmitted category is 8 or fewer years of education.^cOmitted category is managerial jobs.^dOmitted category is independent, other or no party identification.

Results

The dependent variable is a dichotomous, vote-nonvote one with voting coded 1 and nonvoting coded 0. Because there are problems with the use of ordinary least squares regression with dichotomous dependent variables, I have chosen to use logistic regression. Good discussions of the problems of OLS with dichotomous variables and the benefits of logit analysis can be found in Dwyer (1983) and Hanushek and Jackson (1977).

The results of the logit analyses are shown in Table 2. Because I am interested only in the distance variables, I discuss only their effects. Furthermore, because I have no *a priori* reason to expect any of these distance variables to be more important than the others and because they are positively correlated with each other, I am more concerned with the performance of each set of them rather than any particular one.

For this reason, and because there is no clear rationale for the order in which the alienation and the indifference variables should be entered into the equations relative to each other, I chose to present the results of all the possibilities. I entered the alienation variables first, the indifference ones first, and both sets together. In order to determine whether or not the lack of representation is related to voting, I compared the increments in chi-square associated with including both the alienation and the indifference factors.⁹ I also examined the chi-square increments of alienation alone and alienation net of indifference, and vice versa. Taken together these comparisons should enable us to assess 1) if accounting for whether or not one's interests are represented increases our ability to explain nonvoting, and 2) if this is the result of indifference, alienation, or both.

In all four elections, the alienation and indifference variables when considered together significantly increase our ability to explain nonvoting (see Table 2). In 1968 this is primarily the result of the effects of indifference to the Vietnam

War (-.16), in 1972 to indifference to liberalism (-.15), in 1976 to indifference on the role of women (.19), and finally in 1980 to alienation on the jobs question (.19). At the most general level, then, this constitutes initial evidence that the lack of representation increases nonvoting.

One still needs to consider the separate impacts of alienation and indifference. Turning first to alienation, as a set the alienation variables alone significantly increase the explanation of nonvoting in 1972 (chi-square increment = 22.62 with 4 *df*), in 1976 (chi-square increment = 9.78 with 4 *df*), and in 1980 (chi-square increment = 14.14 with 4 *df*). In 1972 the lack of representation of liberalism (-.30) and minority rights (-.14) is primarily responsible, in 1976 liberalism (-.23) again is significant, whereas in 1980 it is because of alienation on the jobs question (-.21). Finally, in all elections the direction of the significant coefficients is as expected: increases in alienation are associated with decreases in voting.

Accounting for indifference significantly enhances our understanding of voting in 1968 (chi-square increment = 9.60 with 2 *df*), in 1972 (chi-square increment = 33.79 with 4 *df*), and in 1976 (chi-square increment = 16.69 with 4 *df*). In 1968, it results primarily from the joint distance between the respondents' and the candidates' views on the Vietnam War (-.18), with the gap between the respondents' and the candidates' views on liberalism (-.20) and on minority rights (-.10) statistically significant in 1972, and liberalism (-.14) and an equal role for women (.19) significantly related to voting in 1976. Further, except for the women issue in 1976, the direction of the coefficients is as expected: as indifference increases, voting decreases.¹⁰

From the above, it is clear that both indifference and alienation lead to nonvoting. To determine better which factor has a greater impact, I have undertaken two procedures. First, I examined the increments in chi-square for each of the two sets of distance variables net of the other set. The results of these comparisons (also listed in Table 2) indicate that, net of indifference, aliena-

this is a problem, if such a process is operating in these data, it will result in having most people report being closer to the candidates than they actually are. This will attenuate the relationships between these measures and voting, and thus the results may underestimate the effects of having one's interests represented on the likelihood of voting.

⁹Testing for the significance of a set of variables involves comparing the chi-square value obtained from a model with those variables to the chi-square value obtained from a nested model excluding that set of variables. If there is a significant increment in chi-square when the set is included, then that set of variables has significantly increased our explanatory power.

¹⁰As an anonymous reviewer brought to our attention, failure to answer the candidate placement questions could lead to nonvoting if this failure indicated that the respondent was unable to perceive a candidate's position. I checked for this possibility by creating new variables representing missing-nonmissing on each of the self- and candidate-placement items. As a set, these variables were not significant in 1968 and 1976, and in the other two elections their significance is mainly due to the respondents' failing to answer the self-placement questions (these respondents were then not asked the candidate placement questions). Thus, this explanation largely is not valid.

Table 2. Logistic Regression Coefficients for Voting, 1968-1980

	1968			1972			1976			1980		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Middle aged	.92*	.92*	.92*	.67*	.64*	.66*	.99*	.95*	.96*	.78*	.74*	.76*
Older aged	1.24*	1.24*	1.24*	.80*	.74*	.76*	1.01*	.97*	.96*	1.23*	1.17*	1.22*
Sex	.19	.21	.21	.15	.15	.14	.37*	.32*	.32*	.11	.07	.06
Race	.09	.24	.21	-.17	-.08	-.08	-.11	-.05	-.06	-.09	-.03	-.07
South	-.38*	-.35*	-.36*	-.46*	-.54*	-.51*	-.45*	-.44*	-.44*	-.24	-.25	-.24
Some high school education	.31	.31	.30	.20	.15	.16	.17	.14	.14	.00	.03	.02
High school graduate	.97*	.98*	.98*	.76*	.74*	.74*	.63*	.59*	.58*	.74*	.71	.74*
Some college education	.52	.52	.52	1.12*	1.04*	1.04*	1.26*	1.22*	1.20*	.68*	.67*	.66*
College graduate	.72*	.71	.71	1.15*	1.10*	1.06*	.98*	.92*	.91*	1.67*	1.71*	1.66*
Professional-technical	.32	.30	.32	.22	.22	.22	.46	.48	.48	-.28	-.27	-.28
Clerical-sales	.41	.46	.44	.20	.16	.18	.05	.06	.07	-.01	-.02	-.01
Blue collar	-.29	-.30	-.30	.05	.05	.05	.04	.06	.07	-.17	-.18	-.16
Family income ^a	.64*	.66*	.66*	.45*	.44*	.44*	.29*	.30*	.28*	.10	.08	.09
Contact	.56*	.54	.55*	.76*	.74*	.74*	.71*	.73*	.74*	.71*	.68*	.70*
Political efficacy	.41*	.45*	.44*	.27*	.30*	.29*	.30*	.31*	.30*	.48*	.49*	.48*
Political interest	.34*	.33*	.32*	.32*	.30*	.30*	.33*	.33*	.33*	.42*	.43*	.42*
Democratic ID	.63*	.63*	.63*	.61*	.62*	.62*	.42*	.39*	.40*	.74*	.71*	.74*
Republican ID	.57*	.57*	.56*	.79*	.78*	.78*	.56*	.56*	.55*	.83*	.78*	.80*
Alienation: Urban	-.07		-.05									
Alienation: Vietnam	-.14		-.09									
Indifference: Urban		-.04	-.02									
Indifference: Vietnam		-.18*	-.16*									
Alienation: Liberalism												
Alienation: Jobs				-.39*		-.16	-.23*		-.17	-.20		-.12
Alienation: Minority rights				.00		.04	.03		.07	-.21*		-.19*
Alienation: Women				-.14*		-.12*	-.06		-.04	-.14		-.15
Indifference: Liberalism				-.02		.03	-.02		-.06	-.01		-.03
Indifference: Jobs					-.20*	-.15*		-.14*	-.08		-.14	-.08
Indifference: Minority rights					-.06	-.08		-.06	-.07		-.10	-.06
Indifference: Women					-.10*	-.08		-.04	-.03		-.01	-.02
Constant					-.01	-.02		.19*	.19*		.07	-.07
	-2.22	-3.36	-2.96	-1.30	-2.99	-2.46	-1.79	-2.56	-2.00	-1.53	-2.95	-1.70
Chi-square	335.78*	340.67*	342.53*	514.91*	526.08*	534.52*	446.23*	453.14*	458.54*	391.63*	394.37*	401.11*
df	20	20	22	22	22	26	22	22	26	22	22	26
Basic Model Chi-square ^b	331.07*			492.29*			436.45*			384.98*		
df	18			18			18			18		

Table 2 (continued)

	1968			1972			1976			1980		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Increment in Chi-square for: Alienation and Indifference <i>df</i>	11.46*			42.23*			22.09*			16.13*		
	4			8			8			8		
Alienation	4.71			22.62*			9.78*			14.14*		
Alienation net of Indifference	1.86			8.44			5.40			9.48†		
Indifference	9.60*			33.79*			16.69*			6.65		
Indifference, net of alienation <i>df</i>	6.75*			19.61*			12.31*			1.99		
	2			4			4			4		

*Multiply coefficient by .0001.

†All independent variables, except for alienation and indifference.

* $p < .05$.† p approximately .055.

tion never produces a significant increment in chi-square, whereas accounting for indifference significantly augments our understanding of non-voting—net of alienation—in 1968 (chi-square increment = 6.75 with 2 *df*), in 1972 (chi-square increment = 19.61 with 4 *df*), and in 1976 (chi-square increment = 12.31 with 4 *df*). Thus, it appears at this point that indifference is more important than alienation.

I have approached this issue in a second way. Logistic regression coefficients, unlike OLS coefficients, do not have a constant effect on the dependent variable across all values of the independent variables, so the effect of one independent variable must be evaluated at its different values and at certain values of all other independent variables. It is conventional to make these evaluations at the means of the other independent variables. Thus, to provide further insight on the impacts of alienation and indifference, I have calculated the probabilities of voting for the alienation and indifference variables, holding all other variables—including whichever set of distance variables is not being evaluated—at their respective means (see Table 3). I can illustrate how I arrived at these figures by using alienation in 1968 as an example. (The same procedure was used in all years and also for indifference.) I first multiplied the logistic coefficient of each of the independent variables listed in Table 3, column 3, except for the alienation measures, by the mean of the respective variable. For each alienation measure, I multiplied the logistic coefficient by the respondent's actual score on the respective variable. Thus, each respondent was given the mean value on all the independent variables except for alienation, on which they received their actual value. After summing these together along with the intercept and terming this new quantity XB , I calculated the probability of voting as equal to: $1/(1 + \exp(-XB))$. This gives the probability of voting across levels of alienation, with every other factor held at its mean. Because all variables, except for alienation, are fixed at their means, all the variance in the probability of voting results from alienation (or likewise from indif-

ference, when I calculate the results for it). Thus, the standard deviations of these probabilities supply information about the relative effects of alienation and indifference.

These results, shown in Table 3, indicate that in all but 1980 the standard deviations of indifference are considerably larger than those of alienation.¹¹ This difference in standard deviations can be interpreted to signify that there is greater variability in the likelihood of voting across levels of indifference than across levels of alienation. To illustrate, let us assume that being one standard deviation above or below the mean indicates that one is very high or very low on a particular measure. Given this, in 1968 the difference in the probability of voting between a highly indifferent (.7825) and not indifferent person (.8511) was almost 7%. The same comparison for an unalienated (.8241) and an alienated individual (.7923) produced a difference of slightly more than 3%. Similarly, the differences between those who were one standard deviation above and one standard deviation below the mean were 9% for indifference and 6% for alienation in 1972, and 8% for indifference and 5% for alienation in 1976. Taken together, these results confirm that both alienation and indifference lead to non-voting, but that indifference has a greater impact.

Summary and Discussion

The most important finding is that measuring the gap between an individual's positions on issues and his perceptions of all the candidates' positions on the same issues, net of a host of factors usually invoked to explain voting, significantly increases one's ability to explain turnout. In general, if one has a clear choice among the candidates (is not indifferent) and one's policy preferences are close to at least one candidate (one is not alienated), one is much more likely to vote. Furthermore, although both alienation and indif-

¹¹The means are approximately equal throughout, as they should be.

Table 3. Probabilities of Voting by Alienation and Indifference, 1968-1980^a

	Alienation				Indifference			
	\bar{X}	SD	+1SD	-1SD	\bar{X}	SD	+1SD	-1SD
1968	.8082	.0159	.7923	.8241	.8168	.0343	.7825	.8511
1972	.7835	.0297	.7538	.8132	.7712	.0449	.7263	.8161
1976	.7829	.0269	.7560	.8098	.7849	.0392	.7457	.8241
1980	.7652	.0447	.7205	.8099	.7757	.0253	.7504	.8010

^aAll other variables are evaluated at their means.

ference can lead to nonvoting, being indifferent to the candidates has a greater impact on nonvoting than does being alienated from them. The fact that these results were obtained net of an extensive set of factors related to voting in different elections provides additional evidence of their existence and persistence.

There are several implications of these results. First, this research was prompted by a concern that the options available to individuals generally have been neglected in explaining nonvoting in the United States. The extant literature focuses almost solely on individualistic characteristics, ignoring the interplay between the attributes of individuals and the choices presented to them. For example, in his study of turnout Cavanagh (1981, p. 63) contended that "the current rightward drift in policy initiatives may owe its source to the contemporaneous class shift in participation patterns." Thus Cavanagh blames the victim: if more lower class people had participated, the current rightward shift might not have occurred. His interpretation assumes that the politics of the elite faithfully mirror those of the mass electorate. Although this assumption is central to responsive democratic government, research in both the United States (e.g., Hamilton, 1972) and other countries (see Zipp, 1978, for a summary) calls it into question. Although we know that various individual-level factors are associated with nonvoting (see Table 2) and cannot be ignored, I am suggesting that the fit between individuals and the options presented to them also needs to be incorporated into explanations of nonvoting.

These results have a second implication for individualistic approaches to explaining nonvoting. Another way of saying that the lack of a representative candidate decreases voting is that nonvoting may be the outcome of a reasoned, thoughtful political position. However, recent research on nonvoting tends to treat it as the failure to act politically rather than as a chosen form of political action. The difference between these two is quite important. For example, treating nonvoting as a failure to act, Brody (1978), Cassel and Hill (1981), Cavanagh (1981), Reiter (1979), and Schaffer (1981) all predicted that increased levels of education from 1960 to 1980 would result in increased levels of voting, because education increases the awareness of the needs for and the benefits of voting. The underlying assumption is clear: nonvoting stems from a lack of education or knowledge, not from an intelligent, reasoned position. Thus, the solution to the problem of nonvoting is to educate the nonvoters. However, taking the perspective that nonvoting may be a thoughtful, chosen stance directs one away from educating the nonvoter and toward an examination of the factors that cause nonvoting

to be a legitimate alternative way of expressing political preferences. In this article I have contended that one of these reasons is that individuals do not have their interests represented in the political sphere. For some citizens, nonvoting may be a failure to act politically, but for others it is a chosen form of political action. Neither reason for nonvoting can be treated as the other.

There is one approach to explaining nonvoting with which my results generally are quite consistent: rational choice theory and the idea of rational abstention (e.g., Brody & Page, 1973; Davis, Hinich, & Ordeshook, 1970; Downs, 1957; Ordeshook, 1970). At the risk of oversimplification, this view argues that, given the costs of voting, if the outcome does not make any difference, the rational citizen does not vote. Indeed I have shown that not having a clear choice among the candidates (i.e., not having a preferred outcome) leads to nonvoting, and thus these results can be used to support this notion of rational choice theory.

However, as noted above, this research was not motivated by seeking to test the utility of Downsian spatial models. Rather, I was influenced most by a concern for the lack of options that exist for many individuals,¹² and because I have established that the absence of representation leads to nonvoting, it is appropriate for scholars to ask why these options do not exist for many citizens. Although a thorough examination of this question is beyond the scope of this article, a brief outline is suggested. Single district, single member, "first-past-the-post" presidential electoral systems encourage the development of two centrist, brokerage-style parties and weaken support for third parties. Combined with historical and other forces (e.g., American Political Science Association, 1950), such rules have resulted in a less-than-responsible two-party system in which the parties do not provide coherent and distinct ideologies and programs. This system negates even the requisites of the "competitive theory of democracy" and elevates nonvoting to the level of a third choice. In addition, several current factors create a considerable distance between candidates and most citizens: the upper-middle-class backgrounds of most candidates; the need of candidates to have or to be able to attract the large sums of money required to conduct viable cam-

¹²Looking only at the marginals for the indifference variables, for 10 of the 14 variables more than 50% of the respondents are in the two lowest categories—i.e., the "least representation" categories. In two of the remaining four, more than 40% are in these categories. Thus, it is safe to say that these options do not exist for a good number of citizens.

paigns; the importance of the media and their almost complete neglect of third-party candidates; and, perhaps owing to the increased role of the judiciary and other unelected officials (Burnham, 1967), the belief on the parts of some that elected candidates cannot really make good on their promises (e.g., Kimball, 1972; Parenti, 1980, especially chap. 11).

In conclusion, I feel that it would be more fruitful intellectually if scholars shifted their focus away from explaining nonvoting primarily by noting the deficiencies of nonvoters to one that also includes an understanding of the circumstances that lead some individuals to use nonvoting as a reflection of political beliefs.

Appendix:

Candidate Distance Questions, 1968-1980

(1) Urban (1968 only)

There is much discussion today about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to use all available force to maintain law and order—no matter what the results. Others say it is more important to correct the problems of poverty and unemployment that give rise to the disturbances. And, of course, other people stress opinions in between. Suppose the people who stress the use of force are at one end of this scale—at point number 7. And suppose the people who stress doing more about the problems of poverty and unemployment are at the other end—point number 1. Where would you place (yourself, Humphrey, Nixon) on this scale?

(2) Vietnam (1968 only)

There is much talk about "hawks" and "doves" in connection with Vietnam and considerable disagreement as to what action the United States should take in Vietnam. Some people think that we should do everything necessary to win a complete military victory, no matter what results. Some people think we should withdraw completely from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale at point number 1. And suppose people who support a complete military victory are at the other end of the scale at point number 7. At what point would you place (yourself, Humphrey, Nixon)?

(3) Liberalism (1972-1980)

We hear a lot of talk these days about liberals

and conservatives. I'm going to show you a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

(4) Jobs (1972-1980)^a

Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of this scale—at point number 1. Others think that the government should just let each person get ahead on his own. Suppose that these people are at the other end—at point number 7. And, of course, some people have opinions in between. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about this?

(5) Minority Rights^a (1972-1980)

Some people feel that the government in Washington should make every possible effort to improve the social and economic position of blacks and other minority groups. Others feel that the government should not make any special effort to help minorities because they should help themselves. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

(6) Women (1972-1980)

Recently there's been a lot of talk about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government. Others feel that a woman's place is in the home. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

^aThere were slight changes in wording from 1972 to 1980, but only in the way in which the respondent was informed about the ends of the scale. In all cases, the options were the same, as was the substance of each question.

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Voter Participation and Strategic Uncertainty

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The paradox of not voting is examined in a model where voters have uncertainty about the preferences and costs of other voters. In game-theoretic models of voter participation under complete information, equilibrium outcomes can have substantial turnout even when voting costs are relatively high. In contrast, when uncertainty about preferences and costs is present, only voters with negligible or negative net voting costs participate when the electorate is large.

This article provides a unified treatment of two areas of longstanding theoretical attention in political science. The first is the paradox of not voting as addressed by Downs (1957), Tullock (1967), and Riker and Ordeshook (1968). Why does anyone bother to vote, given that voting is presumably costly and that the probability that one's vote will affect the outcome is presumably small? The second area is the role played by uncertainty or the lack of information in affecting political processes.

Uncertainty and the Paradox of Not Voting

There are three types of uncertainty that are potentially germane to the paradox of not voting. The first type, uncertainty over alternative outcomes, has attracted substantial attention in the theory of competitive elections, beginning with Shepsle (1972). Just what would be the policies of a candidate if he or she were elected? In the strategic interaction among voters, this form of uncertainty is of no consequence. The randomness occurs at the end of the game tree and, in the context of a game-theoretic model, each citizen will simply replace the final lottery with its expected utility value. In contrast, the second and third types involve strategic uncertainty.

The second type results from voters not knowing whether other citizens will vote or abstain. Because there is no compulsion to reveal one's decision before the poll, the appropriate game-theoretic model is one in which all citizens make decisions simultaneously. In terms of a game tree, the presence of simultaneous play implies that players do not see the results of all prior or contemporaneous moves. Consequently, voter participation games are games of imperfect information.

The third type of uncertainty recognizes that any one voter is generally poorly informed about the voting costs and preferences of other voters, which leads to a game of incomplete information. In terms of classical game theory, this uncertainty can be modeled by having "nature" move at the beginning of the game, assigning costs and preferences to the voters. Each voter, however, is permitted to observe only his or her own costs and preferences. Thus, one's own costs and preferences remain private information. Once nature has moved, the game advances to a branch corresponding to a game of complete (but imperfect) information with the costs and preferences assigned by nature. Voters then move in this game but with an additional source of uncertainty; not only do they not know the moves of other citizens, they also do not know the characteristics (costs and preferences for political alternatives) of these citizens.

Model building in political science has generally focused on strategic uncertainty regarding moves while ignoring uncertainty regarding player characteristics. When this latter type of uncertainty is absent, the appropriate form of analysis is to ascertain Nash equilibria. Nash equilibrium analysis has been most prominent in the analysis of spatial competition by candidates. In simple cases, like the classic Hotelling-Black median voter theorem, pure strategy equilibria result, but in more general situations there is equilibrium

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only in mixed strategies (McKelvey & Ordeshook, 1976).¹

The Bayesian Nash Equilibrium Concept

When we add uncertainty about player characteristics, the strategies available to the players change in a fundamental way. Whereas in a voter participation game with complete information strategies are limited to vote or abstain, in the game of incomplete information, strategies take the form, "If my characteristics assigned by nature are so and so, vote; otherwise, abstain." Put somewhat differently, analysis takes place in a framework where each player has a decision rule that maps his own characteristic into a specific action (vote or abstain). The decision rule chosen by a voter will depend upon his or her beliefs about both the decision rules that other voters will use and the distribution of other voters' characteristics. In equilibrium, all the decision rules and beliefs must be consistent in the sense that 1) these beliefs are correct, and 2) given the rules adopted by the other players, no single player will wish to alter his own decision rule. The equilibrium characterized in this way in fact represents a Nash equilibrium to the expanded game where "nature" moves first and assigns player characteristics; this is commonly referred to as a Bayesian equilibrium. In addition to analyzing Bayesian equilibrium in general voter participation games, we also present a simple example that illustrates geometrically the relationship between Bayesian and Nash equilibria in these games.

We believe the Bayesian equilibrium approach has many potential applications in political science beyond this investigation of the paradox of not voting. For example, there is also substantial uncertainty in legislative and bureaucratic settings. In turn, this uncertainty may, as the analysis below indicates, strongly affect our theoretical conclusions about these political processes.

Alternative Models of Voter Participation Games

Our analysis considers voter participation games in the context of two alternative plurality elections, as found in the general literature on the paradox of not voting. Following the literature, we use the simplification of a utility function that

is additive between instrumental benefits and the fixed net cost of voting. This simplification leads to the well-known equation:

$$R = pB - C + D, \quad (1)$$

where R is expected net benefits of voting, p is the probability of casting a decisive vote, B is the difference in benefits engendered by the voter's more preferred alternative winning over his less preferred alternative, C is a fixed voting cost (such as the time spent in going to and from the polls), and D is a fixed benefit which derives from the act of voting, such as a sense of citizen duty or the avoidance of sanctions imposed for not voting. We combine the last two terms and refer to $c = C - D$ as the fixed net cost of voting. Additivity conveniently implies that all the information each voter requires about other voters can be summarized by two pieces of information. First, it is necessary to have prior information of how many other voters prefer each of the two alternatives. Second, prior information is needed only about the ratio of costs to benefits of each other voter and not about the joint distribution of both costs and benefits. That is, without loss of generality, we can, given additivity, normalize equation (1) such that $B = 1$ for all voters.

It has been argued² that with large numbers of voters, p is presumably very close to 0, so if D is less than C , rational voters will abstain since $R = p - c < 0$. Of course, if this is true for everyone and if everyone follows this logic, then $p = 1$. One proposed solution to this "paradox of not voting" (Downs, 1957) is that substantial turnout occurs despite $p \cong 0$, because citizens are imbued with a sense of citizen duty so that $D > C$. However, many theorists considered the paradox unresolved because they were unwilling to accept that voting generally led to net benefits rather than net costs, even though mass voting is widespread. In fact, substantial turnout is found not only in some national elections, like U.S. presidential elections, which are not strictly single issue, two-alternative ballots, but also in other contexts, such as the French presidential runoff, where the institutional setup implicit in equation (1) is in fact present.

An initial attempt to resolve the paradox was made by Ferejohn and Fiorina (1974), who simply asserted that voters were not expected utility maximizers but regret minimaxers. Their approach met considerable resistance.³ Moreover,

¹These games may be contrasted with chess and other games of perfect information which have sequential moves and before any move all previous moves are known to all players. In such games, there is no strategic uncertainty, and nontrivial mixed strategies never constitute equilibria.

²See, for example, Meehl (1977) and Chamberlain and Rothschild (1981).

³See Beck (1975), Mayer and Good (1975), Stephens (1975), Tullock (1975), and Thompson (1982), for discussions of the application of the minimax regret criterion to the voting decision.

the minimax regret hypothesis also seems inconsistent with empirical observations. For example, a regret minimaxer may vote regardless of whether the outcome is expected to be a tie or a 4-to-1 victory; the probability of casting a decisive vote is irrelevant under this decision criterion. Yet there is persistent observation of higher voting rates in closely contested elections.⁴

Rather than abandoning expected utility, more recent research has questioned the presumption that p is virtually zero. After all, equation (1) represents a purely decision-theoretic model if p is exogenously given. If we investigate how p is endogenously determined by the interaction of voters, we will perhaps find that p is indeed far from zero. With p far from zero, substantial turnout can result.

In earlier work, we indeed arrived at the conclusion that substantial turnout could occur (Palfrey & Rosenthal, 1983, 1984). There we assumed that every voter had complete information about the preferences and voting costs of every other voter. Then, for fixed alternatives (e.g., candidate positions), we modeled the voters as playing a non-cooperative game in which each voter has, essentially, two strategies: vote for the more preferred alternative or abstain. Surprisingly, we found that some of the Nash equilibria to this game had substantial turnout. In equilibrium, the presumably small p in equation (1) could in fact be 1 or very close to 1, independent of electorate size.

Although the full information model might have much to say about participation in committees or small groups, the full information assumption is clearly unrealistic for large electorates. In a pair of articles, Ledyard (1981, 1984) explored a more general model in which voters were uncertain about the voting costs and preferences of other voters. He showed that equilibria exist with positive turnout (as long as the alternatives are distinct), and that the condition of everyone abstaining is not an equilibrium. Consequently, some voting is to be expected. Beyond this, Ledyard is unable to obtain conclusive results about how much voting takes place in a voter equilibrium in large electorates.

In this article we demonstrate, within the framework of Ledyard's model, that in very large electorates the only voters are citizens with net positive benefits from the act of voting, citizens whose sense of duty outweighs any cost in voting. We have come full circle and are once again beset by the paradox of not voting.

It is important to emphasize, though, that the paradox requires both large electorates and sub-

stantial uncertainty. We can in fact have equilibrium with high turnout in large but extremely well-informed electorates or in small electorates subject to strategic uncertainty, even when voting costs are relatively large.

In the next section of the article we review the full information model. In our earlier development of this model (Palfrey & Rosenthal, 1983), we assumed all voters had identical costs. Finding high turnout in informed electorates is not dependent on this assumption. One can readily illustrate high turnout equilibria when costs are variable but known to all citizens. After the exposition of the full information model, we move half-way to the Ledyard model in the third section. Although we allow uncertainty regarding relative costs, we maintain full information regarding the number of voters preferring each of the two alternatives. We show that, in large electorates, even this partial degree of uncertainty is sufficient to cause voters with positive net voting costs to abstain in equilibrium. In the fourth section we add uncertainty regarding preferences and show that this does not change our basic results.

Voter Participation Games with Complete Information

There are N voters and two alternatives to vote for, alternative 1 and alternative 2. An election is held, and each voter has three choices: vote for 1; vote for 2; abstain.

If one alternative has more votes than the other, the alternative with the greater number wins. In the event of a tie, two rules are used in practice. First, one alternative is designated before the election as the winner in the event of a tie. Second, a fair coin is tossed to decide the winner. Our three main theorems hold for both rules and for a wide class of other tie-breaking procedures; for expositional clarity, we develop the theorems assuming the first, or status quo, rule. Although the first rule is simplest for general theorems, the second or coin-toss rule leads to a simple but important example. Thus, we henceforth use the first rule except when discussing this example.

Each voter is in one of two groups, T_1 or T_2 . There are N_1 voters in T_1 and N_2 in T_2 , with $N_1 + N_2 = N$. Voters in group T_1 prefer alternative 1, and voters in group T_2 prefer alternative 2. The cost of voting is denoted c_i for voter i . The payoff to a voter depends upon his cost, his preferences, which alternative wins, and whether or not he casts a vote. Denote the number of votes cast for alternative 1 by n_1 and the number of votes cast for alternative 2 by n_2 . Recalling that the benefits of winning and losing have been normalized to be 1 and 0, respectively, the possible

⁴See, for example, Rosenthal and Sen (1973) and Foster (1984).

Table 1. Payoff Matrix for Voters in T_1

	Vote	Abstain
$n_1 > n_2$	$1 - c_i$	1
$n_1 < n_2$	$-c_i$	0

payoffs to a voter in T_1 and T_2 are given in Tables 1 and 2.

The voting game is therefore parametrized by the number of voters in each group and the voters' (possibly different) voting costs. Because pure strategies fail to exist except in a few very special cases, we look to mixed-strategy equilibria where a mixed voting strategy is a probability of voting, which we denote by q_i for voter i . A well-known result from game theory guarantees existence of at least one mixed strategy equilibrium to our voter participation game. We define an equilibrium more precisely below:

(q_1^*, \dots, q_N^*) is a *Mixed Strategy (Nash) Equilibrium (MSE)* if for all i :

(A) $q_i^* > 0$ only if voting yields at least as high a payoff as nonvoting given $(q_1^*, \dots, q_{i-1}^*, q_{i+1}^*, \dots, q_N^*)$.

(B) $q_i^* < 1$ only if nonvoting yields at least as high a payoff as voting, given $(q_1^*, \dots, q_{i-1}^*, q_{i+1}^*, \dots, q_N^*)$.

(C) $q_i^* \in [0, 1]$.

One can immediately deduce from A and B that $0 < q_i^* < 1$ only if voter i is indifferent between voting and not voting, given everyone else's probabilities of voting. Thus we can give conditions that characterize mixed strategy equilibria. We denote n_1^i and n_2^i as the number of votes received in equilibrium by alternatives 1 and 2 if i does not participate. For voters in T_1 :

$0 < q_i^* < 1$ only if $\text{Prob}(n_1^i > n_2^i)$

$$= \text{Prob}(n_1^i > n_2^i - 1) - c_i$$

$$\text{or } c_i = \text{Prob}(n_1^i = n_2^i - 1) \quad (2)$$

$0 = q_i^*$ only if $c_i > \text{Prob}(n_1^i = n_2^i - 1)$ (3)

$1 = q_i^*$ only if $c_i < \text{Prob}(n_1^i = n_2^i - 1)$ (4)

Analogous conditions hold for voters in T_2 .

In Palfrey and Rosenthal (1983), equilibrium solutions are analyzed for the special case in which all voters have the same cost of voting, that is, $c_1 = \dots = c_N = c$. The numerous solutions can be divided into two categories; the first category, where all voters' probabilities of voting are strictly between 0 and 1 is called a *totally mixed strategy equilibrium* (TMSE); that is, no one is definitely voting or abstaining. Most of the analyzed solutions of this kind have the property that q_i^* approaches 0 for all voters as the number of voters becomes large. The other type of equilibrium has all voters in one group voting with some probability $q \in (0, 1)$, and the voters in the other group are divided into two subgroups, one in which voters definitely abstain and one in which voters definitely vote. This second kind of equilibrium does not have the property that voter turnout necessarily becomes very light as N becomes large. Rather, expected turnout may be as great as twice the size of the minority group (i.e., $2\min\{N_1, N_2\}$). Furthermore, even very small minorities can have a substantial chance of winning the election.

Although this result is very interesting because it suggests that substantial voting can occur when expected utility maximizing voters have relatively high voting costs and there is no citizen duty, the type of equilibrium that supports high turnout seems very fragile. In particular one side must be split into subgroups of voters and nonvoters in a precise way so that there is no uncertainty about how many votes one of the two alternatives will receive, because if there is too much uncertainty in equilibrium, then the probability that the election is close (i.e., either a tie or one vote away from a tie) approaches 0 as N gets large.

We also constructed one example of a totally mixed strategy equilibrium to a voting game where there was substantial voting even in arbitrarily large electorates. Because this example will be useful later on to help interpret equilibrium solutions with incomplete information, it is worthwhile to work through it here.

Example. Let all voters have the same cost $c \in (0, 1/2)$. Let $N_1 = N_2$. In order to make the problem completely symmetric, the tie-breaking rule

Table 2. Payoff Matrix for Voters in T_2

	Vote	Abstain
$n_1 > n_2$	$-c_i$	0
$n_1 < n_2$	$1 - c_i$	1

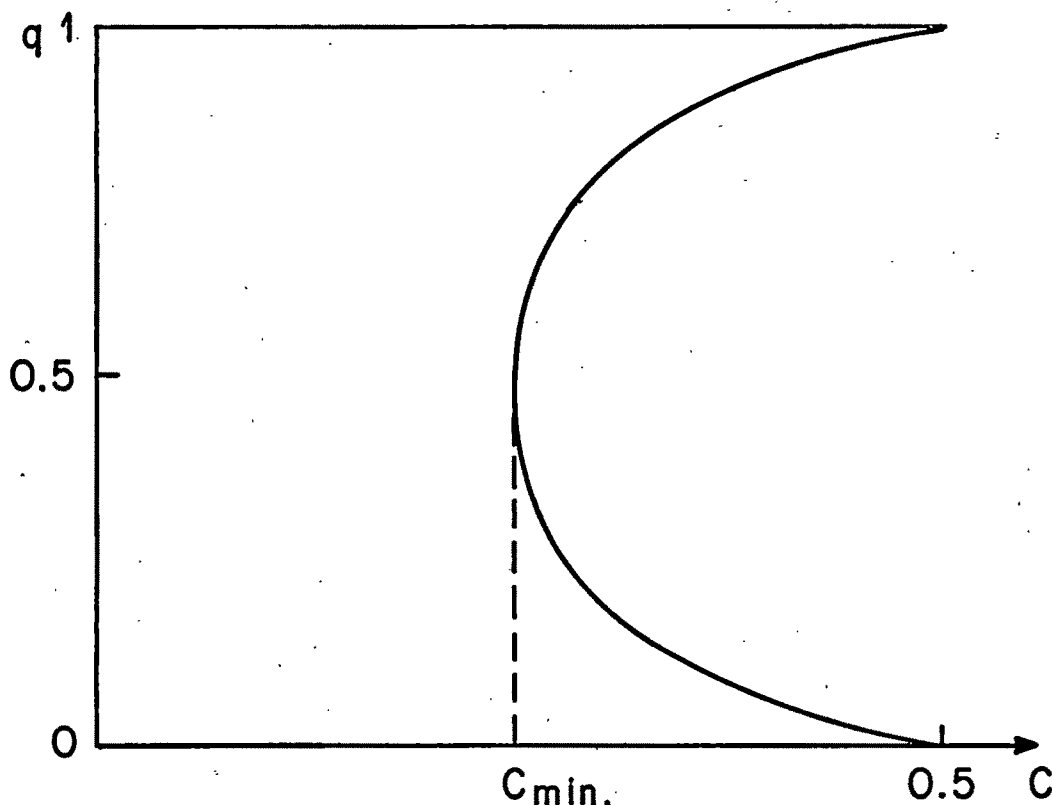


Figure 1. Totally Mixed Strategy Equilibrium Solutions as a Function of c with the Coin Flip Rule if $N_1 = N_2$

will be to toss a fair coin. Palfrey and Rosenthal (1983) show that any symmetric totally mixed strategy equilibria satisfy the following equation, which corresponds to equation (2), except for the difference resulting from the coin-toss tie-breaking procedure:

$$2c = \sum_{k=0}^{N_1-1} \binom{N_1-1}{k} \binom{N_1}{k} q^{2k} (1-q)^{2N_1-2k-1} + \sum_{k=0}^{N_1-1} \binom{N_1-1}{k} \binom{N_1}{k+1} q^{2k+1} (1-q)^{2N_1-2k-2}.$$

If c is between 0 and $1/2$, this equation has either 0, 1, or 2 solutions between 0 and 1. Figure 1 below illustrates the equilibrium solutions for each value of c between 0 and $1/2$, for a fixed value of N_1 .

As one can see from the figure, there exists no equilibrium of this type for $0 < c < c_{\min}$, exactly one equilibrium for $c = c_{\min}$ and exactly two

equilibria for $c_{\min} < c < 1/2$. As N_1 gets large, this figure changes in a systematic and interesting way. The lower bound, c_{\min} , converges to 0, and the "upper" and "lower" equilibria for $c > c_{\min}$ converge to 1 and 0, respectively. Therefore, in large electorates of this special sort, there will be exactly two symmetric totally mixed strategy equilibria for any cost between 0 and $1/2$: one with almost everyone voting and one with almost nobody voting.

This example of high turnout in large electorates has the unappealing feature that there is a second equilibrium with almost no one voting. Apparently the only reason the upper one can be sustained is that the two electorates are the same size, so that for q close to 1, the probability of a tied election is very high. Again, the result rests on the fact that in equilibrium, with a large electorate, there is essentially no strategic uncertainty. The next section examines the effects on equilibrium turnout of introducing an additional source of strategic uncertainty. The results presented below establish that the presence of strategic

uncertainty regarding costs and preferences eliminates all equilibria with high turnout in large electorates.

Voter Participation Games with Incomplete Information

Existence of Bayesian Equilibrium

Large turnout equilibria appear to be very fragile and seem to depend on there being very little uncertainty about the outcome in equilibrium. What these fragile equilibria depend on is that voters know with certainty what some subset of the voters are going to do (in particular the voters in the group who are playing pure strategies of voting and abstaining). It is this strategic certainty which permits p to be substantial in equilibrium even in very large electorates. In contrast to a game of complete information, a game with private information induces uncertainty about the behavior of all voters. In this section we consider a very simple kind of private information, namely that each voter knows his own voting cost (or ratio of cost to benefit), but has only probabilistic information about other voters' costs.³

In order to economize on notation, we will assume for voters in T_1 that each of their voting costs are independently and identically distributed according to a distribution function $F_1(c)$; for voters in T_2 , voting costs are independently and identically distributed according to a (possibly different) distribution function $F_2(c)$. In other words, the probability that any randomly selected voter in T_1 has a cost less than or equal to \hat{c} is $F_1(\hat{c})$ and the probability that any randomly selected voter in T_2 has a voting cost less than or equal to \hat{c} is $F_2(\hat{c})$. Each voter, however, knows her or his own actual voting cost, c_i , but has only the common probabilistic knowledge about others' voting costs. The other two parameters of the model, N_1 , N_2 , are commonly known by all voters.

The strategies in this type of game, a game of incomplete information, are defined differently from strategies in a game of complete information, because each voter i is aware that another voter's decision (vote or abstain) will depend on that other voter's actual voting cost, which voter i does not know. Therefore, each voter is presumed to have a decision rule, which specifies whether to vote or to abstain as a function of c_i . This condi-

tion results in two ranges of costs, one where i votes for his preferred candidate and another where i abstains. As will be apparent shortly, there is no loss in generality here in restricting attention to decision rules of the following form. Each voter i has a critical cost level, $\hat{c}_i \in [0, 1]$, and the decision rule of voter i is then to vote if $c_i < \hat{c}_i$ and abstain if $c_i > \hat{c}_i$.

The following additional notation is also useful. Let:

- $\hat{c} = (\hat{c}_1, \dots, \hat{c}_n)$
- $\hat{c}_{-i} = (\hat{c}_1, \dots, \hat{c}_{i-1}, \hat{c}_{i+1}, \dots, \hat{c}_n)$
- $n_1 =$ the number of voters in T_1 for whom $c_i < \hat{c}_i$
- $n_2 =$ the number of voters in T_2 for whom $c_i < \hat{c}_i$
- $n_1^i =$ the number of voters (excluding i) in T_1 for whom $c_j < \hat{c}_j$
- $n_2^i =$ the number of voters (excluding i) in T_2 for whom $c_j < \hat{c}_j$
- $P_1^i(\hat{c}_{-i}) = \text{Prob}(n_1^i = n_1^i - 1 | \hat{c}_{-i})$
- $P_2^i(\hat{c}_{-i}) = \text{Prob}(n_2^i = n_2^i - 1 | \hat{c}_{-i})$

Notice that P_1^i and P_2^i correspond to p in equation (1). A Bayesian equilibrium in our model is a set of voter decision rules (critical cost levels), $c^* = (c_1^*, \dots, c_n^*)$ such that

$$c_i^* = P_1^i(c_{-i}^*) \quad \text{for } i \in T_1 \quad (5)$$

$$c_i^* = P_2^i(c_{-i}^*) \quad \text{for } i \in T_2 \quad (6)$$

It can be easily shown that if other voters are using c_{-i}^* , and $i \in T_2$, then voting gives i a higher expected payoff than abstention if and only if $c_i < c_i^*$. To see this, observe that the expected payoff of voting equals $(1 - c_i)$ times the probability that $n_1^i \leq n_1^i$, minus c_i times the probability that $n_1^i > n_1^i$. Hence, for $i \in T_1$ the expected payoff of voting exceeds the expected payoff from not voting if and only if $c_i < c_i^*$. The rationale for equation (6) is virtually identical. That c_i^* must lie between 0 and 1 is obvious, because voters have a dominant strategy to vote if $c_i < 0$ and have a dominant strategy to abstain if $c_i > 1$.

Because all voters in a group face a similar decision problem, it is natural to assume that voters in the same group use the same decision rule in equilibrium. We restrict the analysis to this kind of equilibrium, which we refer to as a *symmetric equilibrium*. This approach simplifies the problem considerably, because a symmetric equilibrium is simply a pair of critical points (c_1^*, c_2^*) such that

³This is one of the more common ways of introducing private information to transform a game of complete information into a game of incomplete information. Everyone knows his own payoff function with certainty, but does not know the other players' payoffs. See, for example, Harsanyi (1967-1968, 1973) and Vickrey (1961).

any voter i in T_1 votes if and only if $c_i < c_1^*$, and any voter i in T_2 votes if and only if $c_i < c_2^*$.

Given any pair of critical points, (c_1, c_2) , we can explicitly write out the formulas for P_1^i and P_2^i , and these are given below. First, let $W_k[N, q] = \binom{N}{k} q^k (1-q)^{N-k}$ represent a binomial probability.

$$P_1^i(c_1, c_2; N_1, N_2, F_1, F_2)$$

$$= \sum_{k=0}^{\min[N_1-1, N_2-1]} W_k[N_1-1, F_1(c_1)] \cdot W_{k+1}[N_2, F_2(c_2)]$$

(7)

$$P_2^i(c_1, c_2; N_1, N_2, F_1, F_2)$$

$$= \sum_{k=0}^{\min[N_1, N_2-1]} W_k[N_1, F_1(c_1)] \cdot W_k[N_2-1, F_2(c_2)]$$

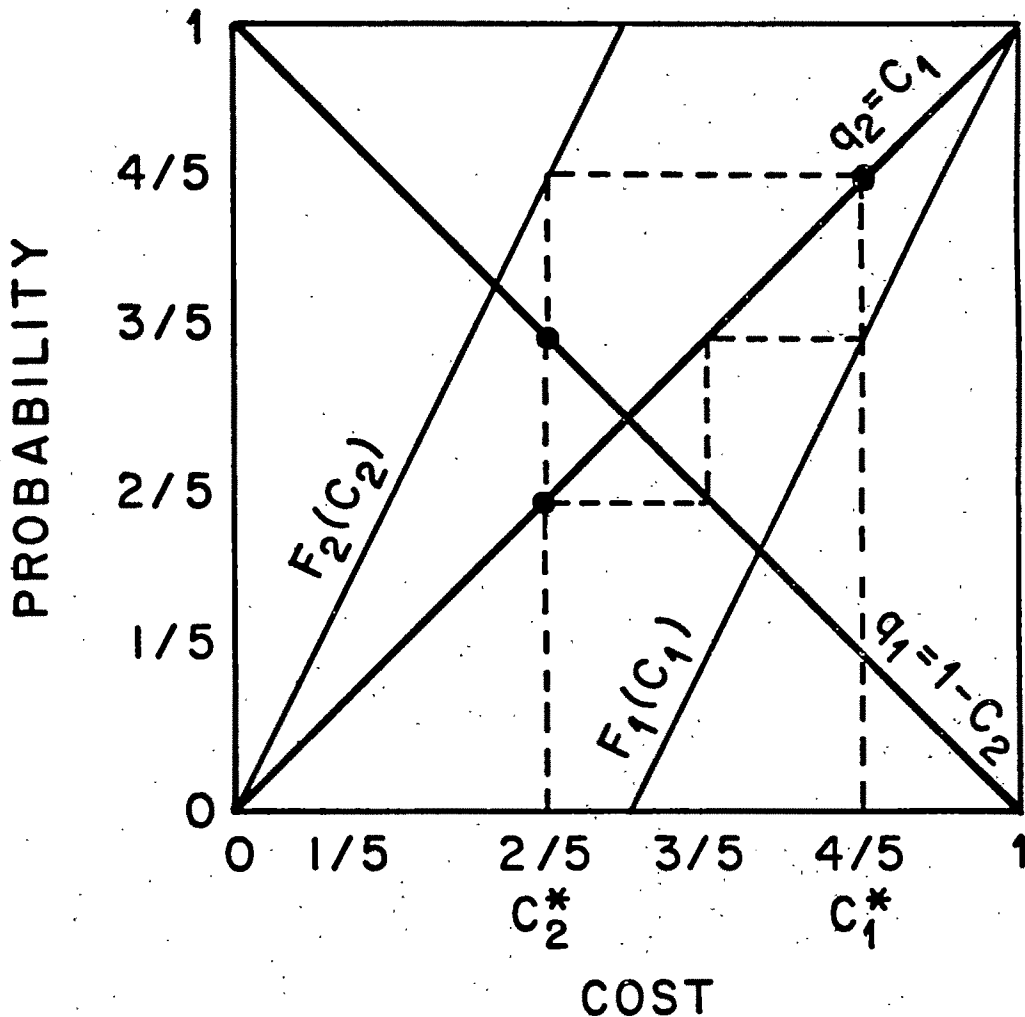
(8)

From equations (5)-(8), the two equilibrium conditions for the critical points are: $c_1^* = P_1^i(c_1^*, c_2^*)$ and $c_2^* = P_2^i(c_1^*, c_2^*)$, or

$$c_1^* = \sum_{k=0}^{\min[N_1-1, N_2-1]} W_k[N_1-1, F_1(c_1^*)] \cdot W_{k+1}[N_2, F_2(c_2^*)]$$

(9)

Figure 2. Correspondence between Mixed Strategy and Bayesian Equilibrium When $N_1 = N_2 = 1$



$$c_2^* = \frac{\min[N_1, N_2 - 1]}{\sum_{k=0}^{N_1-1} W_k[N_1, F_1(c_1^*)]} W_k[N_2 - 1, F_2(c_2^*)] \quad (10)$$

A symmetric Bayesian equilibrium is represented by any pair of critical costs (c_1^* , c_2^*) that simultaneously solves equations (9) and (10).

In order to illustrate a Bayesian equilibrium and contrast it with an ordinary Nash equilibrium, we consider a simple example where there is only one voter on each team. That is, let $N_1 = N_2 = 1$. The status quo rule breaks ties. In this case, the payoff matrix is given in Table 3.

The mixed strategy Nash equilibrium to this game when c_1 and c_2 are known to both players is calculated readily as: $q_1^* = 1 - c_2$, $q_2^* = c_1$. The solution appears as the heavy cross in Figure 2. So, for example, when $c_1 = 4/5$ and $c_2 = 2/5$, $q_1^* = 3/5$ and $q_2^* = 4/5$.

Now assume that the values of c_1 and c_2 are private information to the respective players. Think of each player as having a cutoff cost level, such that if his own cost is below this level he votes, and otherwise he abstains. In other words, these cutoff levels, designated c_1^* , c_2^* represent the decision rules of the two players.

Recalling equation (1), each voter will want to choose his cutoff level, c_i^* such that $c_i^* = p_i$. For costs above c_i^* , an individual will want to abstain, because the expected reward (R) is negative. For costs below c_i^* , the individual will want to vote. We have: $c_1^* = \text{Prob}\{\text{Voter 1 is decisive given voter 2 uses } c_2^*\}$. Because voter 1 is decisive only in the case where voter 2 votes and hence voter 1 can create a tie and "win" by voting, this expression becomes:

$$\begin{aligned} c_1^* &= \text{Prob}\{\text{voter 2 votes, given voter 2 uses } c_2^*\} \\ &= \text{Prob}(c_2 < c_2^*) = F_2(c_2^*) \end{aligned}$$

where $F_2(c_2)$ represents the cumulative probability distribution of voter 2's cost. Similarly, for voter 2, we obtain:

$$\begin{aligned} c_2^* &= \text{Prob}(\text{voter 2 is decisive, given voter 1 uses } c_1^*) \\ &= \text{Prob}(\text{voter 1 abstains, given voter 1 uses } c_1^*) \\ &= \text{Prob}(c_1 > c_1^*) = 1 - F_1(c_1^*). \end{aligned}$$

Summarizing, we have two equations in two unknowns:

$$\begin{aligned} c_1^* &= F_2(c_2^*) \\ c_2^* &= 1 - F_1(c_1^*) \end{aligned}$$

At equilibrium, the cutoff levels must be consistent; that is, they must solve the above two simultaneous equations.

In this example, let

$$\begin{aligned} c_1 &\sim U[1/2, 1] \\ c_2 &\sim U[0, 1/2] \end{aligned}$$

That is, both costs have a uniform distribution, but player 1 is the high cost individual. The distribution functions are:

$$\begin{aligned} F_1(c_1) &= 0 & \text{if } c_1 < 1/2 \\ &= 2(c_1 - 1/2) & \text{if } 1/2 \leq c_1 < 1 \\ &= 1 & \text{if } 1 \leq c_1 \\ F_2(c_2) &= 0 & \text{if } c_2 < 0 \\ &= 2c_2 & \text{if } 0 \leq c_2 \leq 1/2 \\ &= 1 & \text{if } 1/2 \leq c_2. \end{aligned}$$

From the equilibrium condition, we obtain:

$$\begin{aligned} c_1^* &= F_2(c_2^*) \\ &= \begin{cases} 2c_2^* & c_2^* < 1/2 \\ 1 & c_2^* \geq 1/2 \end{cases} \\ c_2^* &= 1 - F_1(c_1^*) \\ &= \begin{cases} 1 & c_1^* < 1/2 \\ 1 - 2(c_1^* - 1/2) & c_1^* \geq 1/2 \end{cases} \end{aligned}$$

Table 3. Payoffs When $N_1 = N_2 = 1$

Type 1	Type 2		
	Vote	Abstain	
Vote	$1 - c_1$	$-c_2$	0
Abstain	0	$1 - c_2$	0

As shown in the figure, these equations are solved by $c_1^* = 4/5$, $c_2^* = 2/5$. Finally, note that $F_1(c_1^*) = 3/5$ and $F_2(c_2^*) = 4/5$, so that were these equilibrium cutoff levels the actual costs in the game of complete information, the $F(\cdot)$ values correspond exactly to the mixed strategy probabilities given for these costs.

Another interesting feature of this example is that it illustrates that the equilibrium of the game

of incomplete information does not correspond to the equilibrium of the "certainty equivalent" game of complete information. This certainty equivalent game with $c_1 = 3/4$ and $c_2 = 1/4$ has equilibrium voting probabilities of $q_1^* = 3/4$, $q_2^* = 3/4$, in contrast to the solution to the incomplete information game which has equilibrium voting probabilities of $F_1(c_1^*) = 3/5$, $F_2(c_2^*) = 4/5$. Conversely, one cannot solve for c_1^* , c_2^* by simply plugging in expected values of the distribution of c_1 and c_2 . Such a procedure would result in the misleading conclusion of $c_1^* = 3/4$, $c_2^* = 1/4$.

Before proceeding, we make two assumptions about the distribution functions, F_1 , F_2 .

ASSUMPTION 1. F_1 , F_2 are continuous on $(-\infty, \infty)$.

ASSUMPTION 2.

- (i) $F_1(0) > 0$, $F_2(0) > 0$
- (ii) $F_1(1) < 1$, $F_2(1) < 1$

The first assumption rules out mass points in the distribution. The second assumption states that for each group, there is some probability that a voter from that group has negative voting costs (e.g., from citizen duty), and some probability that a voter from that group has sufficiently high costs so that his dominant strategy is to abstain. Assumptions 1 and 2(i) are used only to simplify the presentation. We later discuss relaxing these assumptions. Assumption 2(ii) is not a very strong restriction because $F_1(1)$ and $F_2(1)$ can be arbitrarily close to one. We now state and prove existence of a symmetric equilibrium.

THEOREM 1 (Ledyard, 1981). There exists a symmetric equilibrium (c_A^*, c_B^*) , with $1 \geq c_A^* > 0$, $1 \geq c_B^* > 0$.

Proof. See Appendix A.

Parallels between Equilibria in the Complete and Incomplete Information Games

It is useful to notice some strong parallels between the symmetric equilibrium in the game of incomplete information and totally mixed strategy equilibrium in the game of complete information. Some of these parallels are listed below:

- 1) If (c_1^*, c_2^*) is a symmetric equilibrium to the game (N_1, N_2, F_1, F_2) , then the complete informa-

tion game in which N_1 players in T_1 have cost c_1^* and the N_2 players in T_2 have cost c_2^* has a TMSE with $q_1^* = F_1(c_1^*)$ and $q_2^* = F_2(c_2^*)$. This is readily verified by observing that equation (2) and the corresponding equation for T_2 can be rewritten:

$$c_1 = \sum_{k=0}^{\min[N_1-1, N_2]} w_k[N_1-1, q_1] \cdot w_{k+1}[N_2, q_2] \quad \text{for } i \in T_1 \quad (11)$$

$$c_1 = \sum_{k=0}^{\min[N_1, N_2-1]} w_k[N_1, q_1] \cdot w_{k+1}[N_2-1, q_2] \quad \text{for } i \in T_2. \quad (12)$$

These are also the equilibrium conditions for a TMSE (see Palfrey & Rosenthal, 1983). Substituting c_1^* for c_1 , $i \in T_1$ and c_2^* for c_1 , $i \in T_2$, $q_1 = F_1(c_1^*)$, $q_2 = F_2(c_2^*)$, equations (11) and (12) coincide with (9) and (10).

2) By a similar argument, given N_1, N_2 if q_1^*, q_2^* is a TMSE for a complete information game in which voting costs are c_1 for all voters in T_1 and c_2 for all voters in T_2 , then any pair of distribution functions, (F_1, F_2) with the property that $F_1(c_1) = q_1^*$ and $F_2(c_2) = q_2^*$ will support an equilibrium at (c_1, c_2) in the corresponding game of incomplete information.

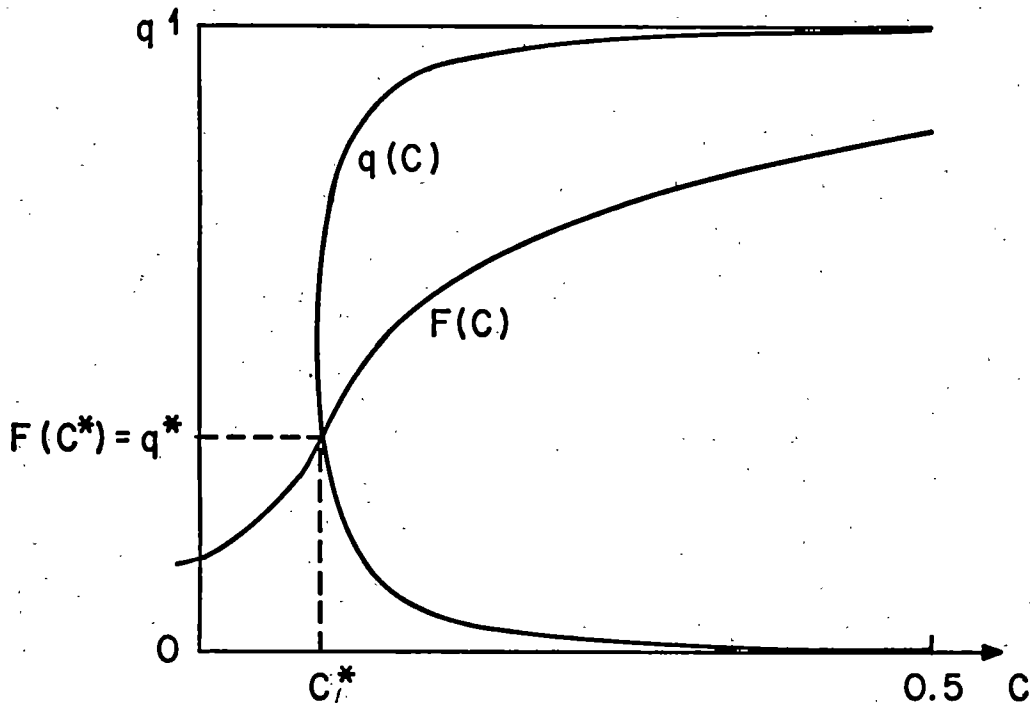
3) The fragile equilibria in the game of complete information require the use of pure strategies by some voters. These pure strategies correspond to decision rules in the game of incomplete information which are completely unresponsive to costs. That such decision rules are never rational follows from Assumption 2, because a voter whose cost is less than zero should vote but should abstain if the cost is greater than one.⁷

The first two parallels described above can be illustrated by a graphical analysis of the coin-toss example above. Consider the incomplete information game analogous to the complete information game in which $N_1 = N_2$ and all voters have the same cost. In this corresponding game, we let the distribution of voting costs be the same for members of both groups. That is, $F_1 = F_2$. Because $N_1 = N_2$, it follows that in equilibrium $c_1^* = c_2^* = c$ and $F_1(c_1^*) = F_2(c_2^*)$. That is, all voters use the same critical cost level to decide whether or not to vote, which corresponds to the equilibrium out-

⁷Under the coin-toss rule, abstention dominates for $c_i > 1/2$.

⁷Note that the existence of "fragile" equilibria in the game of complete information is not dependent on the assumption of identical costs. For example, if some voters have low cost $c_l \in (0, 1)$ and others have high cost $c_h > 1$, the c_l voters play the same game as they would play were there no c_h voters.

Figure 3. Relationship between Nash Equilibrium and Bayesian Equilibrium



come of one particular game of complete information, where all voters have cost c^* and vote with probability $q^* = F(c^*)$. This is illustrated graphically by superimposing the distribution function of voting costs onto the complete information game equilibrium correspondence of Figure 1. (See Figure 3.) The intersection between $F(c)$ and $q^*(c)$ gives c^* , the symmetric Bayesian equilibrium of the game of incomplete information.

Bayesian Equilibrium in Large Electorates

Downs's longstanding conjecture that in a large electorate voters with positive voting costs will abstain is considered in this section. Before explicitly formalizing this hypothesis about the effect of electorate size, the intuition of how the Bayesian equilibrium changes as N_1 and N_2 change is illustrated using the same example in which $N_1 = N_2$ and $F_1(\cdot) = F_2(\cdot)$.

If N_1 and N_2 are increased simultaneously, then it can be shown that the $q^*(c)$ curve tends toward the q -axis in a particular way, as illustrated in Figure 4. One can see from the graph that as N increases, the point of intersection of the distribution function and the $q^*(c)$ curve, which deter-

mines the Bayesian equilibrium, c^* , converges to $(0, F(0))$. This is consistent with the Downs-Riker-Ordeshook-Tullock view that the only reason a rational voter would vote in a large election is if his net voting cost is negative.

We now prove formally that the limiting property illustrated above in the example game is true in general. First we need to introduce the following notation. Fix F_1, F_2 . Consider the sequence of values of $N_1 = 1, 2, 3, \dots$ and any sequence of positive integer values of $N_2 = N_1^1, N_1^2, \dots$. For any j let Γ_j be the game with $F_1, F_2, N_1 = j, N_2 = N_1^j$. Let $\{(c_1^j, c_2^j)\}_{j=1}^\infty$ denote any sequence of equilibria to $\{\Gamma_j\}_{j=1}^\infty$.

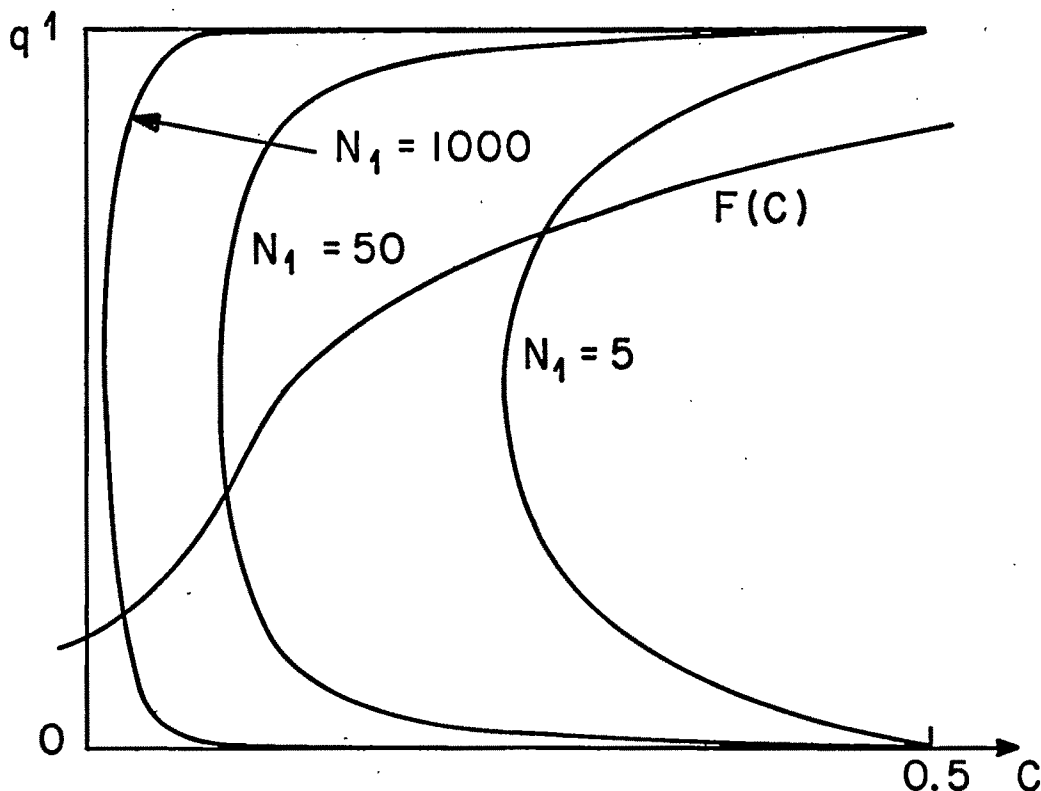
THEOREM 2. $\lim_{j \rightarrow \infty} c_1^j = 0$.

Proof. See Appendix A.

Notice that these limiting results do not depend on the sequence $\{N_1^j\}_{j=1}^\infty$. A similar result for c_2^j can be obtained if we let $\{N_1^j\}_{j=1}^\infty$ be arbitrary and let $N_2^j = j, j = 1, 2, \dots$. Hence, for either group the cut point approaches 0 as that group gets large, regardless of the size of the other group.

Assumptions 1 and 2 were made in order to simplify the proof. However, this convergence result can be proved more generally. Because such a proof is straightforward but overly tedious, the

Figure 4. Equilibrium as a Function of Electorate Size



role of these two technical assumptions is simply illustrated in Appendix B using the coin-toss example.

Therefore in very large electorates where N_1 and N_2 are both large, both critical cost levels are approximately zero. In other words, voters will not vote in large elections if the net cost is positive. Thus, at least according to this strictly game theoretic framework, outcomes are determined by the relative sizes of the negative cost subgroups within T_1 and T_2 .

Preference Uncertainty

The kind of preference uncertainty we deal with in this section is uncertainty about N_1 . We assume that the total number of voters, N , is known, but the number of voters supporting alternative 1 is not known. Each voter knows which alternative he supports, but knows only the probability distribution over the proportion of the other $N-1$ voters who favor alternative 1. We denote the probability that j of those $N-1$ voters prefer alternative 1 by $P_N(j)$, where $P_N(j) \geq 0$ and

$$\sum_{j=0}^{N-1} P_N(j) = 1.$$

We retain the rest of the structure developed in earlier sections of this article. Again, an equilibrium to the game with N voters is a pair of critical cost levels, c_{1N}^* , c_{2N}^* such that the expected payoff from voting for a group 1 (group 2) voter with cost c_1^* (c_2^*) equals the expected payoff from abstention. A symmetric equilibrium is characterized by the following two equations:

$$c_{1N}^* = \sum_{j=0}^{N-1} \sum_{k=0}^{\min(j, N-j-2)} w_k [j F_1(c_{1N}^*)] \cdot w_{k+1} [N-j-1, F_2(c_{2N}^*)] \cdot P_N(j) \quad (13)$$

$$c_{2N}^* = \sum_{j=0}^{N-1} \sum_{k=0}^{\min(j, N-j-1)} w_k [N-j-1, F_1(c_{1N}^*)] \cdot w_k [j, F_2(c_{2N}^*)] \cdot P_N(N-j-1) \quad (14)$$

In order to take limits on (c_{1N}^*, c_{2N}^*) , we must specify how $P_N(j)$ changes as N gets large. Because we are interested in *large* electorates, we make the following assumption about the limiting properties of the sequence $\{P_N(\cdot)\}_{N=2}^\infty$:

ASSUMPTION 3. For all T , $\lim_{N \rightarrow \infty} \sum_{j=T}^{N-T} P_N(j) = 1$.

This assumption guarantees that in the limit, N_1 and N_2 will be infinite with probability 1, which is precisely the type of sequence we wish to consider. Given some F_1 , F_2 and an infinite sequence $\{P_N(\cdot)\}_{N=1}^\infty$, let $\{(c_{1N}^*, c_{2N}^*)\}_{N=2}^\infty$ be any sequence satisfying (13) and (14) for all N .

THEOREM 3. If F_1 , F_2 and $\{P_N(\cdot)\}_{N=1}^\infty$ satisfy Assumptions 1-3, then

$$\lim_{N \rightarrow \infty} c_{1N}^* = 0$$

$$\lim_{N \rightarrow \infty} c_{2N}^* = 0$$

Proof. See Appendix A.

Conclusions and Normative Implications

In this article we have shown that in the presence of a relatively small degree of strategic uncertainty, at a Bayesian equilibrium, voters with positive net voting costs will abstain. The outcome will essentially be determined by voters who, for unspecified reasons, find it at least as costly to abstain as to vote, regardless of the likelihood that their vote will affect the outcome of the election.

The fact that voters with positive voting costs abstain implies that the outcome of an election need not reflect the underlying distribution of preferences in the population. If the distribution of cost-benefit ratios for the portion of the population that favors one alternative is identical to the distribution for those who favor the other alternative, the majority side can be expected to win.^{*} But if the low and negative cost individuals are concentrated in the minority, the minority can win.

This hypothesis suggests a useful direction for further research. An important aspect of competition between candidates must involve the mobili-

zation of support groups. Thus, much work in formal theory which focuses almost exclusively on ideological or spatial competition is missing a key ingredient of electoral competition. In order to win a vote, a candidate must not only convince the voter that he or she is the preferred candidate but must also get these voters to the polls. Exactly how to combine these two problems in a coherent theoretical model is an open and difficult question. Campaign organization would obviously be a critical aspect of such a model. Parties, as stable, institutionalized organizations, might emerge endogenously as efficient means for both generating support for candidates and getting out the vote. Also important, the distribution of voting costs, exogenously given in our model, would be endogenously determined in a more general theory of electoral competition.

This more general theory would emphasize candidate strategies designed to affect the cost of voting in a way that increases the turnout of likely supporters. These strategies might alter the cost of voting directly, as in providing transportation to the polls, or indirectly, as in providing cash or services contingent on monitoring turnout. In some situations, organized interests may use monitoring to increase the cost of voting. For example, in 1974, the French Communist Party advocated abstention in the presidential runoff between the Gaullist Georges Pompidou and the Centrist Alain Poher. Record abstention resulted, perhaps influenced by the ability of party poll watchers to monitor voting. Less subtle tactics have been used to disenfranchise entire blocs of voters in the United States (Key, 1949; Kousser, 1974).

A key feature of voting institutions in modern democracies is that turnout can in fact be monitored while choice cannot. Positive theory is needed to explain this regularity. We note that if sanctions cannot be imposed for nonvoting and rewards offered for voting, the net cost of voting would increase and turnout would fall.

We also note that the cost of voting appears to be small. Consider the following observations. 1) Fines for not voting are often modest in democracies with compulsory registration. 2) Wolfinger and Rosenstone (1980, p. 129) estimate positive effects on turnout for some simple changes in registration procedures, such as maintaining evening and Saturday office hours. 3) Perform your own Gedanken experiment on the likely effect of turnout in U.S. presidential elections if each voter were handed \$20 in cash after voting. If costs are low, turnout is likely to be very responsive to mobilization efforts by candidates and parties.

In addition to providing some suggestions for further work in positive theory, our work also highlights an important tradeoff from a norma-

^{*}Although this may seem a rather obvious conclusion to most readers, it is worth pointing out that with complete information, this is not necessarily true. At a Nash equilibrium with complete information, if all voters have the same voting costs, the minority candidate has a substantial chance of winning even when the majority vastly outnumber the minority. For details, see Palfrey and Rosenthal (1983).

tive viewpoint. As resources are spent on mobilizing voters, increased participation is likely to increase the chances that the majority side will win, as it must when there is total participation. Most proponents of majority rule would thus find increased participation socially desirable. But the expenditure of resources to increase participation is a cost to society. Thus, the theory of the efficiency of elections (see Ledyard, 1984; Palfrey & Rosenthal, 1983) remains an open question. A model that addresses mobilization issues directly is needed if we are to make headway answering this normative question as well as positive questions about the nature of electoral competition.

Appendix A. Proofs of Theorems

Proof of Theorem 1. We first prove that there exists an equilibrium with $(c_1^*, c_2^*) \in [0,1] \times [0,1]$ and then show that both critical points must be strictly positive. Consider the mapping from $[0,1] \times [0,1]$ into itself defined by

$$y_1 = P_1(c_1, c_2)$$

$$y_2 = P_2(c_1, c_2)$$

with P_1 and P_2 as defined in equations (7) and (8). Because P_1 and P_2 are continuous functions and $[0,1] \times [0,1]$ is compact and convex, by Brouwer's fixed point theorem there exists a fixed point (y_1^*, y_2^*) such that

$$y_1^* = P_1(y_1^*, y_2^*)$$

$$y_2^* = P_2(y_1^*, y_2^*)$$

Let $(c_1^*, c_2^*) = (y_1^*, y_2^*)$. Then (c_1^*, c_2^*) is a symmetric equilibrium in $[0,1] \times [0,1]$. To show the critical points are strictly positive, suppose $c_1^* = 0$. Then it must be the case that $P_1(c_1^*, c_2^*) = 0$. But this is impossible because $F_1(c_1^*)$, $(1 - F_1(c_1^*))$, $F_2(c_2^*)$, $(1 - F_2(c_2^*))$ are all strictly positive by Assumption 2.

Q.E.D.

The proof of Theorem 2 uses Dini's theorem on uniform convergence, stated below.

DINI'S THEOREM. Suppose that (f_n) is a sequence of continuous functions on a compact set, K , which converges monotonically to a continuous function f on K pointwise. Then (f_n) converges to f uniformly on K .

Proof of Theorem 2. We construct an upper bound sequence $(\bar{c}_j)_{j=1}^\infty$ such that $c_{1j}^* < \bar{c}_j$, $j = 1, 2, \dots$. We

then establish that $\lim_{j \rightarrow \infty} \bar{c}_j = 0$. It follows immediately

$$\text{that } \lim_{j \rightarrow \infty} c_{1j}^* = 0.$$

By equation (9), c_{1j}^* must satisfy

$$c_{1j}^* = \frac{\min[j-1, N_2^j-1]}{\sum_{k=0}^{\min[j-1, N_2^j-1]} W_k[j-1, F_1(c_{1j}^*)]} \cdot W_{k+1}[N_2^j, F_2(c_{2j}^*)]$$

$$\text{Since } \sum_{k=0}^{N_2^j-1} W_{k+1}[N_2^j, F_2(c_{2j}^*)] < 1,$$

$$c_{1j}^* < Q_j^* \sum_{k=0}^{N_2^j-1} W_{k+1}[N_2^j, F_2(c_{2j}^*)]$$

$$< Q_j^*$$

$$\text{where } Q_j^* = \max_{c_1, k \leq j-1} W_k[j-1, F_1(c_1)].$$

$$\text{Define } Q_j^*(c_1) = \max_{k \leq j-1} W_k[j-1, F_1(c_1)].$$

We next establish that $Q_j^*(c_1) > Q_{j+1}^*(c_1)$ for all $c_1 \in [0,1]$. To see this, choose c_1 arbitrarily between $[0,1]$. Then $Q_j^*(c_1) = W_{k^*}[j-1, F_1(c_1)]$ where k^* is the unique integer between $jF_1(c_1)-1$ and $jF_1(c_1)$. (See Feller, 1957, p. 140.) Hence $Q_{j+1}^*(c_1)$ equals either $W_{k^*}[j, F_1(c_1)]$ or $W_{k^*+1}[j, F_1(c_1)]$. In the first case,

$$\begin{aligned} \frac{Q_j^*(c_1)}{Q_{j+1}^*(c_1)} &= \frac{\binom{j-1}{k^*} [F_1(c_1)]^{k^*} [1-F_1(c_1)]^{j-1-k^*}}{\binom{j}{k^*} [F_1(c_1)]^{k^*+1} [1-F_1(c_1)]^{j-1-k^*}} \\ &= \frac{j-k^*}{j(1-F_1(c_1))} = \frac{j-k^*}{j-jF_1(c_1)} > 1 \end{aligned}$$

because $k^* < jF_1(c_1)$.

In the second case,

$$\begin{aligned} \frac{Q_j^*(c_1)}{Q_{j+1}^*(c_1)} &= \frac{\binom{j-1}{k^*} [F_1(c_1)]^{k^*} [1-F_1(c_1)]^{j-1-k^*}}{\binom{j}{k^*+1} [F_1(c_1)]^{k^*+1} [1-F_1(c_1)]^{j-1-k^*}} \\ &= \frac{k^*+1}{jF_1(c_1)} > 1 \end{aligned}$$

because $k^* > jF_1(c_1) - 1$.

Hence, we have that $Q_j^*(c_1)$ is monotonic in j . Moreover, by Assumption 2, $Q_j^*(c)$ converges pointwise to 0 on $[0,1]$. Therefore, by Dini's theorem, $Q_j^*(c_1)$ converges to 0 uniformly on $[0,1]$. Then $Q_j^* =$

$\max_{c_1 \in [0,1]} Q_j^*(c_1)$ converges to 0. (See Rudin, 1976, Theorem 7.9, p. 148.) Now just let $\bar{c}_j = Q_j^*$, $j = 1, 2, \dots$, and we have a sequence with the desired properties. Hence $\lim_{j \rightarrow \infty} c_{1j}^* = 0$.

Q.E.D.

Proof of Theorem 3. For each integer $t > 0$, define N_t as the least integer such that for all $N > N_t$, $\sum_{j=0}^t P_N(j)$

$< \frac{1}{t+1}$. By assumption 3, N_t is well defined for every $t > 0$. Next, for each integer N , define $t(N)$ as the largest integer such that $N_{t(N)} \leq N$. Because by construction, N_t is an unbounded, monotonically increasing sequence, $t(N)$ is also well defined and generates an unbounded monotonically increasing sequence.

From these definitions, it follows immediately that

$$c_{1N}^* < \frac{1}{1+t(N)} + \sum_{j=t(N)}^{N-1} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \\ \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)] P_N(j) < \frac{1}{1+t(N)}$$

$$+ \max_{t(N) < j < N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \\ \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)].$$

Taking limits, we have

$$\lim_{N \rightarrow \infty} c_{1N}^* < \lim_{N \rightarrow \infty} \frac{1}{1+t(N)} \\ + \lim_{N \rightarrow \infty} \max_{t(N) < j < N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \\ \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)].$$

By construction,

$$\lim_{N \rightarrow \infty} \frac{1}{1+t(N)} = 0.$$

By Theorem 2,

$$\lim_{N \rightarrow \infty} \max_{t(N) < j < N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \\ \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)] = 0.$$

Hence, $\lim_{N \rightarrow \infty} c_{1N}^* = 0$.

A similar argument is used to show

$$\lim_{N \rightarrow \infty} c_{2N}^* = 0.$$

Q.E.D.

Appendix B.

The Role of Assumptions

The coin-toss example is used here to illustrate the role of Assumptions 1 and 2 in the model.

ASSUMPTION 1. If the distribution function is discontinuous, then the simple existence proof doesn't work, because the fixed point mapping will not be continuous. Such a situation in which a pure strategy symmetric equilibrium as we have defined it does not exist is illustrated in Figure 5.

The source of the discontinuity is a mass point in the distribution of costs at \bar{c} . If all voters with costs less than or equal to \bar{c} vote, then $F(\bar{c}^*) > q^*$, whereas if only voters with costs strictly less than \bar{c} vote, then $F(\bar{c}^*) < q^*$. However, a symmetric mixed strategy equilibrium does exist in this example. If the strategy described is amended so that voters with costs exactly equal to \bar{c} vote with probability

$$\frac{q^* - F(\bar{c})}{F(\bar{c}) - F(\underline{c})}$$

then it is as if $F(\bar{c}^*) = q^*$ and the equilibrium conditions 2-4 are satisfied. Thus, from this example it is clear that a mixed strategy Bayesian equilibrium will exist in the absence of Assumption 1, and these equilibria will share the convergence property of Theorem 2.

ASSUMPTION 2(i). We assume that there is some chance that some voters have a negative net voting cost (citizen duty). In contrast, Ledyard (1981, 1984) assumed that there were no such voters. When this happens, $F(0) = 0$, and the graph looks like Figure 6, where \underline{c} is the lowest possible voting cost.

In this case $c_n^* \rightarrow \underline{c} > 0$, and turnout is infinitesimal (0%) in large electorates. Although this is not a citizen-duty story, it is similar in the sense that for any $c > 0$ in the support of F , there exists \bar{N} sufficiently large such that $c_N^* < c$ for all $N > \bar{N}$.

ASSUMPTION 2(ii). Assumption 2(ii) is needed to guarantee that in large electorates essentially the only voters are citizen-duty voters (i.e., $c_i < 0$). Suppose in the example $F(c) = 1$ for $c > 3/8$. This situation is illustrated in Figure 7.

Figure 5. Nonexistence of Pure Strategy Symmetric Equilibrium Due to Discontinuity of the Distribution Function

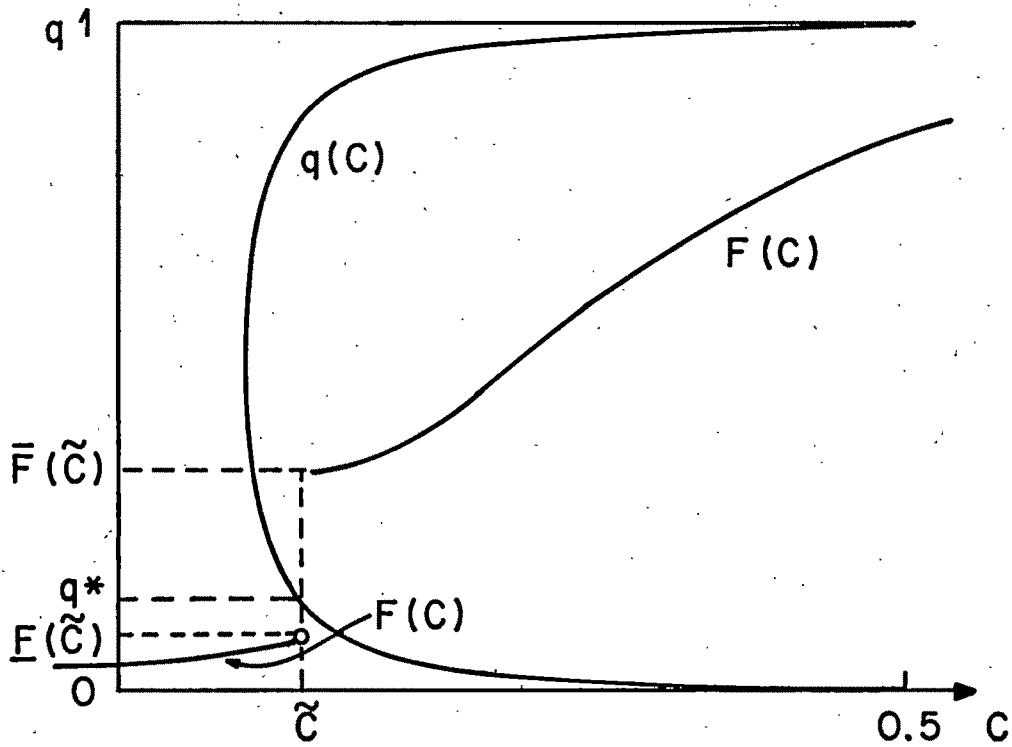
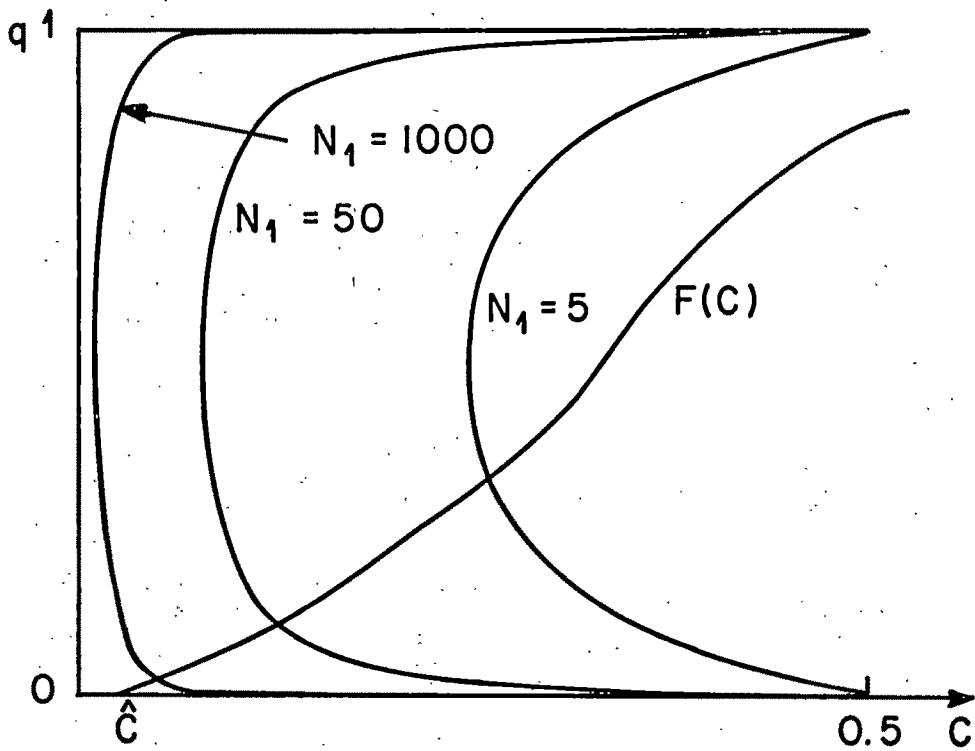
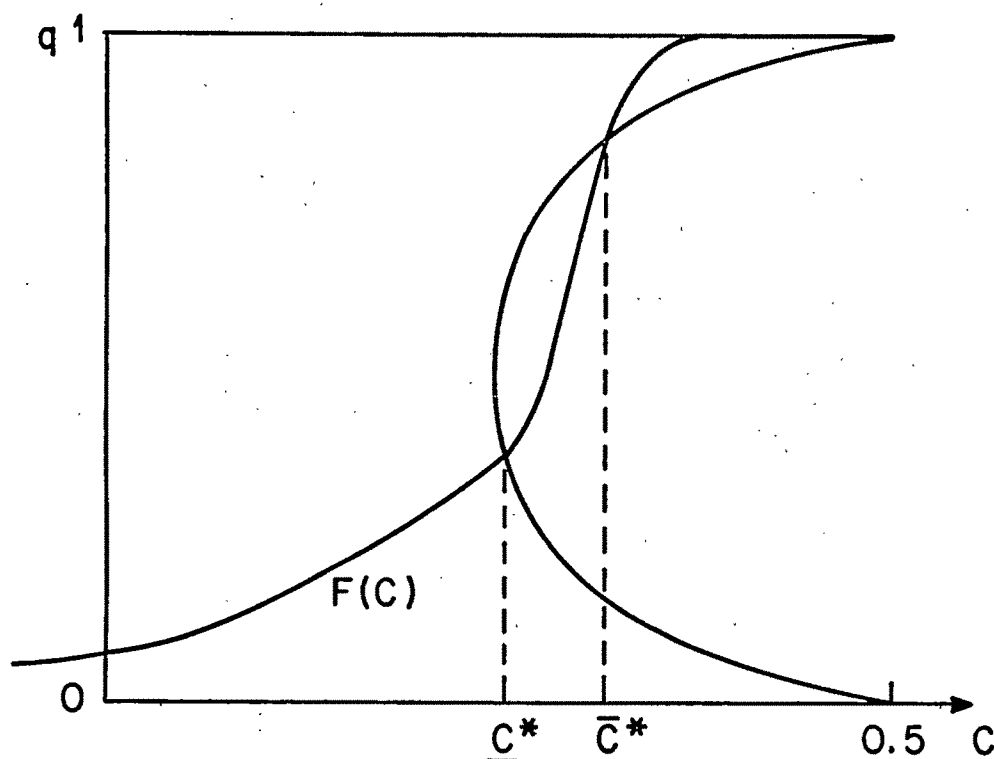
Figure 6. Convergence If $F(0) = 0$ 

Figure 7. Equilibrium if $F(3/8) = 1$ 

In such a case there may be more than one equilibrium. One at c^* (which in the limit corresponds to the citizen duty equilibrium), and one at \bar{c}^* , which is a high turnout equilibrium. There is also a third "degenerate" equilibrium in which everyone votes, regardless of voting costs. This corresponds to the pure strategy equilibrium identified for this special case in Palfrey and Rosenthal (1983). What is happening here is that there is not enough uncertainty if $F(c) = 1$ for some $c < 1/2$, because the probability that a voter will have a dominant strategy of not voting is zero.

The purpose of this extended example has been to illustrate the role of Assumptions 1 and 2. Assumption 1 is technically convenient and innocuous. As for Assumption 2, it can be shown that in more general cases (i.e., $F_1 \neq F_2$, $N_1 \neq N_2$), the same variations in the equilibria as in the example will emerge when Assumption 2 is relaxed. Assumption 2(ii) is used primarily because we feel it is a realistic assumption that some voters find it too costly to vote, regardless of the probability that they would be decisive. Assumption 2(i) is technically convenient and does not change the qualitative nature of the results at all.

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The Political Economy of Group Membership

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Despite its normative importance, the question of why people join interest groups remains open. It has certainly provoked a wealth of theoretical attention. Regrettably, however, it has inspired only a handful of empirical tests. The introduction of this article places the empirical debate into its normative context. The first section develops a rational model of individual evaluations of group membership, focusing upon the effect of changing personal circumstances—preferences, needs, resources, insecurity, and information—on the calculus. In particular, the theory predicts responsiveness to political or collective benefits in threatening times. Analyses of aggregate changes over time in membership in the Farm Bureau, the League of Women Voters, and the Home Builders, reported in the second section, bear the model out. Finally, the conclusion takes on the complementary question of group supply, sketching a theory of group mobilization that emphasizes subsidization.

Observers of American democracy have greeted political interest groups with praise and with condemnation, but seldom with both. According to some commentators, associations are the upholders of the interests of intense minorities; according to others, they are the selfish saboteurs of majority rule.

These normative conclusions, however, depend vitally on an empirical premise about why people join interest groups. If, as Truman (1971, pp. 26-43) hypothesized, people create and join organizations in response to "disturbances" in the social environment, then interest groups represent legitimate grievances that should be heeded in a democratic political system. But suppose, as Olson (1965) argued, that people who share interests with a large group join less readily than people who share interests with a small one, and suppose that joiners of large groups act in the collective interest only as an unintended consequence of their seeking benefits unrelated to the association's political purpose. Then interest

groups, if they have any influence at all, bias public policy away from majority preferences.

Here I argue that political mobilization of large groups does indeed reflect political concerns, but only under certain conditions. I develop a model of interest group membership in which group incentives interact with individual circumstances and test the model's propositions against membership data from three prominent lobbies. My conclusions are at once encouraging and sobering. Political benefits do matter, especially when groups are threatened, but they do not always matter.

A Context-Sensitive Model of Interest Group Membership

Olson's by-product theory focuses on two classes of benefits derived from group membership: collective and selective (Olson, 1965, Chap. 6). Collective or political benefits (I use the terms interchangeably) are received jointly by both members and nonmember constituents of the group, and for that very reason are insufficient inducements toward activity. Selective benefits, in contrast, are essential for cooperation because they can be provided to dues payers alone. In concept, the class of selective inducements is varied, although Olson by that term understands goods and services that can be expressed monetarily. Others cast the term more broadly, adding intangible inducements of two kinds: solidary benefits—"rewards created by the act of associating" like fun and friendship—and expressive benefits—"rewards that derive from a sense of satisfaction at having contributed to the

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attainment of a worthwhile cause" (Wilson, 1973, p. 34).

The context of the incentive model remains unspecified, however, and explicitly or implicitly, interest group theorists assume three things about it: the existence of associations offering political benefits, the extent and nature of the information available to individuals, and the configuration of individual preferences and resources. The first, of course, is simply an assumption that interest groups exist, that they supply the incentives outlined above at some cost, and that some of the benefits supplied are political. I, too, follow convention and assume that interest groups exist, although I return to the question of supply in the conclusion.

The content of the last two assumptions, my primary concern here, is usually stated as fixed and immutable, if indeed it is stated at all. But no single set of assumptions about information, preferences, and resources is universally applicable; rather, the set of appropriate assumptions changes as circumstances change. People in different contexts have different information about alternatives. Depending upon the circumstances, people will be differentially uninformed and misinformed about the actual benefits, costs, and risks of collective action.

More important, people in different contexts have different preferences and resources and hence different subjective weightings of the benefits and costs of group participation. When resources like income and time are ample, people can more easily bear costs; when people have particular needs and preferences, they are more attracted by certain benefits; and when people take different attitudes toward risk, they are more or less willing to engage in actions whose success is uncertain and contingent on the actions of others. In short, information, preferences, and resources that arise in particular situations interact with the actual incentives that organizations offer, forming the subjective assessments of benefits and costs that enter into personal calculations. In equation form, this conceptual model of the individual calculus is:

$$\begin{aligned} M &= E(B_g^s) + \Sigma B_s^s - \Sigma C^s \\ &= (B_g^a I_g T_g) (P I_p A_p) + \Sigma (B_s^a I_s T_s) \\ &\quad - \Sigma (C^a I_c R_c) \end{aligned} \quad (1)$$

where

M = membership (if $M > 0$, the individual joins; if $M \leq 0$, the individual does not)

B = benefits

P = probability that the collective good will not be provided if the individual does not join

C = costs

I_g, I_p, I_s, I_c = information about collective benefits, probabilities, selective benefits, and costs

T = preferences and needs

R = resources

A = risk attitudes

subscript g = collective or political benefits

subscript s = selective benefits (services, and solidary and expressive benefits)

superscript s = subjective levels

superscript a = actual levels

An individual joins an interest group if the subjective benefits (B^s) he derives from membership exceed the subjective costs (C^s) he incurs. His subjective assessments depend, however, upon the information (I) that he has about actual benefits (B^a), costs (C^a), and probabilities (P) (cf. Moe, 1980). In addition, the assessments depend upon environmental and dispositional factors that impinge upon specific elements of the calculus: resources (R) on costs, preferences (T) on benefits, and risk attitudes (A) on probabilities. As information, resources, preferences, and risk attitudes change, so too does the attractiveness of group affiliation.

To see how they change, consider in turn each of the three major terms in equation (1).

Expected Political Benefits:

$$E(B_g^s) = (B_g^a I_g T_g) (P I_p A_p)$$

At base, collective action is a strategic problem (Hardin, 1982). The benefits of cooperation indisputably are large. Even a small construction firm with a \$100,000 payroll (approximately 10 employees), for example, will save \$1000 a year if the unemployment tax is reduced by a single percentage point. But if the political good is won, group constituents receive the windfall regardless of whether or not they cooperated. Given the choice, then, a prospective member would rather remain prospective, reaping any benefits while avoiding all costs.

The strategic aspect, then, is in avoiding costs while being sure that the benefits keep coming. The individual has some idea of what the present and potential benefits are worth to him (B_g^a). He also has some notion of the likelihood that those benefits will *not* be provided, now or in the future, if he does not cooperate (P).¹ In large

¹This conditional probability corresponds to Hardin's (1982, chaps. 9-11) notion of collective action as an

groups and in normal times, the product of the two, the expected political benefits $[E(B_q^s)]$, will for many people be smaller than costs, although not perhaps as small as some have imagined.

Threatening times, be they political, economic, or social, alter three of the premises that underlie the conclusion that political benefits lack motivational force in large groups. First, different kinds of information are available and different kinds of information are salient in times of threat. Groups are always fending off threats; that is, the benefits are always there, but people do not notice them until they are brought to their attention. Threats bring defensive benefits to people's attention. As the adage goes, "You don't know what it's worth until you need it."

Second, potential losses weigh more heavily in people's minds than potential gains (Kahneman & Tversky, 1979). In other words, the utility of a given amount lost exceeds the utility of the same amount gained.

Finally, threats change people's risk attitudes. Faced at most times with potential political gains or at worst small losses, people are risk averse. Given a choice between a certain outcome and a probabilistic outcome of slightly greater expected value, they will choose the sure thing. Faced with large losses, however, people are risk seeking. They will gamble on a probabilistic loss of greater expected value rather than take a sure loss (Kahneman & Tversky, 1979). Realistically, a single individual's contribution to an interest group will make no more difference to its ability to ward off a loss than its ability to secure a gain, but people are more willing to take a chance that it might avert a loss.

In sum, people are more easily mobilized in response to threats than in response to prospects. Threats increase awareness (I_g) of the collective benefits of group membership. Political benefits that avoid losses are weighed more heavily than political benefits that promise gains (i.e., T_g is higher). And instead of discounting collective benefits for risk, threatened people place a premium upon them (i.e., A_p changes from less than one to greater than one). This is the insight that prompted Truman's (1971, pp. 26-43) emphasis on the motivational force of socioeconomic disturbances, and this is the insight that inclines interest group leaders to emphasize threats over prospects in their appeals. The environmental lobbies, for example, stress the environmental deter-

ioration that will occur if the person solicited does not act, rather than the environmental improvement that will come about if he does (Mitchell, 1979). After James Watt's resignation, these same groups scrambled to find a new *bête noire* as convincing as Reagan's Secretary of the Interior.

The motivational force of political benefits also varies, of course, with other elements of context. First, actual political benefits (B_q^a) change as the political agenda changes. Promoting or staving off regulation of business, for instance, is a political benefit only if policymakers can conceive of business regulation. Second, the probability that collective goods will not be provided if an individual does not cooperate (P) decreases, roughly, with group size. If the political context (for instance, a decentralized political system) makes possible provision of collective goods by local chapters, then a national federation can provide still other benefits on a more inclusive scale (cf. Hardin, 1982, pp. 195-197).

Political benefits, in short, will have different motivational value in different milieux. Like any other good, they are more or less in demand according to people's needs at the moment.

Selective Benefits: $\Sigma (B_g^s) = \Sigma (B_g^a I_g T_g)$

As with collective benefits, demand is more stable for some selective incentives than for others. Some services, such as insurance, are necessities. As a Farm Bureau officer pointed out, "You don't cancel your insurance when things get bad." On the other hand, solidary and expressive benefits by and large are luxury goods. This distinction is implicit in the frequently made assertion that associations offering only intangible incentives are less stable than those offering tangible benefits (Salisbury, 1969; Wilson, 1973), but it need not be unalterably true. For many people, for instance, religious rewards are more important than discount travel tours, and organizations offering the former intangible benefit are apt to be more enduring than those offering only the latter tangible good.

Still, the generalization that services are often necessities whereas expressive and solidary incentives are luxuries allows three predictions. First, demand for intangible incentives will be highly income elastic—that is, small fluctuations in income will produce large shifts in quantity demanded—whereas demand for tangible incentives will be income inelastic. Consequently, income changes that scarcely disturb interest groups offering tangible benefits may be catastrophic for groups relying on intangible inducements. Second, demand for intangibles will be quite price elastic, that is, sensitive to the level of dues. Price elasti-

iterated game in which anticipated reprisals of non-cooperation for noncooperation provide an incentive for cooperation that would not exist in single play games.

city will be higher still if close substitutes exist.² Because substitutable expressive and solidary benefits are so widely available—and not only in organizations—interest groups relying upon intangibles are all the more vulnerable to income and price fluctuations (cf. Knoke & Wright-Isak, 1982, p. 243).

Finally, demand for intangible selective goods will be extremely sensitive to changes in fashion. The Sierra Club, for example, has existed since 1892, yet only recently has it attracted a large following. Clearly, part of the reason is the heightened salience of conservation in the wake of Earth Day in 1970. In some circles Sierra Club membership is a status good, a symbol of the member's environmental consciousness. Sierra, like the other public interest lobbies, traffics in status. In exchange for dues no more costly than a subscription to the *New Yorker*, Sierra Club members receive a handsome magazine for their coffee tables and a distinctive decal for their car windows.

To sum up, demand for selective material incentives depends less upon context than demand for intangible selective goods. Stability of demand for incentives translates into membership stability, if members are equally able to bear costs.

Costs: $\Sigma C^i = \Sigma (C^a I_c R_c)$

Material costs, such as dues and demands on time, often are not the only price of membership. Costs might also be solidary or expressive. Many people, for example, are put off by Ralph Nader. For them, associating with Nader would be a solidary cost of joining Public Citizen. Likewise, imagine the expressive costs faced by a liberal outdoor enthusiast who joins the National Rifle Association to obtain discounts at hunting lodges (cf. Moe, 1980, pp. 608-609). In the two examples, the tastes that interact with actual costs are clear: expressive costs of NRA membership are higher for ideological liberals than for conservatives.

In general, however, for the same reasons outlined in the preceding section, the most important costs are material. People join interest groups by and large because they can afford it; studies of

interest groups virtually always find income effects (e.g., Salisbury, 1969; Tontz, 1964). Yet high income hardly implies infinite resources. When consumers choose to join interest groups, they forgo other products they might have purchased, other avenues of political participation (e.g., contributing to parties or candidates), or other consumption goods. Hence, subjective costs will be altered by the existence of substitutes (cf. Hirschman, 1971, chap. 2). Fulltime employment and membership in the League of Women Voters, for example, require an identical resource (time) and offer similar benefits (social interaction and intellectual engagement). Thus, when career opportunities for women increase, as they did during the seventies, the opportunity costs of membership in a participatory group like the League also increase.

Additivity

In some cases, the same contextual factor interacts with both benefits and costs, but because benefits and costs are additive, the ultimate effect on participation depends upon whether the factor affects benefits or costs more strongly. Income, for example, clearly is a resource that enables one to bear material costs. Some people, however, use income as a psychological indicator of personal economic well-being, affecting how they weight political benefits. The total effect of a change in income, then, depends upon which interaction is stronger.

The model's additivity also underscores the importance of quantity and quality of information. People obviously lack the time and capacity to perform all of the calculations seemingly required by equation (1). In everyday life, though, the decisional elements that people consider are the ones that are available, those that most easily come to mind (Taylor, 1982). The media hoopla that surrounded the financial irregularities of the 1972 Nixon reelection campaign, for example, heightened people's awareness of the political and expressive benefits of Common Cause membership. Moreover, the format of the information people receive greatly influences its evaluation, a psychological phenomenon known as framing (Tversky & Kahneman, 1981). If people buy lottery tickets more readily when the purchase is described as insurance against loss rather than as a gamble on gain—as they do—people will more readily join an association when the information they receive about group membership is framed in the same way. As I noted earlier, interest groups go to great pains to cast their appeals in just that manner.

In conclusion, a theory that looks only at benefits or only at costs misses a great deal of

²This holds true if consumers' demand functions are such that relative quantities purchased do not change if income and all prices are multiplied by the same scalar. Given this assumption, the sum of the own-price elasticity (almost always negative), the income elasticity (almost always positive), and cross-price elasticities (positive for substitutes, negative for complements) equals zero. This is known in microeconomics as the homogeneity condition.

what is important. Benefits and costs do not simply exist; rather they exist in particular milieus. The effects that costs and benefits have on group membership depend upon what people know about them, upon whether people need or want the benefits, and upon whether or not they can afford the costs. In the next section, which analyzes changes in membership in three lobbies, I explore empirically the context-dependence of interest group incentives.

An Overview of the Analysis

Devising tests of interest group theories has proven far more difficult than devising the theories themselves. Two modes of inquiry predominate: appeals to anecdotal evidence and case studies of particular interest groups, based sometimes upon surveys of their members.

I take a different approach, examining the annual changes in the memberships of three groups: the American Farm Bureau Federation, the League of Women Voters, and the National Association of Home Builders.¹ The Farm Bureau and the Home Builders are "economic" lobbies representing large constituencies, groups for which Olson's by-product theory is most suited. They were chosen for that very reason. The League is ordinarily thought to be a social and expressive organization; it is included to demonstrate the range of the theory.⁴

Because the theory is dynamic, the data are time series. Benefits, costs, risks, context, and membership are all constants at a single slice in time. Joining and quitting interest groups are behaviors that vary only over time, and the things that cause those behaviors vary only over time.

Because the analysis is multivariate, moreover, I can properly test a model that posits multiple causes. In each equation, my explanatory variables are environmental factors, such as actual benefits and costs, resources, and events, with which subjective benefits and costs (and

hence membership levels) should be associated, given my earlier argument. The theory predicts that membership can be expected to increase with the development of additional selective incentives. It predicts membership increases when people have higher incomes or more free time. It holds that political and economic disturbances heighten the attractiveness of political benefits, and that people consequently are more likely to join when they are threatened. When school budgets are inadequate, for example, people who value public education will rally to its support. When farmers and builders are in financial binds, they will flock to organizations that claim to have obtained vital subsidies for them. Finally, the theory asserts that group benefits are more available psychologically when the group has a high profile, so that greater visibility alone will stimulate joining. In short, the theory predicts that political and economic context affects membership because context influences subjective evaluations of benefits and costs. In turn, subjective evaluations of incentives determine membership.³

The American Farm Bureau Federation (AFBF)

The county agent has been the John the Baptist of the farm bureau movement.

Extension official W.A. Lloyd⁴

By any standard, the American Farm Bureau's success in recruiting members has been phenomenal. When it burst onto the scene in 1919 as a federation of state farm bureaus it was already, with 317,000 members, the second largest farm organization in the country, after the Grange (Figure 1). Like the other farm groups, its membership waned in the 1920s and early 1930s, then shot up quickly during and immediately after the Second World War. Now it is by far the largest farm group; its membership of 3.5 million dwarfs the memberships of the four other general farm organizations combined.

Two features of the Farm Bureau have figured prominently in conventional explanations of its

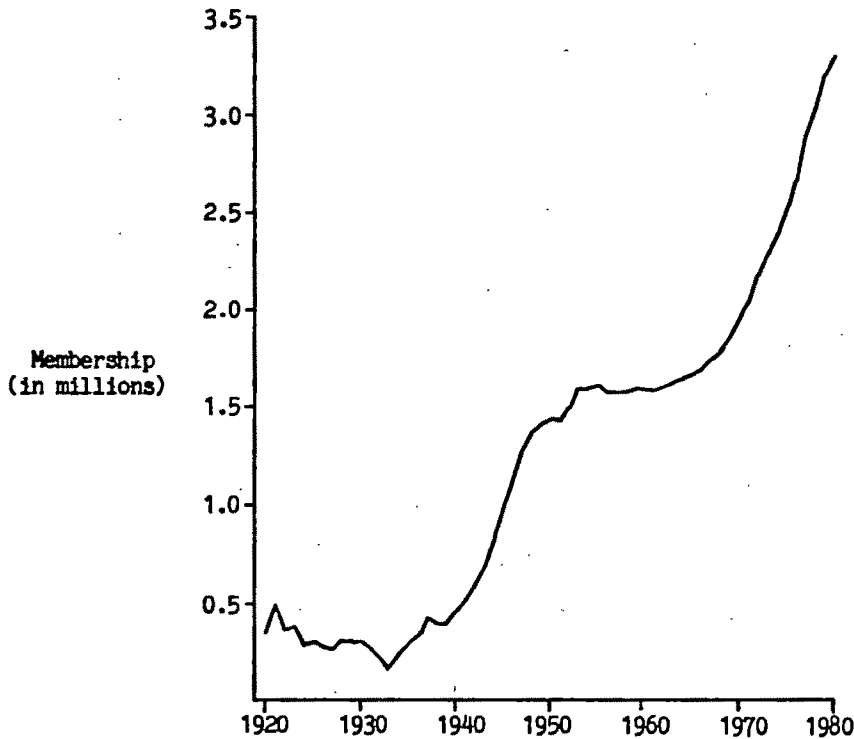
¹Few aggregate time series analyses of group membership exist. See Tontz (1964) and Russell (1937) for casual analyses of farm group membership, and Ashenfelter and Pencavel (1969) (plus a comment by Mancke, 1971) for an econometric analysis of union membership.

⁴I contacted a number of organizations within the categories of farm, business, public interest, and labor groups. Most organizations (notably unions) claimed not to have past membership tallies at all, and several more declined to divulge the information. Only the Farm Bureau had already compiled figures. These three groups, then, were selected both for variety and because they were willing to release the membership data they did have.

³Two problems should be mentioned. First, material resources and preferences for material benefits are more easily quantified than nonmaterial resources and preferences for intangible benefits. As a consequence, my estimates are biased if the omitted intangibles are causes of group membership and are associated with the included independent variables. Second, any aggregate analysis risks committing the ecological fallacy, i.e., equating the effects of the independent variables on individuals with their effects on the population. Where possible, then, I supplement the econometric evidence with historical evidence.

⁴Quoted by Baker (1939, p. 21).

Figure 1. American Farm Bureau Federation Membership, 1920-1980



Source: American Farm Bureau Federation.

organizational success. First, to an unusual degree it was created and nurtured by a government agency, the Cooperative Extension Service (Olson, 1965/1971, pp. 148-157). The Extension organized many local farm bureaus, assisted in forming the state and national federations, recruited for Farm Bureau, and sometimes favored Farm Bureau members in allocating the county agent's services. Over time, however, the Federation's reliance upon the Extension has declined markedly. Efforts to separate the public agency from the private association began as early as 1920 and proceeded slowly (Block, 1960), but even the most hostile contemporary accounts make it clear that the present relationship is but a shadow of what it was (Berger, 1971).

Second, the state farm bureaus offer their members an impressive array of services, including everything from life insurance to farm supplies. Although nominally independent, Farm Bureau businesses are legally controlled by the state lobbying organizations (Berger, 1971, chap. 4; Olson, 1965/1971, pp. 153-156). Illinois pioneered both with insurance services and with the legal arrangement when it launched the County Mutual Insurance Company in 1925. By 1940, about a quarter of the nation's farmers,

mostly in the Midwest, could purchase insurance from Farm Bureau companies. By 1950, however, nearly 85% were in the Farm Bureau insurance trade area, owing to the incorporation of companies in the South and West.

The estimates reported in Table 1 confirm the contribution of insurance services to the Farm Bureau's growth. A four-point increase in the percentage of U.S. farms for which Farm Bureau insurance was available boosted AFBF membership more than 1% in the year before introduction, anticipating the offering, and by 1% in the year of introduction.⁷ Insurance was an even bet-

⁷In none of the equations I estimated are the dependent variables normalized, that is, standardized by dividing by the relevant population. Undeniably, they should be. Changes in the size of the potential membership pool are causes of changes in the size of membership, and if population changes are associated with other independent variables, the coefficients are biased. The simple methodological problem, however, masks a thorny theoretical one: what is the proper normalizing population? In each of my cases the answer would seem to be clear, but a moment's reflection makes the quandary apparent. Farm Bureau membership since 1977 has exceeded the number of U.S. farms; the League, which

has never considered itself a "women's" organization, has admitted men since 1972; and two-thirds of NAHB membership is drawn from "derived demand" occupations, such as architects, realtors, and bankers. Conceptually, the market is clearly bounded (all who benefit from the policies espoused by the group), but operationally the market is ill-defined and ever-shifting. One of the strategies of leaders, after all, is broadening the group's appeal. Hence, normalizing by the wrong population does not insure that the bias is avoided or obviated.

The solution offered here is, I believe, more palatable. Expressing the dependent variables as percentage changes in raw membership levels introduces a sort of normalization; it narrows the relevant comparison from all observations to adjacent observations. Thus, the estimates in the equation are unbiased if the percentage change in the size of the true normalizing population is uncorrelated with the other independent variables (few of which are themselves percentage changes). Such an assumption is not unreasonable.

The Farm Bureau equation is estimated on data that were weighted to correct for nonconstant error variance and differenced to correct for autocorrelation, with rho estimated by maximum likelihood methods. The League equation likewise corrects for autocorrelation, and the Home Builders equation likewise corrects for heteroskedasticity. Both the Farm Bureau and Home Builders data are weighted by the square root of time.

ter selling point in the year after its offering—percentage point increases in membership followed from 3% increases in coverage.

It would overstate the importance of insurance services, however, to give them the full credit for the huge increases in Farm Bureau membership after World War II. As Table 1 shows, there were other factors involved, such as political benefits, rival groups, and income. Farm Bureau insurance was available to three times as many farmers after the war, but real agricultural income per farm was also about 65% higher. Farm Bureau benefits attracted new members or retained old ones only when farmers had income sufficient to pay dues (Russell, 1937; Salisbury, 1969; Tontz, 1964). Even a casual inspection of Figure 1 shows that membership trends most parallel economic trends. The twenties and thirties were bad both for farmers and the AFBF; the postwar period was good for both. The estimates in Table 1 verify powerful income effects. Roughly a threefold increase in real agricultural income per farm produced a 6 percentage point increase in Farm Bureau membership. Membership was subjectively less costly when resources were ample.

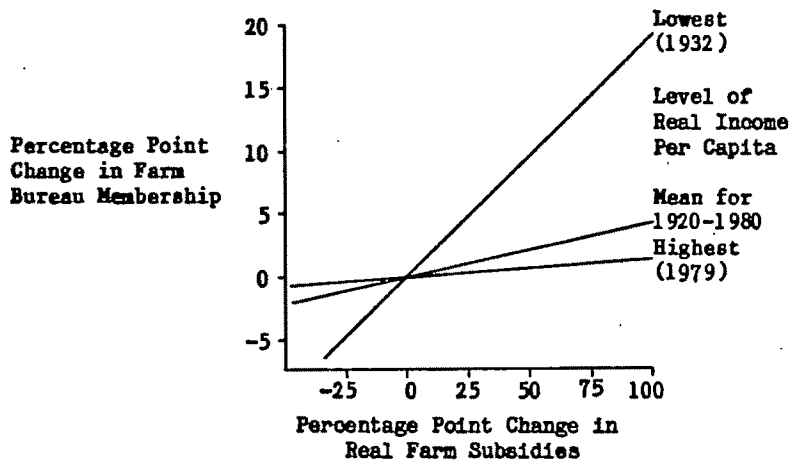
Part of the reason for the Farm Bureau's revival in the late 1930s, then, was the revival of

Table 1. Determinants of Percentage Changes in American Farm Bureau Federation Membership, 1922-1980
(Weighted maximum likelihood estimates)

	Coefficient	Standard Error
Change in percentage of farmers in a Farm Bureau insurance trade area		
($t+1$)	.30	.17
(t)	.26	.17
($t-1$)	.32	.17
Log (real agricultural income per farm in thousands of dollars)	5.59	4.10
Percentage change in real subsidy payments/ (Real agricultural income per capita in thousands of dollars)		
(t)	.14	.03
($t-1$)	.08	.03
($t-2$)	.12	.03
($t-3$)	.07	.03
Active farm protest group	-5.61	2.29
Constant	-18.82	28.65
$R^2 = .57$		
$\rho = .35$		
D.W. = 2.07		
$N = 59$		

See Appendix for data sources and definitions.

Figure 2. The Effect of Changes in Farm Subsidies on Farm Bureau Membership for Different Income Levels



Source: Table 1.

the farm economy brought about by the subsidy programs of the New Deal. But another factor in its resurgence was the AFBF's success in obtaining those subsidies. The Farm Bureau was an important—and well publicized—proponent of the 1933 Agricultural Adjustment Act (AAA), and its political success paid big dividends. As shown in Table 1, increases in agricultural subsidy payments boosted Farm Bureau membership then and for several years after, but the magnitude of the increase was contingent upon the level of farm income. When income was at its lowest point, each additional 5 percentage point current-year increase in farm subsidy payments boosted AFBF membership by 1 percentage point (Figure 2). When income was at its highest point, however, it took nearly a 100 percentage point increase in government payments to produce a percentage point increase in AFBF membership. The response to the Federation's political benefits, in short, depended upon the seriousness of the economic threat.

Of course it is impossible to tell from aggregate estimates whether joiners really were responding to political benefits. They might, for instance, have been attracted by expressive incentives. The history of the Farm Bureau, however, argues against that possibility. Although farmers have been prone to expressiveness, the AFBF has not. Even while lobbying aggressively for fundamental changes in agricultural policy, the AFBF was cool, if not hostile, toward all radical agrarian groups from the Nonpartisan League to the American Agriculture Movement. Expressive benefits have not been absent, of course. During

the New Deal, Farm Bureau rhetoric emphasized that parity prices meant "equality for agriculture," and one critic has characterized its recent ideology as "right-wing in overalls" (Berger, 1971, p. 5). By and large, however, farmers seeking expressive benefits have had to look elsewhere.

In fact, the expressively motivated farmers have looked elsewhere. Farm Bureau membership dropped by over 5 percentage points in years when the Farmers Holiday, the National Farmers Organization, and the American Agriculture Movement were active and visible (Table 1). The Farm Bureau simply did not provide the expressive benefits that farmers often sought, and when other groups did provide them, many substituted protest group activity for AFBF membership.

A more formidable alternative interpretation of the finding has it that payments under the AAA were seen by farmers not as a collective good but as a selective benefit contingent upon Farm Bureau membership. The Extension Service initially administered the AAA, and Olson (1965/1971, pp. 151-152) argues that the Farm Bureau's influence over the Extension enabled it to coerce subsidy recipients into joining. County agents "encouraged" farmers to enlist with local farm bureaus, implying that those who failed to do so would notice the difference in their checks.

The historical record indicates, however, that an explanation emphasizing collective goods cannot be dismissed so easily. Two elements of Olson's empirical argument need revision. First, the administrative authority of the Extension Ser-

vice and the influence of the AFBF over the Extension varied by farm program and by region. County-agent control over administration was greatest in areas where Farm Bureau strength was least and vice versa. The Extension possessed the most authority in the tobacco, cotton, and peanut programs of the South (Baker, 1939, pp. 75, 94), but during the thirties the Southern farm bureaus were relatively small and weak and often subservient to, rather than dominant over, the Extension services (Baker, 1939, p. 141). The influence of the farm bureaus over the Extension was greatest in the Midwest, but there the wheat and corn programs were administered mainly by committees elected by local farmers. This did not, of course, preclude Farm Bureau influence. In fact, an estimated 85% of AAA committeemen in New York, Illinois, and Iowa (all AFBF strongholds) were Farm Bureau members (Baker, 1939, p. 73; Kile, 1948, p. 205). My point here is not that coercion was insubstantial. Rather, it is that the potential ability to coerce was greatest in regions where Farm Bureau had the least to gain from it.

Second, and more important, the Extension did not administer the farm programs for very long. After the passage of the 1936 Soil Conservation and Domestic Allotment Act,

responsibilities for local administration were withdrawn from the county agents of the Extension Service. . . . The [Agricultural Adjustment Administration] had its own county offices and its own farmer committeemen. While the Farm Bureau was in some cases able to dominate the farmer committeemen of the A.A.A., the influence of the Farm Bureau with a centralized agency was not likely to be as strong as with a decentralized agency like the Extension Service, since the organization and development of the Farm Bureau virtually paralleled that of the Extension Service (Campbell, 1962, p. 157; Block, 1960, pp. 16-17).

The replacement of one bureaucracy with another makes no appreciable difference to the quality of the model; the equation predicts membership changes for 1937 to 1940, the prewar years during which the AAA administered the subsidy programs, just as well as for 1933 to 1936, when the Extension possessed the authority.⁴

The AAA subsidy programs were important for two other reasons. First, the AFBF feared that the local committees set up by the farm programs might be organized into a rival interest group, especially in the South. Hence, starting in 1935, it waged an aggressive campaign to enlist Southern committeemen as farm bureau members and

leaders (Campbell, 1962, chap. 6). The informational (and, perhaps, coercive) campaign paid off—membership growth in the South in the 1930s outpaced growth in all other regions. Second, the New Deal farm legislation was popularly associated with the Farm Bureau, the AFBF encouraged that association, and farmers joined out of responsibility or gratitude or simply to insure the flow of benefits in the future. "In states where the Farm Bureau had an active program," Baker (1939, p. 7) reports, "it claimed considerable credit for the enactment of the AAA. In these states, non-farm bureau members often felt a moral obligation to become members and promoters of the county farm bureau organization." (See also Campbell, 1962, p. 63.) The benefits that produced the aggregate relationship were not wholly selective, as Olson asserts, but also were collective. Political success and high visibility combined to boost Farm Bureau membership growth past the level determined only by income and selective benefits.

Similar circumstances may have occasioned the Federation's best recruiting year ever, 1921. By all accounts, membership should have dropped. Real income was off about 20% from 1920. More important (from Olson's standpoint), Dr. A.C. True of the States Relations Bureau (precursor of the Extension Service) and AFBF President J.R. Howard had concluded a "memorandum of understanding" that forbade county agents from recruiting for Farm Bureau.

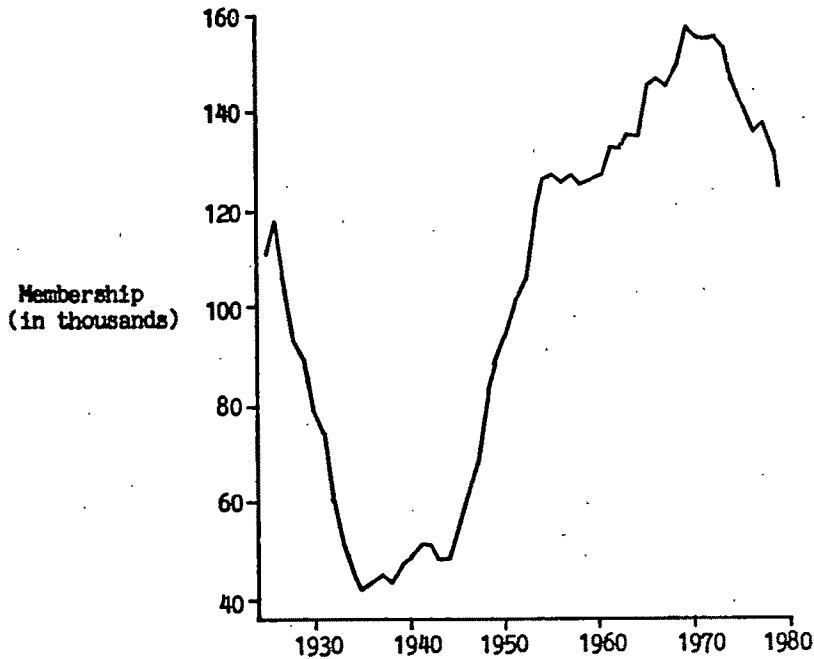
Instead of falling, though, membership jumped by 50%, AFBF's largest percentage gain ever.⁵ May 1921 saw the creation of the infamous Farm Bloc in the Washington office of AFBF lobbyist Gray Silver. By the end of the year, the Farm Bloc had passed six priority pieces of legislation, including bills regulating meat packers and the grain exchanges (Kile, 1948, chap. 7). The Federation's legislative clout was no doubt an important selling point in the recruitment campaign the new organization undertook (Kile, 1948, chap. 5).

And what of the True-Howard Agreement? Clearly, if the memorandum had any effect at all, it was not immediate. Farm Bureau membership began to drop a full year after its signing. Of course, in August 1922 the agreement was reissued as USDA policy, and this separation of the Farm Bureau and the Extension Service may have pro-

⁴The mean residual for 1933 to 1936 is 5.0. The mean for 1937 to 1940 is 3.0.

⁵Because of the differencing used in autocorrelation correction procedures, the coefficients for 1921 and 1922 dummies cannot be reported. Weighted least squares estimates indicate a 52 percentage point increase in membership in 1921 over what would be expected on the basis of other variables in the equation. The 1922 decline not accounted for by lower income, on the other hand, is only 17 percentage points.

Figure 3. League of Women Voters Membership, 1925-1980



Source: Compiled from League of Women Voters records and archives.

voked the 1922 membership decline, but it is doubtful. Neither edict, it is clear, changed long-standing practice. The separation of the Extension and the Farm Bureau proceeded slowly and by starts (Block, 1960), and as late as 1954 the USDA saw fit to reprimand the county agents for performing Farm Bureau tasks (Berger, 1971, chap. 8). The importance of the True-Howard Agreement has been overstated.¹⁰ Farm Bureau membership declined in 1922 and throughout the twenties because of a long and severe farm depression.

Even under the most cautious interpretation, then, this analysis of Farm Bureau membership is solid in its support of the theory. Political success begot membership success, especially when farmers were threatened. But the Farm Bureau's influence over the Extension is a compelling alternative explanation that cannot, and should not, be dispelled totally. This ambiguity is not present in the two remaining cases.

¹⁰Part of the confusion stems from unfamiliarity with the time frame. The agreement was signed in April, 1921, but AFBF membership figures are current as of November 30.

The League of Women Voters (LWV)

What does the average member want when she joins the League? . . . She wants to feel that the *League as a group* is effective in doing something to improve the governmental situation. . . . She wants sociability(?)[sic].

From materials prepared for the 1938 LWV Board of Directors meeting.

The League of Women Voters is a social and political organization nonpareil. Organized in 1920 at the victory convention of the National American Woman Suffrage Association, the League offers only one tangible selective incentive—policy study groups to inform its members on a broad range of political issues. As the memorandum quoted above and their own surveys indicate, its primary benefits are political and social (Cantril & Cantril, 1974, pp. 7, 15).

As a consequence of its reliance on intangible incentives, League membership has been particularly sensitive to changes in income and in fashion (Figure 3). Its membership remained high during the early 1920s but began to drop in the latter part of that decade as the enthusiasm of the suffragette days wore thin. Then came the Depression—at its nadir in 1935 League membership stood at only

40% of its total in the early twenties. But membership revived with prosperity, and only recently has it again begun to lag. Since the late 1960s, the League has had to compete with careers and with new women's groups.

Women's entry into the workforce had contradictory effects on the League. On the one hand, jobs boosted the discretionary incomes of upper-middle-class women, making membership more affordable. On the other hand, jobs provided many of the same benefits as League membership—sociability and intellectual engagement—and consumed time, a resource especially precious to the highly participatory League. The estimates in Table 2 reflect the contradiction. A percentage point increase in the workforce participation rate for females decreased LWV membership by 1.5 percentage points a year later. Two years later, however, the effect of a percentage point increase was the opposite, but not offsetting—a percentage point increase in LWV membership.¹¹ For the League, then, the coincidence of the influx of women into the workforce and the creation of the National Organization for Women (NOW), the National Abortion Rights Action League (NARAL), and other women's groups was doubly debilitating. The existence of both career and group substitutes increased the price (in this case, time) elasticity of demand for LWV membership. Because jobs produced greater discretionary income, the new women's groups, which made greater monetary demands but lesser time demands, were aided at the League's expense. A change in the relative availability of different

resources affected the relative attractiveness of groups with different cost structures.¹²

The League's decentralized, participatory character, though, made it better able to provide an important collective benefit. In many communities, the League of Women Voters was most prominent as a lobbyist of school boards.¹³ Quality of public education was just the sort of highly salient issue that could mobilize educated, upper-status women, many with school-aged children of their own. The League's 1974 self-study found, for example, that one and a half times as many women with children at home were very active in local leagues as women without children at home (Cantril & Cantril, 1974, p. 5). League involvement in school politics, moreover, was invariably directed toward *defense* of existing educational services, and relative to the more specialized teachers' unions and parent-teacher associations, it was rather episodic. Zeigler and Jennings (1974, p. 117) conclude, for example, that "non-issue-specific groups, such as the . . . Leagues of Women Voters, . . . provide support for the ongoing system, but inject little conflict. . . . They constitute a resource from which decision makers may draw in times of crisis." Accordingly, my estimates indicate that LWV membership grew by a percentage point when per capita state and local support for public education fell by \$10, in other words, when support for public education was inadequate. In some measure, then, membership in this national federation reflects neither attention to national affairs nor responsiveness to selective inducements, but

¹¹The equation is estimated on only part of the data available because yearly workforce participation rates for females do not exist before 1940. Omitting this variable for the period from 1925 to 1940 would introduce a serious specification error. Moreover, unlike the membership change series for the Farm Bureau and Home Builders, the membership change series for the League is not stationary. Hence, the equation was estimated on differenced data to insure that the patterns of association found here are not the result of both the dependent variable and the independent variables exhibiting parallel trends (this is in addition to the correction for autocorrelation).

Differencing also reduces drastically the collinearity between school expenditures and disposable income (aux. $R^2 = .39$), allowing their effects to be distinguished. But even here, disposable income has no effect on changes in LWV membership—while the coefficient is positive, it is dwarfed by its standard error ($p = .55$). The problem would seem to be the inability to measure adequately the competition from new women's groups in the 1970s. This omitted variable, which is associated with high levels of disposable income, probably biases downward the estimate of the income effect.

¹²Entry into the workforce and the founding and growth of NOW were, of course, both a consequence and cause of rising women's consciousness, and the connection between workforce participation and League membership may be as much the result of the relatively greater attractiveness of NOW benefits as of price advantages. It is clear, however, that as of 1970 few women had dropped out of the League to join other women's groups but that dropouts were more likely to be holders of full-time jobs (Cantril & Cantril, 1974, pp. 12-13). Although the League appears to have lost members as a result of its equivocal stance on the Equal Rights Amendment, the competition between LWV and NOW seems to be for women who were not members of any women's groups. The argument here is that many of these women would have joined the League had NOW and NARAL not been available. League membership held its own in the 1940s, when women entered the war economy in large numbers but no alternative women's political groups existed. The most recent LWV membership decline, however, dates from 1969, which coincidentally or not is only three years after NOW's founding.

¹³The importance of local educational issues to the League, which did not initially occur to me, was emphasized by a staffer in the LWV's membership division.

Table 2. Determinants of Percentage Changes in League of Women Voters Membership, 1946-1980
(Maximum likelihood estimates on differenced data)

	Coefficient	Standard Error
Change in female workforce participation rate		
($t-1$)	-1.63	.32
($t-2$)	.97	.31
Real per capita state and local expenditures on public schools (in dollars)	-.11	.05
Presidential election years	4.90	.82
Constant	.69	.59
$R^2 = .67$		
$\rho = -.35$		
D.W. = 2.22		
$N = 35$		

See Appendix for data sources and definitions.

rather the great salience of the political benefits offered by its locals.

The LWV's voter education and get-out-the-vote crusades naturally were no less important, but primarily because of the publicity and excitement they generated. The League has been most active, and most visible, in presidential election years. In the 1928 Hoover-Smith contest, for example, it sponsored a weekly series of pre-election radio programs, and in 1976 and 1980 it staged the presidential debates. The favorable light these events cast on the LWV boosted membership by about 5 percentage points.

Two lessons emerge, then, from this analysis of the League of Women Voters. First, League membership responds to the same stimuli to which Farm Bureau membership responds—changes in resource availability and visibility—even though the League relies more on intangible incentives. Second, threats to valued collective goods mobilized League members to the same extent as threats mobilized farmers. These conclusions apply equally strongly to the final case I consider, the National Association of Home Builders.

The National Association of Home Builders (NAHB)

[NAHB members] have a tendency to swing erratically and sharply with the trends in homebuilding: When building drops, then they get radical; but when times get good and they get fat, then they go conservative.

A National League of Cities-U.S. Conference of Mayors official¹⁴

The years after World War II brought unprecedented prosperity to the residential construction industry. As a result of forced savings during the war, federal loan and tax subsidies, population growth, and affluence, the stock of owner-occupied housing increased 246% between 1940 and 1980, compared to a population increase of only 72%. The National Association of Home Builders rode the crest of the postwar housing boom (Figure 4). Founded in 1942, NAHB grew spectacularly from its original 500 members to its current 124,000, most organized into local chapters. Only one-third are builders, "anyone who at any point in his lifetime has constructed one home for sale" (Lilley, 1971, p. 444); the majority are associates—subcontractors, architects, realtors, banks, and other suppliers.

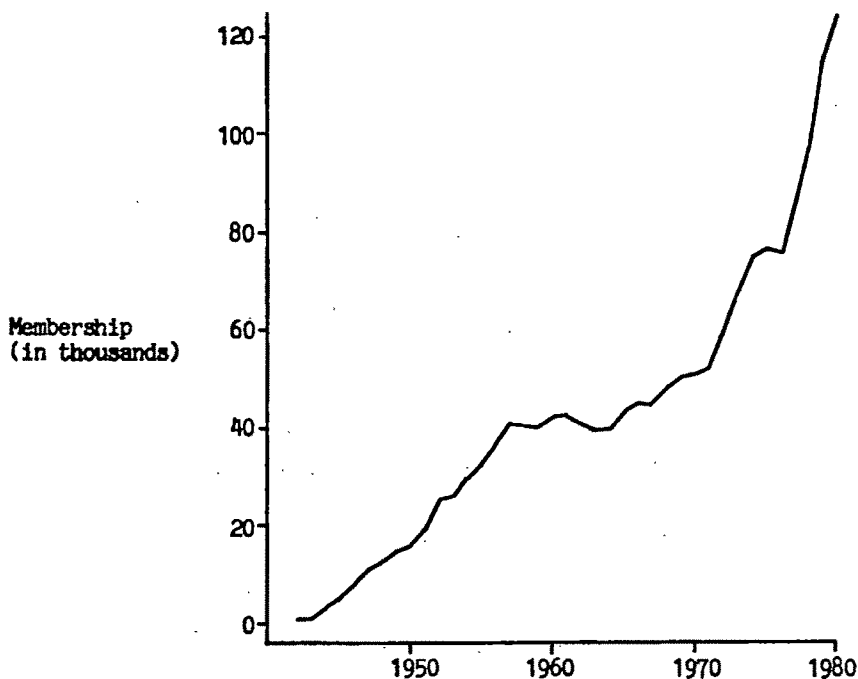
The estimates in Table 3 confirm that money in builders' pockets meant builders and suppliers on NAHB rolls and cash in NAHB coffers. A 2-billion-dollar increase in aggregate industry income upped membership by over a percentage point.¹⁵

But if prosperity was the road to organizational success for the Home Builders, it was a bumpy, uncertain ride. Federal monetary manipulations designed to stabilize the economy fell especially hard on the interest-rate-sensitive residential con-

¹⁴Unlike the independent variables in the Farm Bureau and League equations, the explanatory variables in the NAHB equation are not normalized. The Census Bureau stopped tabulating the number of firms in contract construction in 1963. Estimation of the equation with normalized independent variables for those 20 years, however, demonstrates only one specification change—the coefficient for construction income fails to attain significance.

¹⁵Quoted by Lilley (1971, p. 445).

Figure 4. National Association of Home Builders Membership, 1942-1980



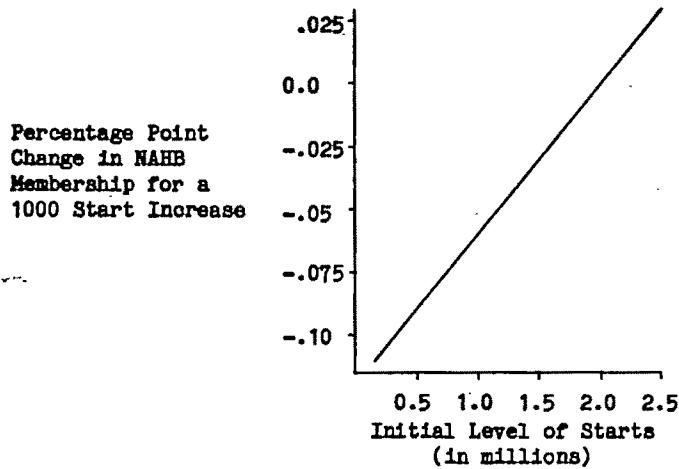
Source: Compiled from National Association of Home Builders records.

Table 3. Determinants of Percentage Changes in National Association of Home Builders Membership, 1943-1980
(Weighted least squares estimates)

	Coefficient	Standard Error
Real residential construction income in billions of dollars	.60	.17
Housing starts in thousands	.12	.02
Housing starts (in thousands) squared ($\times 1000$)	.30	.07
Housing starts under FHA insurance or VA loan in thousands/(Real residential construction income in billions of dollars)	4.41	.73
Constant	49.7	20.3
$R^2 = .87$		
D W. = 2.06		
$N = 38$		

See Appendix for data sources and definitions.

Figure 5. The Effect of Changes in Housing Starts on National Association of Home Builders Membership



Source: First derivative, with respect to starts, of equation reported in Table 3.

struction industry. Because building activity fluctuated widely, future prospects were always uncertain. Accordingly, when housing starts were below about 2 million annually, NAHB growth was greatest when starts were low, but for new construction activity above that level, growth was greatest when starts were high. At the lowest level of building activity, a decrease of 10,000 starts produced an NAHB membership increase of a percentage point (Figure 5). At the highest level, a decrease of 10,000 produced a small membership drop of approximately .2 of a percentage point.

The nonlinearity results, I suspect, from a difference between the type of firms that populated the industry in good times and the type that remained in bad times. When building activity and potential profits were high, new firms, which tended to be smaller and undercapitalized, entered the industry. In bad times, however, these marginal firms exited, leaving the large-volume, well-capitalized operations. Thus, decreases in building activity when it already was high mainly affected the small, low-volume builders, who, even though threatened, lacked the accumulated resources to pay dues. Decreases in starts when activity already was low affected large builders, who had resources even in hard times. Housing starts for builders, like educational expenditures for upper-status women, were a psychological indicator of well-being, an indicator to which large builders were better poised to respond.

Of course if homebuilders' woes stemmed from policy decisions made by the Federal Reserve Board, there clearly was little that a lobbying organization could do to help. The alternative was

to seek help elsewhere, in the bureaucracies that administered the most important and extensive housing subsidy programs, federally backed Federal Housing Administration (FHA) and Veterans Administration (VA) loans. "FHA," as NAHB's chief executive lobbyist put it, "is our major point of contact with the federal bureaucracy" (Lilley, 1971, p. 441). Even in good times the loan subsidies were of considerable importance: "NAHB membership is responsible for a [significant] percentage—90 percent or higher—of houses built under some government insurance or subsidy program" (Lilley, 1971, p. 433).

The effect of realized building subsidies on NAHB membership paralleled the effect of realized farm subsidies on AFBF membership. As shown in Table 3, the value of the collective benefits depended upon builders' incomes. The effect was greatest when income was low. When income was least adequate, a rise of only 1000 government-backed starts swelled NAHB rolls by 1 percentage point (Figure 6). When income was at its highest, the same percentage point membership increase required an additional 25,000 federally subsidized starts. The home builders did indeed get radical in hard times; they did indeed go conservative when they got fat.

As before, of course, the finding may reflect an associated selective benefit provided to members. Although FHA and VA guidelines are legislated, the agencies decide who meets the loan criteria and who does not. If the Home Builders were able to intercede on behalf of particular builders' customers, preferential treatment might constitute sufficient explanation.

Not only does such a contention lack supporting evidence, it is also implausible. NAHB does lobby FHA on behalf of individual members, but the thrust of such efforts is settlement of disputes over building code requirements for FHA-insured houses (Lilley, 1971, p. 441). The FHA and VA bureaucracies are simply not as open to NAHB influence as the decentralized Extension Service was to the Farm Bureau.

A more plausible explanation is that the Home Builders could at least put on a show of pressuring FHA and VA. As an NAHB membership division staffer commented to me, when the industry is emerging from a recession, "NAHB benefits if it has fought hard to end the crunch." FHA, like its parent bureau of Housing and Urban Development (HUD), "is renowned for slowness bordering on catatonia" (Headey, 1978, p. 193; Weicher, 1980, pp. 113-114). The Association of Home Builders could press FHA and VA to speed processing of loan applications so that builders could build.

Until recently, moreover, the loan insurance and subsidy programs were "off budget." Hence, faster processing and more lenient evaluations of loan applicants were perhaps a handy means by which the federal government could compensate the construction industry for the hardships it suffered as a result of anti-inflation policy. Whether NAHB pressure was responsible for the subsidies or not is immaterial. All NAHB had to do was claim the credit for it. Like farmers joining the Farm Bureau, builders joined the Home Builders out of gratitude, responsibility, or to insure that the flow of benefits was not interrupted.

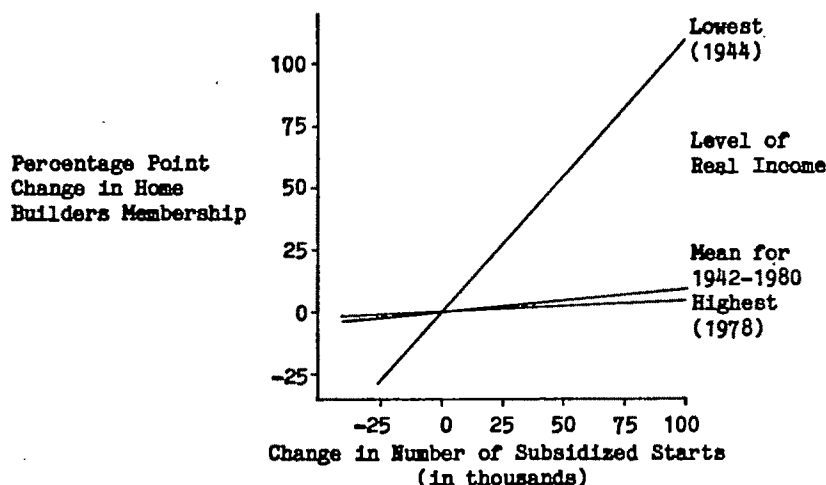
The National Association of Home Builders, in sum, presents a strong case: mobilization in response to threats and mobilization in response to political benefits offered in times of stress.

Discussion

Interest group incentives do not exist in a vacuum. As my three case studies have shown, incentives have different effects in different contexts. Two consistencies are especially important. First, political benefits matter. Although the case of the Farm Bureau is murky because of its influence over the Extension and over local committeemen, the case of the Home Builders is not. Membership responded to realized subsidies, a collective benefit. Second, political benefits matter most when groups are threatened. Subsidy benefits interacted with lower income to swell AFBF and NAHB rolls. Inadequate expenditures on public schools stimulated League growth. Contrary to Olson's claim, then, individuals do indeed join interest groups in response to collective benefits.

My conclusion, however, sets aside a complementary question, that of supply. It makes little difference whether or not members of large groups respond to collective incentives if associations representing large groups never come into being; that is, for individuals to respond to political incentives (or to *any* incentives) they first must exist. Developing membership benefits is extremely costly, and the larger the group, the more costly the organizing endeavor. For a group even to be organized, therefore, it must be subsidized

Figure 6. The Effects of Changes in Housing Subsidies on Home Builders Membership for Different Income Levels



Source: Table 3.

by entrepreneurs, by other groups, or by governments.

The key to understanding which organizations form, then, is understanding which groups get subsidized and when. Producer groups predominated early in the twentieth century, for example, because of smaller size and easier access to organizing resources. The concentration of U.S. industry insured numerous small groups and monopoly rents, that is, lower initial costs and greater resources. Producer groups, moreover, held a privileged position with government (Lindblom, 1977, chap. 13). Political leaders needed the support of business and hence were often eager to create groups as a conduit for communication. The greatest impetus to business (and labor) organization, for example, was war, when government most needed to secure the concurrence of producers. During World War I the federal government encouraged cooperation among firms in vital war industries (McConnell, 1966, pp. 60-64) and hastened the creation of local farm bureaus by the States Relations Bureau in an effort to boost food production (Baker, 1939, p. 44; Kile, 1948, p. 42). Finally, producer groups were more likely than consumers to call forth entrepreneurs willing to bear initial costs. Miller et al. (1981) have documented the importance of group identification in predicting other forms of participation. Part of the advantage of producer groups over diffuse groups, then, was more natural identification with the former and hence greater responsiveness to intrinsic, group-centered rewards such as solidary and expressive benefits.

The proliferation of consumer and environmental groups in the 1970s reflected an expansion of subsidization. Foremost among the developments was the growth of charitable foundations, the bread-and-butter of the public interest sector (Berry, 1977, pp. 72-74; Walker, 1983). Second, government programs in the 1960s often mandated or encouraged the creation of countervailing groups. And finally, entrepreneurs' consciousness of membership in more diffuse groups preceded that consciousness among the masses. Nader's experience with the Corvair certainly alerted him to the marketability of consumer crusading, but it probably also made him more willing to subsidize the consumer movement for its own sake.¹⁶

¹⁶Clearly, many large groups organized by entrepreneurs will fail, but one cannot conclude from that fact alone that rational individuals will not undertake organizing endeavors (Hardin, 1982, pp. 36-37). We commonly presume, after all, that economic entrepreneurs are rational, yet each year many thousands optimistically launch small businesses even though 62% will fail within four years (Small Business Administration, 1983).

There is, in sum, a Say's law of interest groups: supply creates its own demand. Individuals can only respond to whatever incentives are there. Subsidization does not end with creation, however; it extends even to maintenance. In a sense, politics subsidizes services. Interest groups' service organizations quite ironically are by-products of their lobbying organizations, not the other way around. In all but a few cases, services were added to political benefits, not political benefits to services (Hardin, 1982, p. 34). Interest groups entered into competition with commercial firms. If service markets are competitive and if interest groups and commercial companies face similar cost structures, no service provider can undercut the market price, and the only competitive avenue left is product differentiation. In addition, out of the many firms that offer a particular service, *only interest groups offer political benefits in addition*. Part, therefore, of the politically induced changes in interest-group membership levels stems from competitive advantage.

One can, then, identify three classes of joiners motivated by political benefits. For some people, political benefits are sufficient of themselves. Others join both for services and for policy, but either alone is insufficient (Moe, 1980, p. 607). For still others, political benefits are the crucial quality difference; they make the difference between buying from the interest group and buying from a commercial firm. In each case, political benefits are the pivotal component because they subsidize services. Far from being a marginal phenomenon, therefore, responsiveness to political incentives is more widespread and noteworthy than heretofore believed.

Two implications follow. First, the organizational prospects for interests common to large groups are not nearly so bleak as critics of group theory have asserted. On the other hand, because start-up costs are related directly to group size, subsidization is essential for organization. In a sense, this conclusion is more pessimistic even than Olson's, pointing as it does to the explicitly political calculations that give rise to group formation. If certain associations do not exist, it is because it was not in the political interests of resource holders to put them there. That is not to say, of course, that subsidization is the *sine qua non* of organizational success. Demand for interest group benefits must also exist. Perhaps this seems equivocal; that is, to argue that the high risk of failure among expressive groups, to take one instance, results from the absence of effective demand. But groups offering material services fail or lose members as well. The Farmers Alliance, for example, offered a variety of farm services but collapsed when the 1893 depression undercut its members' buying power and the People's Party

stole its political program. In short, there are no sufficient conditions for interest group formation and maintenance, only a host of necessary—and highly contingent—conditions.

Appendix: Data Sources

Unless otherwise noted, the data were drawn from *Historical Statistics of the U.S. from Colonial Times to 1970* and updated from the *Statistical Abstract of the United States*, both published by the U.S. Commerce Department. Real money series are in 1980 dollars, and all were deflated by the personal consumption price deflator. The deflator for 1920 to 1970 is drawn from *Long Term Economic Growth, 1860-1970*, a U.S. Commerce Department publication (1973), pp. 222-223, and updated from the *Statistical Abstract*.

American Farm Bureau Federation membership as of November 30, 1920-1982: Courtesy of the American Farm Bureau Federation.

League of Women Voters membership as of approximately January 1, 1925-1982: Compiled from League records and archives by the author.

National Association of Home Builders membership as of January 1, 1942-1982: Compiled from NAHB records by the author.

Agricultural income: 1929-70: National income from agriculture, forestry, and fisheries, Series F-227. 1921-28: Robert F. Martin, *National Income in the United States, 1799-1938*. New York: National Industrial Conference Board, p. 65.

Direct subsidy payments to farmers: Series K-326. When expressed as a percentage change, 1933, the year payments commenced, is coded zero.

Farm population: Series K-1.

Number of farms: Annual: Series K-4. Quinquennial, by state: Series K-17 to K-81.

States with Farm Bureau insurance companies: *Farm Bureau Insurance*, pamphlet, American Farm Bureau Federation, 1981; *Best's Insurance Reports: Property/Casualty, Life/Health*. Oldwick, N.J.: A.M. Best Co., 1983; Kile, 1948, pp. 346-352; Berger, 1971, chap. 4. States were considered to have Farm Bureau insurance from the date at which the company currently offering either property and casualty or life insurance (whichever was earliest) was founded. Because one company often serves several states, this estimate probably errs toward crediting states with an insurance program before they actually had one. For other states, however, it errs toward lateness, because many contracted for insurance through the Nationwide Insurance Company (an Ohio Farm Bureau affiliate) before forming their own companies.

Active farm protest groups: A dummy variable for the Farmers Holiday Association strikes and penny auctions (1932 and 1933), the National Farmers Organization protests and holding actions (1956-1962, 1964, 1967 and 1968), and the American Agriculture Movement tractorcades (1978 and 1979).

State and local expenditures for public schools: Series Y-686. Data are annual for 1952-1980 and are interpolated from biannual data for 1924-1952.

Labor force participation rate for females: Series D-36. Housing starts: Series N-156.

New privately owned housing units started under FHA insurance or VA guarantee: Series N-180 and N-181. Residential construction income: Value of new residential construction put in place, Series N-3.

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Aggregate Stability and Individual-Level Flux in Mass Belief Systems: The Level of Analysis Paradox

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Converse's findings of low constraint and stability among mass attitudes are only one side of the story; mass attitudes often manifest much more coherent and stable patterns at the aggregate level than would seem possible if one took the results of panel survey analysis at face value. Items designed by Rokeach and Inglehart to tap basic value priorities, show modest individual-level stability, together with remarkably high aggregate stability structured in ways that could not occur if random answering were the prevailing pattern. Materialist/postmaterialist values show large differences between birth cohorts that not only persisted throughout 1970-1984 but seem to reflect distinctive formative experiences that occurred as much as 50 years ago. These aggregate results are much too skewed to result from equiprobable random answering and cannot be attributed to methods effects. They reflect underlying attitudinal predispositions in the respondents themselves. While random response to given items does play an important role, it is much less widespread than Converse's Black and White model implies and does not generally reflect an absence of relevant preferences. Structural equation analysis of multiple indicators demonstrates much stabler, broader orientations underlying the response to given items that account for the high aggregate-level stability observed here. Because it usually measures such orientations imperfectly, individual-level survey data tend systematically to underestimate constraint and stability in mass attitudes.

The development of behavioral research in the postwar era provided disturbing evidence about the coherence and stability of mass political orientations which has haunted us ever since. A central role was played by Converse (1964, 1970), who found that the political ideas of the American public generally display surprisingly little struc-

ture and seem to fluctuate capriciously over time. Converse's analysis of panel survey data from 1956, 1958, and 1960 showed that on the average, less than two-thirds of the American public took the same side on important political issues at both time points in a two-year period. Because approximately 50% would do so by chance alone, public attitudes seemed disarmingly unstable.

The implications of these findings were disturbing. To many people they seemed to imply that representative democracy was an unattainable myth. Under democratic norms, public officials are supposed to implement policies that reflect the preferences of the majority of citizens, but if most citizens don't really have any coherent or stable preferences about major political issues, why should political decision makers take them into account? Indeed, how could they?

In some quarters, Converse—the bearer of unwelcome news—was even depicted as an advocate of elitism. Although the charge is clearly unwarranted, the implications of his findings are troubling indeed. They have given rise to a wide-ranging, often heated debate (Achen, 1975; Converse, 1974, 1980; Erikson, 1978; Jackson, 1983; Judd, Krosnick & Milburn, 1982; Judd & Milburn, 1980; Kinder, 1983; Martin, 1981; Pierce & Rose, 1974).

The debate continues: In 1983, Converse's (1964, 1970) original articles were cited in literally scores of publications, some agreeing and others

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disagreeing. There are several reasons why this controversy has been so enduring. The first reason is the powerful normative implications; it raises the question of whether or not representative democracy is meaningful or even possible if non-attitudes are as widespread as Converse's analysis implies. Second, it raises the basic epistemological question of survey research: Can we trust the evidence of our measuring instruments? The findings suggest that survey evidence is mostly noise. Converse concluded that more than three-quarters of the public were giving random answers to the question used to illustrate his Black and White model. He never claimed that this held true of all political attitudes, of course, but the impact of his analysis stems from its implications concerning the nature of belief systems in mass publics, not the stability of one particular item. One commonly finds continuity correlations as low or lower than that found with the item used in the Black and White model, and when one does, the model implies that the overwhelming majority have no real preference and are giving random answers. If this is true, it is important and alarming information for anyone involved in survey research.

There is a third reason why the debate has been so enduring; both sides are partly right. As I will argue in this article, Converse was right; there is a major component of random answering in most survey data. Moreover, it can be traced to the respondents themselves—it is not simply an artifact of poor questionnaire construction (although vague or confusing questions would increase it). Random answering exists, but it is not nearly as widespread as Converse's analysis indicates, and it is not located where he placed it, concentrated entirely in one of two radically different types of respondents. Instead, the evidence presented here indicates that most respondents *have* preferences that are relevant to most important social issues, but it is difficult to measure them accurately, given the constraints of time and motivation that normally characterize survey research. As a result, survey data tend systematically to underestimate both stability and constraint in mass attitudes.

In short, Converse contributed an immensely important insight. Survey data are not nearly as hard as they would seem if one took all marginal distributions at face value. But this insight must be supplemented by another one, growing out of a cumulative body of research: survey data are not nearly as soft as they would seem, if one took the turnover displayed in a typical panel survey at face value. Instead there are underlying elements of stability which can be discerned when one applies more refined measurement techniques and which manifest themselves directly when one examines certain types of survey data at the ag-

gregate level, as I do in this article.

Figure 1, which depicts three different models of attitudinal stability, illustrates the argument. The first model is a Markov Chain model of the type that Converse (1970) starts with in his analysis of attitudes and nonattitudes. Using this model, let us assume a correlation of .40 between the public's issue preferences at time 1 and the same individuals' preferences on the same issue at time 2, two years later; similarly, there is a .40 correlation between preferences at time 2 and preferences at time 3, four years later than time 1. The broken arrow indicates the correlation that would exist as a result of these two causal linkages. Assuming that no other factors are involved, this model implies that there is a correlation of .16 between preferences at time 1 and preferences at time 3 ($.4 \times .4 = .16$); one would expect a pronounced decline in the correlation as the time interval gets longer.

But the findings Converse actually observed in his 1956-1958-1960 panel survey deviated drastically from this pattern. The correlations from time 1 to time 2, and from time 2 to time 3, were both approximately .39, but the correlation from time 1 to time 3 was also about .39. The correlations over two years were not very high, but they showed no decline when extended over longer periods of time.

These findings were astonishing because they seemed to violate normal causal principles. In order to resolve this paradox, Converse developed the Black and White model depicted in Figure 1. The observed phenomena could be explained if one assumed that a large share of the sample (80%, in this case) had no real preferences, or nonattitudes, and were responding literally at random. Because their responses were random, they would show no correlation whatever from one time to the next, regardless of whether the time intervals were long or short. The remaining respondents' behavior was totally different (hence, the "Black and White" label for this model). This second group not only had real preferences, but their preferences were totally stable; regardless of how much time elapsed, the correlation remained a perfect 1.00. For completely different reasons, the attitudinal stability of both groups shows no change over time. Thus, when the two groups were mixed together in a representative sample of the public, one would observe an overall continuity correlation of only .39 from one survey to the next, but the correlation would not decay over time.

This model fit the data neatly, explaining some findings that otherwise seemed astonishing. But the Black and White model is problematic. Apart from its alarming normative and epistemological implications, it is inherently implausible, because

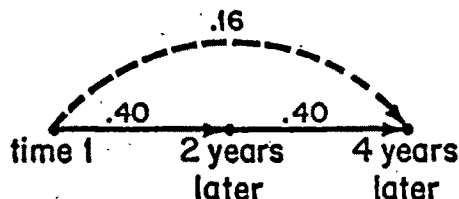
it postulates that the public is dichotomized into two radically different types of respondents—a large group of apoliticals and a small group of rigid ideologues, with nothing in between. A large body of survey research, including some of Con-

verse's own work, suggests that this is unlikely—the public normally falls at various points along a continuum, rather than being polarized at the extremes.

The third model in Figure 1 provides an alter-

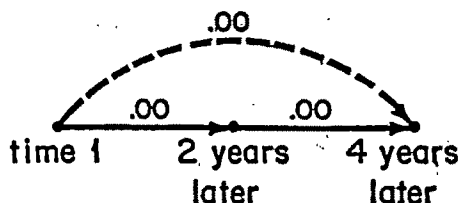
Figure 1. Three Models of Continuity and Change in Mass Attitudes

1. Markov Chain model:

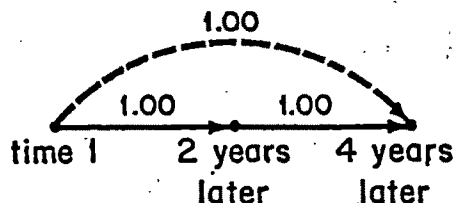


2. Black and White model:

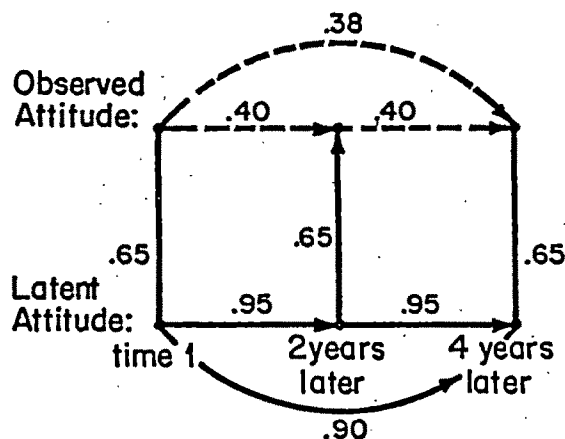
(a) 80% of sample



(b) 20% of sample



3. Latent Attitude model:



native explanation. This model is based on the assumption that practically everybody has relatively stable underlying preferences that shape one's responses to important political questions, but that any given survey question measures them imperfectly. If, as in the example shown here, the latent attitude has a stability correlation of .95 over each two-year period, but there is a correlation of .65 between the observed attitude and the latent attitude at any given time, then one would find a continuity correlation of approximately .40 between the observed attitude at time 1 and at time 2 ($.65 \times .95 \times .65 = .40$). In addition, one would find a correlation of .38 over four years ($.65 \times .90 \times .65 = .38$), a figure only slightly lower than the two-year correlation of .40. In empirical research, a difference of this magnitude might not be detected, leaving the impression that there was literally no decay over time. Over long periods of time, the decline in correlations would become more pronounced, of course. Over an eight-year period, stability in the latent attitude would fall to $r = .81$, resulting in an observed attitudinal correlation of .34, which would be less likely to pass unnoticed. But over a period of four years, the observed results from the Latent Attitude model would closely approximate those from the Black and White model in Figure 1.

In other respects, however, the two models have profoundly different implications, which can be tested empirically. Given a two-year continuity correlation of .39, the Black and White model implies that the overwhelming majority of respondents have no real preferences and are giving random answers. The Latent Attitude model postulates that all or most respondents have real preferences. At any given time, some may be giving random answers (this is one possible interpretation of why there is an imperfect correlation between latent and observed attitudes), but if so, it holds true of only a minority of the sample. The Black and White model postulates that the public consists of two radically different types of respondents: Blacks, characterized by total flux, and Whites, characterized by total stability, whereas the Latent Attitude model implies that the public is characterized by various shades of gray. Finally, the Latent Attitude model implies that one may find certain patterns of aggregate stability in the attitudes of mass publics that could not exist if the Black and White model applied. In the next section I present extensive evidence of such stability.

The Black and White model is more parsimonious than the Latent Attitude model in one respect; it deals only with directly observed attitudes, whereas the Latent Attitude model postulates the existence of a second level of attitudes which is not observed directly. On the other hand, the

Black and White model is less parsimonious in that it postulates the existence of two radically different types of respondents, whereas the Latent Attitude model only requires one. Thus far, no one has been able to isolate the Blacks from the Whites, and I suspect that the task cannot be accomplished. However, the Latent Attitude model raises the question, "Why is there an imperfect correlation between underlying preferences and observed responses?"

One possible answer is because the questions are too vague or confusingly worded; better-written questions would produce a 1.00 correlation between the respondents' real preferences and the observed responses, resulting in much higher levels of stability over time and much greater ideological coherence. This possibility was emphasized in some of the early criticism of the Black and White model. But although this explanation may apply in certain cases, its general applicability has become almost impossible to maintain. During the past few decades hundreds of investigators in dozens of countries have attempted to develop and administer clear and meaningful political questions in public opinion surveys. The results have been dismayingly uniform. Political party identification often shows high stability, but apart from methodological artifacts, one almost never finds high levels of constraint or stability in response to questions about policy preferences among the general public. On the other hand, one does find relatively stable and coherent policy attitudes among political elites, even in response to the same questions that produced low stability and constraint among the general public (Converse & Pierce, n.d.; Inglehart, 1984; Putnam, Leonardi, & Nanetti, 1979). Thus the problem does not seem to lie primarily in the questions themselves, but rather is largely one of motivation and time. Even if people have preferences that are relevant to a specific sociopolitical question, it may take time and effort to articulate them. In the typical survey situation, a stranger appears at the respondent's door and asks him or her to respond to as many as 300 or 400 questions with a promise that the interview will be over within a reasonably short time. Most members of the public are cooperative under these circumstances. They give answers and sometimes very thoughtful ones, but the process of working out the cognitive implications of one's basic preferences may involve a lot of work. When faced with a large number of questions on which he or she has not previously articulated a position, the respondent may be under considerable time pressure. One is most likely to obtain stable responses that reflect basic preferences in connection with those topics that the respondents have frequently discussed and about which they

have already made the necessary inferences before the interview.

It is probably for this reason that one obtains substantially higher stability coefficients for political party identification than for virtually any other political variable. Throughout one's life, one is repeatedly called on to express overt support for one party rather than another through explicit actions such as voting and in political discussions and other forms of political action. One's religious affiliation is one of the few variables that shows a comparable degree of stability, probably for similar reasons. A majority of the American public report that they attend church at least once a month. With both political and religious orientations, the likelihood is relatively high that the respondent has already worked out the necessary cognitive connections before the survey interview takes place.

The key point is that it may take a good deal of effort to put one's gut feelings into words. Zajonc (1980) argues that affective and cognitive orientations are distinct systems that influence each other but have a large degree of autonomy; furthermore, reversing the tendency to view one's basic preferences as the logical consequence of all one's relevant cognitions, he presents evidence that affect may precede cognition (Zajonc, 1980; Zajonc & Markus, in press; Zajonc, Pietromonario, & Bargh, 1982). In other words, one does not necessarily start with articulate, rational considerations and then derive general preferences from them; it is an interactive process in which feelings sometimes come first. Reasoning along similar lines, I suggest that even the apparently "random" respondents in panel surveys often have underlying affective orientations that are only loosely linked with their cognitions about specific public policies, but are logically relevant to them. Although affective orientations seem to arise quickly and effortlessly, the process of working out their logical implications for each of the innumerable policy questions that face a society would require a great deal of time and effort. Unless they are involved in daily discussion of policy questions, most people have not made all of the relevant inferences. When asked their opinions in a public opinion survey situation, they do not always stop and take the time required to derive the policy implications of their underlying preferences. Instead they may give superficial answers that are not necessarily consistent with their basic value preferences. Consequently, random answering is most likely to occur when the respondent does not start the interview with a prefabricated cognitive-affective linkage relating to the given topic.

Thus, although I reject the Black and White model, I am in partial agreement with Converse:

random answering does play an important role in survey research. But our interpretations differ in several respects. Mine implies that: 1) Random answering is much less widespread than the Black and White model suggests, and evidence presented below indicates that at any given time it is limited at most to 20 to 30% of the sample; 2) Random answering is not concentrated among a distinct group of respondents who have no relevant preferences and therefore always give random answers on a given topic, but is scattered throughout the sample; 3) Random answering is largely a question of time and motivation. Consequently, under favorable circumstances the percentage of the public giving constrained and stable answers can become relatively large; conversely, with low incentive and high time pressure, it increases; and 4) If relevant preferences exist and the problem is mainly one of measurement error, then multi-item indicators should enable us to tap these preferences more accurately and with greater stability and constraint than their components.

During the past decade, the development of increasingly refined techniques has led to cumulative advances in measuring latent variables and estimating the impact of measurement error (Achen, 1975; Erikson, 1978; Jackson, 1983; Judd & Milburn, 1980; Pierce & Rose, 1974). The results provide increasingly strong support for a latent attitude interpretation. In the latest and most advanced of these investigations, Jackson (1983) has reanalyzed the data examined by Converse using the LISREL structural equation approach.

He finds: 1) a substantial amount of measurement error, although whether this is the result of nonattitudes among respondents or poor questionnaire design is not ascertained; 2) a good deal of constraint in the form of a common policy orientation underlying the issue questions included in the panel; and 3) a relatively high level of stability: the common policy orientation shows a continuity correlation of .66 across the four-year period from 1956 to 1960.

Jackson's analysis explains the observed phenomena in an elegant and persuasive fashion. Nevertheless, there is still some room for doubt about this type of explanation, because to some extent, one's results depend on how one constructs the model. Although Jackson's results point in the same general direction, they are significantly different from those obtained by Achen (1975) or Erikson (1978) or Judd and Milburn (1980). The raw data are straightforward, and they show low stability. Evidence of underlying stability emerges only after the data have been subjected to some relatively complex processing. As Converse put it in his response to

Judd's and Milburn's LISREL analysis:

To my eye, the authors obliterate these signs of confusion analytically and then, turn around to proclaim proudly that no such confusion exists, at least not "really," for example in the estimates from the structural model. I am not unaware of what this kind of "real" means. At the same time, I cannot forget how the data looked before the authors began to process them, and I am convinced that those raw data say something about the reality out there, too. (Converse, 1980, p. 646)

Is the stability that Jackson and others detect produced by statistical sleight-of-hand, or does it reflect stability in the real world of a kind that sometimes manifests itself directly? In the following section, I present a massive body of empirical evidence that strongly supports the latter interpretation. This evidence indicates that the belief systems of mass publics are underpinned by remarkably stable components that seem to persist not merely over periods of two to four years, but over decades.

This evidence is of two complementary kinds. The first kind is panel surveys carried out over various intervals, ranging from two months to seven years. In each of these surveys the respondents gave high or low priority to various societal goals; the results show continuity correlations that are rather modest over short periods, but decay very slowly over long periods of time, in keeping with the Latent Attitude model. The second kind of evidence is cohort analysis of these same goals, as ranked by successive cross sections of western publics at numerous time points across the period from 1970 through 1984. The results display a remarkable component of stability, in the form of substantial differences between birth cohorts which have persisted throughout the tumultuous economic ups and downs that characterized this era. And—of crucial significance here—this stability is structured in a way that is incompatible with a Black and White model; it can only be explained by a Latent Attitude model, because the respective birth cohorts maintain very different positions throughout the 14-year period. Among the oldest cohort, there is a huge preponderance of materialist over postmaterialist responses; among the youngest cohort, the two types are almost equally numerous. These distinctive skews could not result if more than a small minority were answering at random, and they cannot be attributed to methods effects because both young and old were asked the same questions in the same format. Such a pattern could only result from relatively stable, long-term attitudinal predispositions within all or most of the respondents themselves.

Examining the panel survey response to these items might lead one to underestimate grossly this underlying stability, however, because the turnover is as high as that found with the items used to illustrate the Black and White model. There is no alternative but to turn to a Latent Attitude model. When I do so, LISREL analysis yields results similar to those Jackson and others have obtained with data from the 1956-1960 panel. Although there is substantial measurement error at any given time, most respondents have underlying preferences that remain stable throughout the seven-year period of our longest panel.

The problem is not nonattitudes so much as measurement error. Accordingly, using better measurement techniques, one finds much greater stability. This point can be made more directly, even without going into the complexities of LISREL. In addition to the simple and widely used four-item battery designed to measure materialist/postmaterialist value priorities, a broader-based 12-item battery was also developed in 1973 and has been used in several surveys since that time. As one would expect, 12 items enable us to tap the underlying attitudinal dimension more accurately than is possible with four items. Consequently the 12-item index consistently shows greater stability over time and greater constraint with other attitudes than does the 4-item index.

Similarly, when we move from the public-opinion-survey situation to a classroom setting in which the context tends to stimulate higher levels of interest in the task of articulating one's values and provides a more leisurely setting in which to do so, there are substantially higher levels of stability—even among respondents with relatively modest educational levels.

Let us now examine the empirical evidence on each of these points, starting with a cohort analysis based on cross-sectional survey data extending over 14 years. I will compare these results with findings from a two-wave panel survey carried out in the United States in 1974 and 1981 and (more briefly) with results from other panel surveys.

Cohort Analysis of the Values of West European Publics, 1970-1984

The theoretical framework underlying my cohort analysis implies that there has been a gradual intergenerational change in the relative emphasis placed on certain basic goals—a shift from materialist toward postmaterialist values (Barnes, Kaase et al., 1979; Inglehart, 1971, 1977, 1981). If intergenerational change can be demonstrated, it reflects the persisting effects of distinctive cohort experiences that may have taken place as much as four or five decades ago, and will

constitute strong evidence that mass publics have enduring sociopolitical preferences.

The materialist/postmaterialist thesis is based on two key hypotheses: 1) a *scarcity hypothesis*, that an individual's priorities reflect one's socioeconomic environment, so that one places the greatest subjective value on those things that are in relatively short supply; and 2) a *socialization hypothesis*, that to a large extent, one's basic values reflect the conditions that prevailed during one's pre-adult years.

The scarcity hypothesis implies short-term changes, or period effects; periods of prosperity lead to increased postmaterialism, and periods of scarcity lead to materialism. The socialization hypothesis implies that long-term cohort effects *also* exist; the values of a given generation tend to reflect the conditions prevailing during its pre-adult years. The theory implies nothing about aging or life-cycle effects.

Taken together, the two basic hypotheses imply that the process of value change is characterized by period effects (reflecting short-term fluctuations in the socioeconomic environment) superimposed on long-term cohort effects (reflecting the conditions prevailing during a given age group's formative years).

An empirical test of this theory requires that one distinguish among period effects, cohort effects, and life-cycle effects. This is not an easy task. The three types of effects are linearly depen-

dent on one another and cannot be statistically distinguished from each other if the only information one has is each respondent's year of birth and his or her age at the time of each survey. In order to distinguish among the three effects, one must have theoretical grounds for ruling out or controlling for at least one of the three.

The original 4-item materialist/postmaterialist values battery has now been administered to representative national samples of the populations of Britain, France, West Germany, Italy, Belgium, and the Netherlands in 11 different years (from 1970 to 1984) in surveys sponsored by the Commission of the European Communities. This massive data base is now reaching the point where it provides measurements at enough time points to trace the rise and fall of period effects and to correlate them with their underlying causes. In order to maximize reliability, my cohort analysis is based on the pooled samples from all six nations. These pooled results, providing an average of approximately 2,000 cases per age cohort in each year, enable one to follow the value priorities of West European publics across a 14-year period that began with high prosperity, but later was characterized by two recessions and runaway inflation.

Before I examine the cohort analysis, let us ask: exactly what kind of pattern does each of the three types of effects imply? Figures 2-4 depict three possible patterns. Figure 2 depicts the pat-

Figure 2. Cohort Effects Only

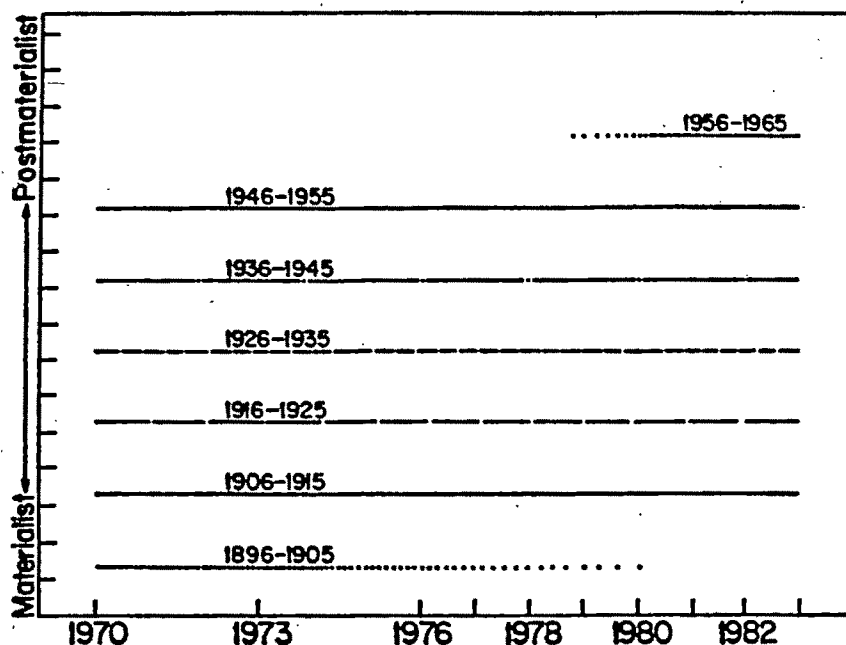


Figure 3. Cohort Effects Plus Period Effects

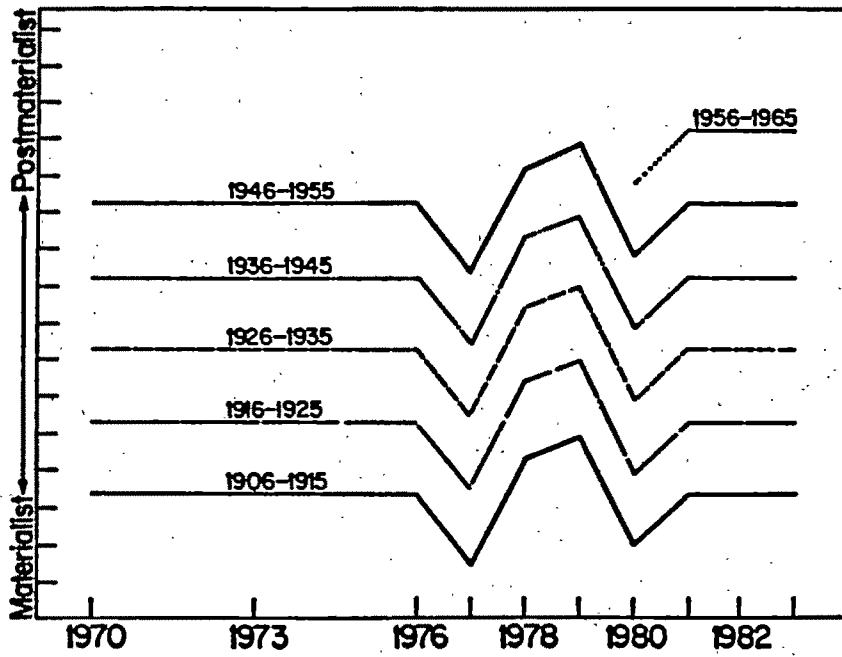
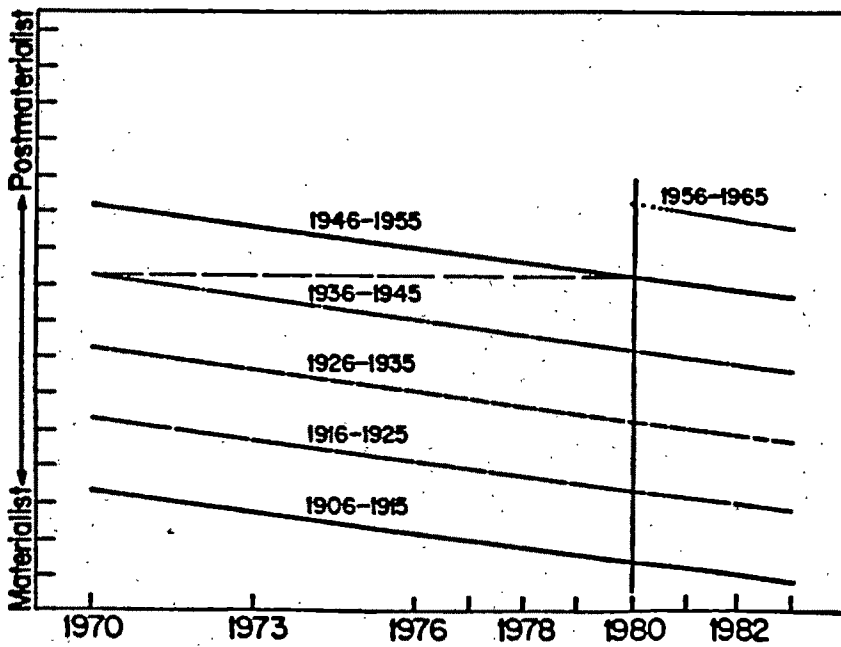


Figure 4. Life Cycle Effects Only



tern of value differences one would find if early socialization were the only influence on adult values, so that cohort effects, and only cohort effects, were present.

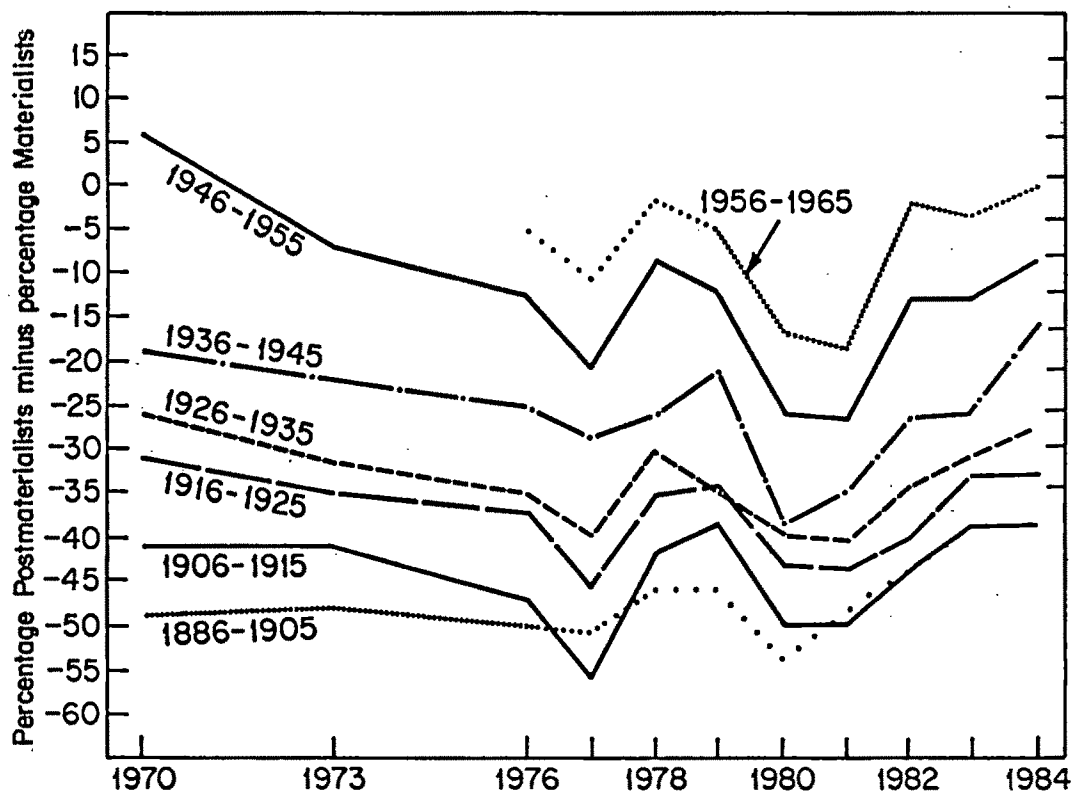
In this ideal-type model, the younger birth cohorts are less materialist than all of the older cohorts at all points in time. Because no period effects are present, each cohort's values remain absolutely unchanged, regardless of any changes in the socioeconomic environment. But because of population replacement, the values prevailing in a given society do change over time. During the 14-year period depicted here, approximately 70% of the 1886-1905 birth cohort died; they were replaced in the adult population by a much more numerous cohort born from 1956 to 1965, a group too young to be included in our samples in 1970, but which constituted a major part of the adult public by 1984. The fact that this youngest and much more postmaterialist cohort replaces the oldest, most materialist one would tend to produce a net shift toward postmaterialism.

But my theory is based on a scarcity hypothesis

as well as a socialization hypothesis: one can expect period effects as well as cohort effects. Figure 3 depicts a pattern of period effects superimposed on stable cohort differences. Although one can observe substantial fluctuations in response to short-term forces in this figure, the cohort effects are fully as strong as those in Figure 2. Consequently, in both cases the process of intergenerational population replacement tends to produce a gradual shift toward postmaterialist values.

Figure 4 depicts a model in which age-group differences exist, but result entirely from life-cycle effects. As a given birth cohort ages it comes to resemble the next older cohort, so that after 10 years have elapsed, a given cohort has shifted to the position held by the cohort that is 10 years older. Although each cohort does change over time, the values of the society as a whole do not change, because population replacement is offset by life-cycle effects. Empirically, life-cycle effects can be distinguished from cohort effects because (aside from short-term fluctuations) in the former, each cohort manifests a downward long-

Figure 5. Value Priorities of Six West European Publics, by Birth Cohort, 1970-1984



Based on representative national samples of the publics of France, Great Britain, West Germany, Italy, The Netherlands and Belgium, interviewed in European Community surveys of 1970 and 1973, plus Euro-Barometers 6 through 21 (total $N = 141,133$).

term trend, whereas in the latter, each cohort remains horizontal, when plotted as in Figures 2-4. Although my theoretical framework does not exclude the possibility of life-cycle effects, it does not predict them; however, it does predict that there will be substantial and durable cohort effects.

Do we find them? To answer this question, let us turn now to a cohort analysis of the Euro-Barometer data gathered in six West European nations from 1970 through 1984. When two surveys were carried out in a single year, I combine them to maximize sample size. Figure 5 shows the results of the combined six-nation sample. Separate analyses for each country are shown in Inglehart (1985); the basic pattern is similar cross nationally.

In Figure 5, each cohort's position at a given time is calculated by subtracting the percentage of materialists in that cohort from the percentage of postmaterialists. Thus, the zero point on the vertical axis reflects a situation in which the two groups are equally numerous (which is approximately where the cohort born in 1946-1955 was located in 1970). An index of -45 would result if, for example, 50% of a given cohort were materialists and only 5% were postmaterialists (with the rest being mixed types): the oldest cohort was located slightly below this point in 1970.

Figure 5 summarizes the results from an immense amount of survey research based on more than 140,000 interviews. It demonstrates rather conclusively that the age-group differences observed in 1970 reflect long-term intergenerational differences rather than life-cycle effects.

Each cohort retains its relative position with striking consistency throughout the 14-year period. The 1946-1955 cohort is less materialistic and more postmaterialistic than any of the older cohorts at every point in time. The only cohort that is even less materialistic is another postwar cohort, born from 1956 to 1965—a group that was too young to be interviewed in 1970, but which becomes an increasingly important part of my sample from 1976 on. As Figure 5 demonstrates, each of the older cohorts proves to be more materialist than all of the younger ones at every time point, with only a few minor anomalies. The intercohort value differences are extremely stable.

Moreover, there is no indication at all that each cohort becomes more materialist as it ages, as would be the case if these differences reflected life-cycle effects. At the end of the 14-year period, virtually all of the cohorts were fully as postmaterialistic as they were in 1970. Indeed, there is something of an upward tendency; most of the cohorts are less materialist in 1984 than they were in 1970. There are also some significant short-term fluctuations; each cohort shows a brief

downward swing in 1977 and again in 1980-1981. These fluctuations reflect period effects that result largely from the impact of inflation, as I demonstrate below. But by 1984, inflation had subsided almost to the 1970 level. With period effects held constant, there is no sign at all of the gradual conversion to materialism that would be present if a life-cycle interpretation were applicable. The pattern reflects intergenerational value change.

These stable intergenerational value differences imply that, other things being equal, we will witness a long-term trend toward postmaterialist values as one generation replaces another. A good deal of intergenerational population replacement had already taken place from 1970 to 1984. During these years, approximately 70% of the 1886-1905 birth cohort died. They were replaced by a much more numerous cohort born after 1955. In 1970 the postwar generation constituted only 20% of the sampling universe (those citizens over 15 years of age); in 1984 it constituted fully 46%.

There has been a corresponding shift in the distribution of value types. In 1970, materialists outnumbered postmaterialists in these six nations by a ratio of 3.4 to 1. In 1984 this ratio had fallen to little more than 2 to 1. Contrary to predictions that postmaterialism would disappear as a result of the economic crisis, the underlying process of intergenerational change continued to function throughout the period, even though its effect was sometimes masked by negative period effects. When short-term forces returned to normal, the results were manifest: a substantial net shift toward postmaterialism had taken place—most of it the result of intergenerational population replacement (see Abramson & Inglehart, 1984).

Perhaps the most striking feature of Figure 5 is the persistence of stable differences between the value priorities of the respective cohorts across a period of 14 years. In all of the years for which data are available, those born in 1956-1965 constitute the most postmaterialist cohort. At all 11 time points, the 1946-1955 cohort ranks second, above all of the older cohorts. At all 11 time points, the 1936-1945 cohort ranks third; at 10 of the 11 time points, the 1926-1935 cohort ranks fourth; at 10 of the 11 time points, the 1916-1925 cohort ranks fifth; and the 1886-1905 cohort ranks last or second to last in all 11 years. Of 75 points depicted in Figure 5, there are only four anomalies, and all of these are minor ones which involve an overlap between two immediately adjacent cohorts and could be caused by samples that deviate by only a few percentage points from the actual values. The pattern is about as close to perfection as one could hope for: the relative positions of the respective cohorts are extremely stable.

The data show a virtually perfect fit with theoretical expectations. As predicted, there are substantial differences between the values of different cohorts and, as predicted, the younger cohorts are consistently less materialist than the older ones. Moreover, as predicted, these differences seem to reflect cohort effects: there is no indication of the long-term downward trend that would be present if one were dealing with life-cycle effects. A given cohort does not become more materialist as it ages. The overall tendency from 1970 to 1984 is horizontal. The respective cohorts are generally at least as postmaterialist in 1984 as they were in 1970, and if there is any net trend at all, it is upward, toward increasing postmaterialism.

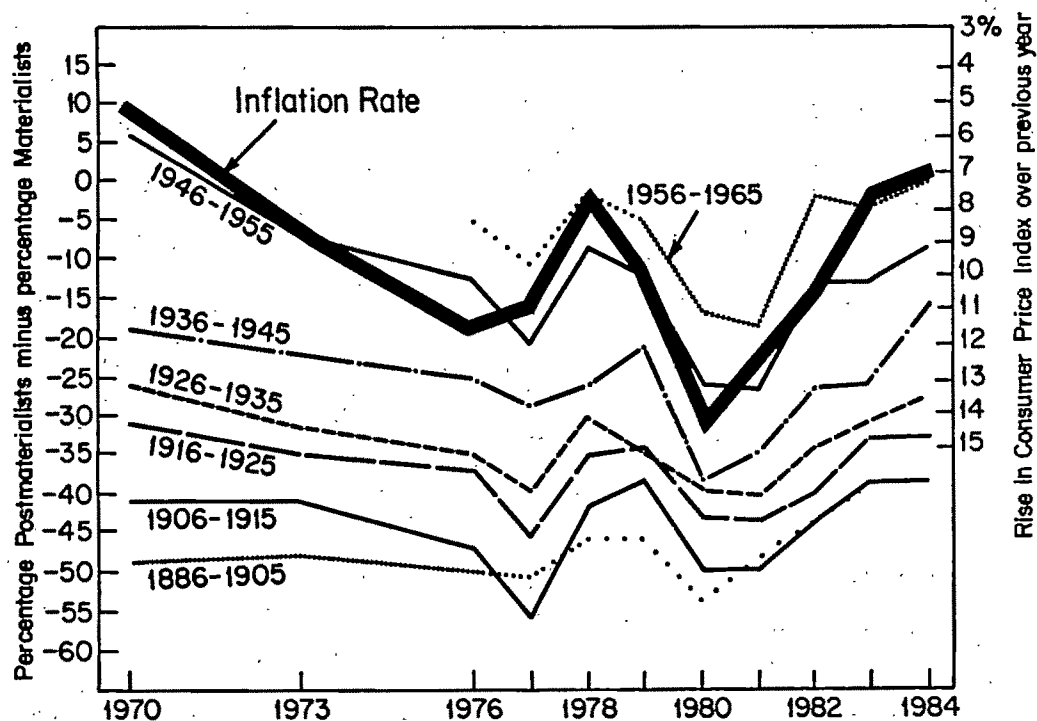
The cohort differences are unmistakable, but there is clear evidence of significant period effects as well, and my interpretation of the cohort analysis will not be complete until I have accounted for them. It is not difficult to do so. When I formulated the battery of items first used to measure materialist-postmaterialist value priorities in 1970, I hypothesized that one's sensitivity to inflation would be a good indicator of materialist priorities. Consequently, one of the two materialist options in the four-item battery

used here is "fighting rising prices." My expectation was that different age cohorts, whose socialization had been shaped by different historical experiences, would respond differently to this item, with the older cohorts more apt to give it a high priority.

When I formulated these items in 1969, I did not anticipate the explosive worldwide inflation that would later take place, following the OPEC oil price shocks of late 1973 and late 1979, respectively. But the logical implications are clear: one would expect such inflation to enhance the chances that a high priority would be given to "fighting rising prices." The theoretically obvious interpretation of the period effects found in Figure 5, then, is that they reflect the two waves of inflation that impinged on Western Europe so dramatically in the mid-1970s and again at the start of the 1980s. Is this interpretation confirmed empirically?

Figure 6, which shows the answer, is identical to Figure 5 except that it shows the mean inflation rate for the six nations from 1970 through 1983. The inflation rate is indicated by the rise in the Consumer Price Index during the year of the survey; high inflation rates are reflected in a downward movement of the inflation line, so that

Figure 6. Value Priorities and Inflation Rates in Six West European Countries, 1970-1984



Both inflation rates and values indices are weighted according to population of given nation.

there is the same polarity for the two sets of variables. In those years for which no survey data are available, the inflation rate is not plotted either.

Again, the fit between data and theory is remarkably good, so good that it is immediately apparent even from simply scanning Figure 6. Each of the two dips toward increased materialism reflects a rise in inflation, and the upward movements of 1978-1979 and 1982-1983-1984 reflect the abatement of inflation, with roughly a one-year lag.

One's impression that the period effects result largely from changes in the inflation rate is confirmed by multiple regression analysis of the data in Figure 6 (see Inglehart, 1985). In this analysis, the dependent variable is the value index for a given cohort in a given year. Inflation is operationalized as the mean change in the Consumer Price Index in these six nations during the year preceding the survey. Life-cycle effects are operationalized as the mean age of a given cohort in a given year; if life-cycle effects are present, each cohort should become more materialistic as it ages. On the other hand, cohort effects theoretically remain constant over time within each birth cohort, so they are operationalized as constants. The oldest cohort is coded as 1 at each time point, the next oldest as 2, and so on, with the youngest cohort coded as 6 at each time point.

Operationalizing the variables in this simple but straightforward way resolves the problem of underidentification that otherwise besets cohort analysis. In the multiple regression analysis based on these variables, the combination of cohort effects plus period effects explains fully 87% of the variance in value priorities, with cohort effects the stronger of the two predictors (the respective regression coefficients are .97 and -.132). Life-cycle effects explain literally no additional variance. One could probably explain a somewhat larger percentage of the variance with a more complex regression model that took account of such other variables as unemployment rates or indexes of industrial production, which might also play a role in explaining the short-term fluctuations. But the combination of cohort effects and inflation rates explains such a high proportion of the variance that it is hardly necessary to develop a more elaborate model. The central question here is whether or not intergenerational differences are a major factor, and the evidence clearly indicates that they are.

These findings have extremely important substantive implications (Inglehart, 1981, 1984) which I will not explore here. What is important for present purposes is that these findings are incompatible with the implications of the Black and White model. Although the stability correlations

of the values index are no higher than that of the item used in Converse's classic study, the empirical pattern observed here could not possibly result from a situation in which a majority of the respondents were giving random answers. Literally random answering would result in equiprobable response distributions, producing a 50:50 split when there are two alternatives. If as much as 80% of the public were giving random answers, they would show a 40:40 split, and even if all of the remaining 20% chose the same option (which is highly unlikely), the most extreme skew possible would be a 60:40 split.

In the present case, pure random answering would produce equal numbers of materialists and postmaterialists, and indeed among our youngest cohorts, postmaterialists are almost as numerous as materialists. But among the cohort born before 1906, the situation is drastically different. In a number of countries, well over 50% of the sample falls into the pure materialist category, whereas only about 4% give postmaterialist responses. By purely random processes, 16.7% would fall into the latter category: the results indicate that no more than 25% of the cohort could be giving random answers, even if one assumes that none of the observed postmaterialist responses among this oldest cohort reflects genuine preferences within the respondent.

In itself, this point is not entirely new. A decade ago, Converse (1974) and Pierce and Rose (1974) had agreed that some observed response patterns could not be attributed to equiprobable random answering (such as would result from rolling unloaded dice); the distributions sometimes displayed skews that could only result from random answering that was not equiprobable (such as would result from rolling loaded dice).

But a crucial question could not be resolved in this debate and remained to be answered: What made the dice loaded?

Pierce and Rose attributed the observed deviations from equiprobable random answering to unmeasured attitudinal predispositions: the low stability correlations did not result from non-attitudes but from imperfect measurement of them. Converse, on the other hand, attributed the skew to methods effects; if the deviations from equiprobable distributions resulted from response set or other artifacts of questionnaire design rather than from underlying preferences within the respondent, the nonattitudes thesis could still apply.

On this point, my cohort analysis provides decisive evidence. The older cohorts display skews that deviate enormously from an equiprobable distribution, and it is impossible to attribute these deviations from pure randomness to any sort of methods effect because both young and old

cohorts were asked precisely the same questions, in the same format, at the same time, and they show drastically different distributions. The differences in responses reflect differences in the respondents, not something in the measuring instrument.

The current evidence is incompatible with Converse's interpretation. The findings *must* result from unmeasured attitudinal predispositions, and the differences in the underlying attitudes of old and young are remarkably enduring. Already, there are direct measurements indicating that these attitudes have persisted throughout a period of 14 years. Further, these findings fit almost perfectly into a theoretical framework which implies that the generational differences can be traced to the persisting impact of formative experiences that took place as much as 50 years ago (Inglehart, 1977). In short, the evidence indicates that underlying the relative fluidity of given attitudinal indicators, the sociopolitical orientations of western publics have components of remarkably great stability.

This stability is by no means limited to the materialist/postmaterialist dimension. Probably the most influential and widely recognized effort to analyze human values with the use of survey methodology has been the work of Rokeach (1968, 1973). The Rokeach terminal values survey was included in both waves of the American panel survey; it enables us to compare these findings

with results from a battery designed to measure the entire spectrum of human values.

Although my items refer to sociopolitical priorities whereas Rokeach's items tend to have more personal referents, the stability of the Rokeach items varies in the same fashion as that of the Inglehart items. Stability coefficients for the 18 items in the Rokeach battery, together with the materialist/postmaterialist index, are shown in Table 1.

As Table 1 demonstrates, materialist/postmaterialist values show a substantially lower stability than the priority one gives to "salvation." Fully 25% of the American panel ranked "salvation" first among the 18 terminal values in 1974, and of those who did so, 69% ranked it first among the 18 items in 1981 as well. Conversely, a relatively large share of the sample (almost 15%) ranked "salvation" last in 1974, and 54% of those who did so ranked it last again in 1981. If one believes in religious salvation, it is the most important thing in the world—one is apt to think about it often, pray, attend church regularly, and consistently give it a very high priority. On the other hand, if one does not believe, it seems altogether useless. Thus, this basic component of one's religious outlook is by far the most stable of Rokeach's 18 terminal values.

However, the other Rokeach values items all have stability coefficients in the same range as my

Table 1. Stability of Values from 1974 to 1981 among American Public

	<i>r^a</i>
Salvation (saved, eternal life)	.68
A comfortable life (a prosperous life)	.44
Mature love (sexual and spiritual intimacy)	.41
National security (protection from attack)	.41
Materialist/postmaterialist values	.39
A world of beauty (beauty of nature and the arts)	.39
Inner harmony (freedom from inner conflict)	.38
Self-respect (self-esteem)	.36
Equality (brotherhood, equal opportunity for all)	.36
Family security (taking care of loved ones)	.35
An exciting life (a stimulating, active life)	.34
Pleasure (an enjoyable, leisurely life)	.34
Social recognition (respect, admiration)	.32
Happiness (contentment)	.30
Freedom (independence, free choice)	.29
True friendship (close companionship)	.29
A world at peace (free of war and conflict)	.28
Wisdom (a mature understanding of life)	.26
A sense of accomplishment (lasting accomplishment)	.26

Source: Two-wave panel survey of American public carried out in summer 1974 and summer 1981 by Survey Research Center, University of Michigan. Panel *N* = 933. Fieldwork and sampling are described in *Political Action* codebook.

^aProduct moment correlations between 1974 and 1981 rankings.

materialist/postmaterialist values index. Indeed, the latter ranks higher than 14 of the 18 terminal values. This does not mean that the Inglehart battery is "better" than most items in the Rokeach battery. On the contrary, it is clear that my values index shows greater stability over time than most of the Rokeach terminal values simply because it is based on multiple indicators and therefore measures the underlying dimension more accurately than any single item does. The stability across time of our individual items is no greater than that found for most of Rokeach's items (and, of course, much lower than that of "salvation"). The problem is not that stable values do not exist, but that it is difficult to measure them. As measurement techniques improve, stability rises.

Deciding one's basic priorities in life is a demanding task for which the only reward is the intrinsic interest of the task itself. Among those who are not interested (at least not at the time of the given survey), some respondents apparently avoid a difficult and unfamiliar job by giving hasty and more or less "random" answers. This process seems to apply to the Rokeach battery as well as to the Inglehart battery. But despite the relatively modest individual-level stability found with most of the Rokeach items, I believe that they too tap basic and deep-rooted values among the American public. If so, one should find con-

siderable stability in the aggregate rankings of these variables.

One can test this expectation against time series data extending over a 13-year period, because the Rokeach terminal values survey has been administered to representative national samples of the American public in 1968, 1971, 1974, and 1981. The overall rankings obtained in each year are shown in Table 2.

The stability we observe is absolutely phenomenal. The six items that were ranked highest in 1981 are identical with the six items ranked highest in 1968, and—amazing as it may seem in view of the social upheavals that took place in this era—not one of the six items varies by more than one rank from its 1981 position in any of the three previous surveys extending across 13 years. The stability of the six lowest-ranking items is almost equally impressive: the six lowest items in 1981 are identical with those in 1968, and most of them do not vary by more than one rank from their 1981 positions in any of the three previous surveys. The public's evaluation of an "exciting life" is relatively volatile: it rises from 18th place in 1968 to 15th place in 1981. By most standards this would be considered an extremely small change. In the present context, it is an exceptionally large one. Although the middle six items show greater volatility than the top or bottom six, they remain within the middle zone almost without exception.

Table 2. Ranking of 18 Personal Values by the American Public, 1968-1981

	1968	1971	1974	1981
A world at peace (free of war and conflict)	1	1	2	2
Family security (taking care of loved ones)	2	2	1	1
Freedom (independence, free choice)	3	3	3	3
Happiness (contentedness)	4	6	5	5
Self-respect (self-esteem)	5	5	4	4
Wisdom (a mature understanding of life)	6	7	6	6
Equality (brotherhood, equal opportunity for all)	7	4	12	12
Salvation (saved, eternal life)	8	9	10	9
A comfortable life (a prosperous life)	9	13	8	8
A sense of accomplishment (lasting contribution)	10	11	7	7
True friendship (close companionship)	11	10	9	10
National security (protection from attack)	12	8	13	11
Inner harmony (freedom from inner conflict)	13	12	11	13
Mature love (sexual and spiritual intimacy)	14	14	14	14
A world of beauty (beauty of nature and the arts)	15	15	15	16
Social recognition (respect, admiration)	16	17	18	18
Pleasure (an enjoyable, leisurely life)	17	16	16	17
An exciting life (a stimulating, active life)	18	18	17	15

Source: Representative national samples interviewed by the National Opinion Research Center, University of Chicago, in 1968 and 1971 (see Rokeach, 1974), and by the Survey Research Center, University of Michigan, in 1974 and 1981.

The overall stability of the Rokeach rankings is extremely impressive. To attribute this stability to sheer chance would strain one's credulity beyond the breaking point. Despite their modest reliability coefficients, the conclusion is inescapable that the Rokeach items tap orientations that are very stably anchored among the public.

Here again, as with the materialist/postmaterialist values items, the distributions are incompatible with the assumption that most of the public is giving random answers. By random processes, each of the 18 goals would be given top priority (or lowest priority) by about 5.5% of the sample. In fact, both "a world at peace" and "family security" are chosen consistently for top priority by about four times as many people as would do so at random. At the other end of the scale, the deviations from a random model are even more extreme: the two combined are ranked last by less than 1%. There is no way in which this could occur if a majority of the sample were answering at random. If only 10% of the sample were answering randomly, these goals would be ranked last by larger numbers than those we actually observe. Again, it would be extremely difficult to attribute these deviations from equiprobability to methods effects. The respondent chose each value from a pool of items printed on adhesive labels and gave it a unique ranking. The usual forms of response set—the tendency to agree, or to disagree, or to give the same answer as one gave to the last question—do not apply here (as was also true of the materialist/postmaterialist battery). Despite the modest continuity correlations, the data manifest a degree of skewed stability that is incompatible with the assumption that nonattitudes are present among more than a small fraction of the sample.

Much of the observed individual-level instability in values apparently reflects error in measurement, rather than the absence of preferences. Because my results are generally contaminated by random answering, the responses to any one item provide an imperfect indicator of the respondent's underlying predispositions. Consequently, our multi-item values index shows greater stability over time than any of its constituent items, because it provides a more accurate measure of the underlying concept. Thus it follows that a still more accurate measure of the materialist/postmaterialist dimension should show still higher stability.

Let us try to obtain a more accurate measurement of this dimension. The materialist/postmaterialist index that was used in the panel analysis thus far simply consists of the unweighted sum of all postmaterialist items given top priority, minus the sum of all materialist items given top priority. This procedure has the virtue of simpli-

city; it is intuitively clear what a given score means. But one should be able to obtain a more accurate measure by weighting each item according to the strength of its correlation with the latent variable (as do factor scores, for example).

An even better measurement can be obtained by using LISREL analysis—a structural equation technique that not only weights each indicator, but also estimates the amount of measurement error present in given indicators and controls for correlated error terms. This is important with my values indicator, since it is based on one's mutually exclusive rankings of items from given groups. With an ipsative battery such as this, the error terms clearly are correlated.

A LISREL analysis of data from the panel study carried out in the United States produces the results shown in Figure 7. In this model, one's rankings of the various items in the materialist/postmaterialist battery are treated as indicators of the latent variable, materialist/postmaterialist values, for the given year in which the items were administered,¹ while one's values in 1974 are treated as an independent variable that accounts for one's values in 1981. This model uses a modified form of the ipsative correction proposed by Jackson and Alwin (1980).²

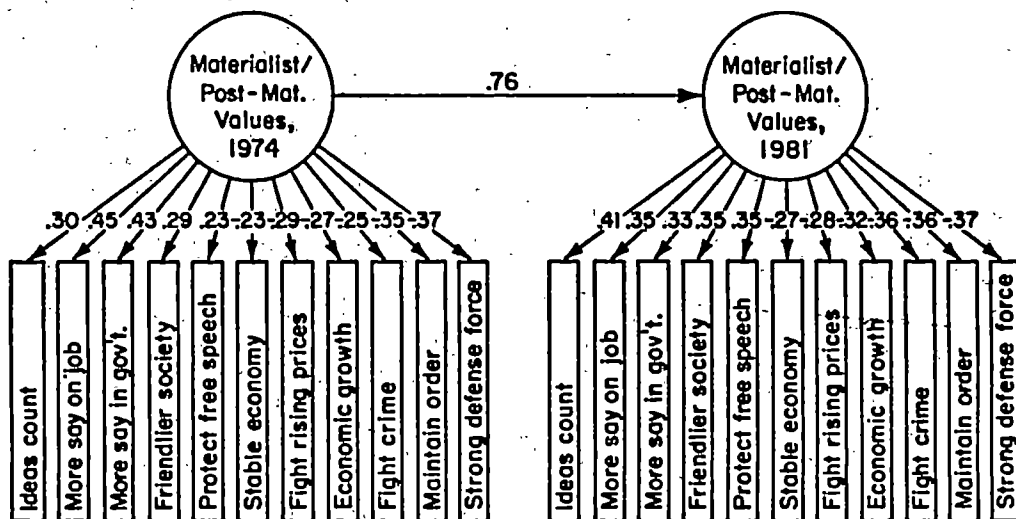
The results of this analysis confirm, once again,

¹It was necessary to omit one item from the battery presented in each year, because otherwise the rankings of any 11 items would fully determine the ranking of the remaining item: hence we have 11 indicators, even though 12 items were presented to the respondent in the interview. The item omitted from this analysis was "more beautiful cities and countryside," which was the weakest indicator of the underlying dimension.

The fact that these items are ranked largely eliminates possible contamination by response set; but it artificially depresses the factor loadings shown in Figure 7. The choice of any given item reduces the likelihood that any other item can be given high priority, which tends to make all items negatively correlated with each other. Thus, the two items with the highest negative correlation tend to define opposite poles of the first factor—but, since they are negatively correlated with both items, none of the other items can attain high loadings on this factor. Despite the seemingly low loadings, when I correct for ipsativity our LISREL model shows a satisfactory goodness of fit.

²The sampling design for the Dearborn panel survey in Figure 7 is described in Appendix A of Stockton and Wayman (1983). The University of Michigan panel was carried out in February and April 1984, with a sample of 26 students enrolled in an undergraduate political science course. Fieldwork for both the four-year panel and the seven-year panel were carried out by the Survey Research Center of the University of Michigan; for details, see ICPSR codebooks for the National Election Studies of 1972 and 1976 and the *Political Action* survey.

Figure 7. The Stability of Values among the American Public from 1974 to 1981.
LISREL Standardized Solution. Chi Square = .086, $df = 197$.



the existence of a materialist/postmaterialist dimension. All six of the indicators designed to tap materialist goals show significant negative correlations with the latent variables measured respectively in 1974 and in 1981, and all of the items designed to tap postmaterialist goals show significant positive correlations with the respective latent variables. There is a good deal of measurement error at the level of the respective indicators. Several of the individual items are relatively poor indicators of the underlying values dimension. Their correlations with the latent materialist/postmaterialist variable range from .23 to .45. Moreover, taken individually, several of these items show little stability over time (with correlations as low as .20).

But the underlying dimension has a remarkably high degree of stability across time; the panel respondents' latent value priorities in 1974 show a .76 correlation with their values in 1981, which indicates that almost 58% of the variance in our respondents' values in 1981 can be attributed to the values these same individuals held in 1974. This finding replicates results obtained by Dalton (1981), who analyzed a two-wave German panel survey. Using methods similar to mine, Dalton found a .70 correlation between materialist/post-materialist values among German youth in 1976 and 1979. That I obtain an even higher correlation in a panel survey extending over seven years rather than three may reflect the fact that I used a somewhat different values battery and a different population from the one studied by Dalton, but it is also an indication of how surprisingly little

these correlations decline over time. The panel I have been analyzing spans seven years, an exceptionally long period. The questions used to measure materialist/postmaterialist priorities have also been asked in a number of other American panels over shorter periods of time. Let us compare the results.

Figure 8 shows the expected and empirically observed correlations between value priorities measured in panels ranging from two months to seven years. The expected values are derived by working backward from the values obtained in the seven-year panel, which shows a continuity correlation in the observed variable of .32 (as measured by the four-item index) and a continuity correlation in the latent variable of .76 (as estimated by LISREL). Given these parameters, a Latent Attitude model implies that one should find a modest but perceptible decline in the continuity correlations over various periods of time, as indicated in Figure 8. A Markov Chain model, of course, implies a much more rapid decline in the correlations for the observed variable; if the observed correlation over two months were .42, it would have declined to zero within less than a year. A Black and White model, on the other hand, implies no decline at all; if the correlation over seven years were .32, it would be .32 over two months or two decades.

The empirically observed correlations fit a Latent Attitude model much better than either of the other two models. Although they are based on different samples instead of one continuous panel, the figures are reasonable.

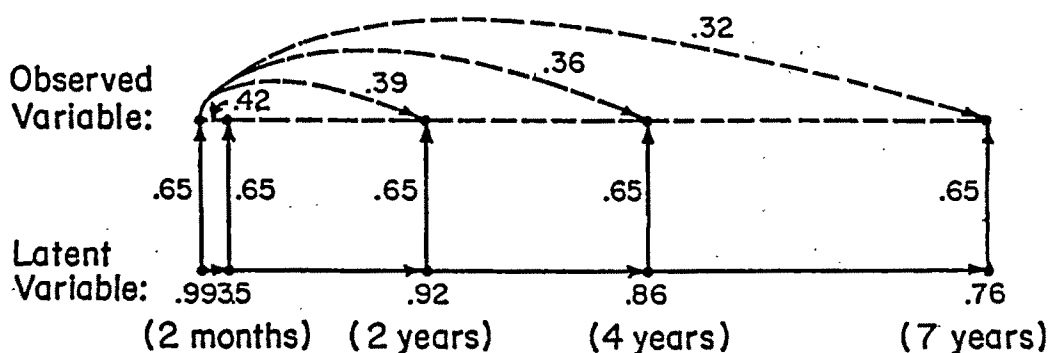
Surprisingly, however, the four-item index shows a weaker correlation over four years than over seven years! This is a result that none of the three models would have predicted. When one examines the details more closely, it seems that this deviation from logical expectations may reflect the problems of time and motivation mentioned above. The four-year panel examined here was carried out in 1972 and 1976. In 1972, these questions appeared fairly early in the interview, but in 1976, they were asked at the end of a long interview, after the respondent had already answered more than 400 questions in an interview that averaged approximately 90 minutes. By this point, respondent fatigue may have become a significant factor, making some respondents eager to get the interview finished. If so, it would tend to increase the rate of random answering. In the seven-year panel, on the other hand, these questions were

asked about seven or eight minutes into the interview in both 1974 and 1981. Here, respondent fatigue would be much less important, resulting in less error in measurement and thus the phenomenon we observe, a higher stability coefficient over seven years than that observed over four years.

Conversely, the stability coefficient observed in the two-month panel is somewhat above the expected value, possibly because we are dealing with a student sample instead of a cross-section of the population, as in the other panels. Those with more education are more likely to discuss politics than those with less education and consequently are likelier to come to the interview with an answer already articulated that reflects their underlying preferences. However, the stability coefficient might also reflect the fact that the two-month panel was carried out in a classroom rather

Figure 8. Expected and Observed Stability of Materialist/Postmaterialist Values over Various Time Intervals

1. Expected correlations for 4-item index
(assuming constant rate of decay in stability of latent variable)



2. Observed correlations:

Time interval:	Survey	Continuity Correlations:	
		4-item index	12-item index
2 months	(University of Michigan student panel, 1984)	.53	.82
2 years	(Darborn panel, Stockton & Wayman, 1974-1976)	.35	—
4 years	(National Election Study panel, 1972-76)	.28	—
7 years	(Political Action panel, 1974-81)	.32	.39

than under typical survey fieldwork conditions. In the classroom, the respondents carry out their rankings in a context that reinforces the idea that the task is important and therefore worth doing carefully. Moreover, the task is performed in a relatively leisurely setting: unlike many other survey respondents, the students are not stealing the time from other activities. Thus, a test-retest experiment carried out in a classroom situation may provide a stronger motivation to answer thoughtfully, with more leisure and fewer distractions present than is true in the usual field interview situation. These factors may account at least partially for the markedly higher stability of results obtained in the classroom situation.

The difference does not seem to result solely from the fact that one is dealing with a more highly educated population. Rokeach (1973, p. 58) reports that the median test-retest reliabilities reported for the 18 terminal values items, administered to samples of 7th and 9th grade students, were about .63 over a three-week period; for college students, they were .78 over three weeks, and (again) .78 over two months. Thus far, little research has compared the stability and constraint of the responses elicited in experimental situations with the stability and constraint obtained in survey situations. The available evidence suggests that one can obtain significantly higher levels of stability and constraint under experimental conditions, even when questions and type of respondents are held constant.

If the apparent instability of survey orientations results in part from lack of motivation and previous practice in making the relevant linkages between underlying preferences and articulated opinions, one would expect to find higher stability of values among the more educated and those most interested in societal problems. We do. In the American seven-year panel, my values index shows stability coefficients of .33 for those who say they are not interested in politics and .40 for those who are very interested. Stability is also greater among the more educated: among those with an elementary school education the coefficient is .19, while among those who attended college, it is .46. Van Deth (1983) reports similar findings from a Dutch panel study of these values.

The panel surveys provide yet another indication that the apparent instability of mass orientations is the result of measurement problems rather than nonattitudes. In two of these surveys, the 12-item materialist/postmaterialist values battery was included, as well as the four-item battery (which is one of its components). The broader-based index shows significantly greater stability over both short and long periods of time, probably because it measures the underlying orientation with greater accuracy. These findings

are consistent with previous results, which demonstrated that the 12-item index also manifests greater constraint with other attitudes than does the four-item index (Inglehart, 1977); both stability and constraint rise as one obtains greater accuracy of measurement.

Moreover it is not only under classroom or experimental conditions that the general public can be motivated to show high levels of constraint in their political attitudes. Stokes (1963) has suggested that under exceptional circumstances, such as a political crisis that evokes intense political polarization, strong ideological focus may be found among mass publics. One such case has been observed in France, in the spring of 1968. In the context of a series of strikes that totally paralyzed France, together with massive demonstrations and the occupation of factories and public buildings, a majority of the French public thought they might be on the brink of civil war. There was an enormous outpouring of political discussion. This situation of extreme politicization gave rise to remarkably high levels of constraint, with correlations as high as .70 being observed among the political attitudes of the French public (Inglehart & Hochstein, 1972).

This was a truly exceptional situation. Although there was a good deal of political activism in the United States at the same time, it was limited to relatively small groups. In France, on the other hand, practically no one was working in May and June 1968. Fully 20% of the sample reported having taken part in protest activities, and virtually everyone was talking about the crisis.

An exceptional situation produced exceptional levels of constraint, as Table 3 demonstrates. Among the American public, observed linkages between one's political party preference and one's stance on current political issues were at their usual modest levels. In France, they were incomparably higher. Among the American public, the mean correlation between voting intention and the five attitudes shown in Table 3 was .29. Among the French public, the mean correlation with five similar attitudes was .59. More recent surveys of the French public show constraint levels closer to the usual pattern, but the evidence suggests that under favorable conditions mass publics can manifest high levels of constraint.

Conclusion

Converse's basic insight still stands; there is a significant random component that must be recognized and dealt with in survey research. But the Black and White model, with its implications of widespread nonattitudes, is no longer tenable.

Table 3. Issue Constraint in France and the United States, 1968

	a
United States	
Favorable rating of President Johnson	.47
Favorable rating of conservatives	-.27
For racial integration of schools	.25
Favorable rating of liberals	.25
Favorable rating of labor unions	.23
France	
Favorable rating of President de Gaulle	.72
Favorable to Gaullist demonstrations	.67
Satisfied with government policy toward students	.54
Favorable to 1968 strikes	-.54
Favorable to labor union demonstrations	-.50

^aCorrelations between political party preference and issue positions.

For the orientations of Western publics show impressive durability, based on latent attitudes that prove to be much more stable than given survey indicators might seem to suggest. These latent attitudes can now be estimated relatively accurately through recent statistical techniques such as LISREL, and the results undermine the credibility of a Black and White model. Interestingly, in a recent analysis of the persistence of political party identification, Converse himself applies a Latent Attitude model (Converse & Markus, 1979).

Moreover, the stability of these latent attitudes does not exist only in the estimates of structural equation models. Its effects are visible when we examine certain kinds of aggregate survey results. Throughout Western society, younger birth cohorts are a great deal more likely to emphasize postmaterialist values than are older cohorts. Direct evidence demonstrates that these differences persisted throughout the period from 1970 to 1984, and cohort analysis indicates that they do not reflect life-cycle effects, but rather intergenerational differences. This fact, combined with other evidence (Inglehart, 1977, 1979), strongly suggests that these differences reflect the persisting impact of distinctive formative experiences that (in the case of the oldest cohort) took place as much as 50 years ago. Similarly, responses to the Rokeach terminal values battery show modest individual-level stability, together with an almost incredibly high aggregate stability, skewed in ways incompatible with assumptions that random answers are being given by anything more than a small fraction of the public.

The conclusion seems inescapable: mass publics have highly stable sociopolitical orientations that survey indicators measure imperfectly. We seem to be dealing with two levels of orientations in

which the responses articulated to given questions do not have a one-to-one relationship with relevant underlying affective orientations. If they have not sorted out these linkages before the survey, some respondents may give random answers to certain questions, especially if they are answering numerous questions in a limited time.

If the correlation between observed survey response and underlying attitude is .7 (roughly the level that applies to the data examined here), then the observed correlation between responses to two such items will be less than half as strong as the correlation between the underlying attitudes. This fact helps explain a pervasive finding that with survey data, relatively "strong" correlations between attitudes generally range from .3 to .45. With aggregate statistical data, the "strong" correlations range from .6 to .9, with the result that models based on economic indicators tend to explain about four times as much variance as those based on survey data. However, when one aggregates survey data to the level of the cohort, as was done above (cf., Dalton, 1977; Inglehart, 1979) or to the level of a given party's electorate (Inglehart, 1984), one explains about as large a percentage of the variance as one does with economic indicators; at the aggregate level, the random noise in survey data cancels itself out.

Thus, in some respects the aggregate results convey a more accurate view of the orientations of individuals than individual-level survey data do. Only by combining insights from the two perspectives can one obtain the full picture. For especially when one deals with topics on which the respondent has not articulated a position before the interview takes place, survey research systematically underestimates both stability and constraint in mass attitudes.

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The Political Bases of Citizen Contacting: A Cross-National Analysis

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This article examines a mode of political participation that frequently has been overlooked—individual efforts to obtain assistance from government officials. Using the seven nation data set of Verba, Nie, and Kim, we develop and empirically evaluate alternative models of citizen contacting. Our first model draws on variations in the distribution of social and economic resources to explain the likelihood of contacting. The second focuses on differences in political ties to locate those most likely to contact government officials. We find greater support for the political ties model. Persons active in political parties and election campaigns are the most likely to engage in citizen contacting. Without political ties, few poor or uneducated persons would ask officials for assistance. We conclude by noting the more general theoretical and normative implications of our study.

Political parties stand at the center of political participation. They form and reflect the preferences conveyed to government officials; they tie individuals into political blocs, maximizing the influence of each person and the group as a whole; they direct the activities of persons seeking to control governing positions and influence government policy. Political parties also are central to the mass mobilization types of political activities (voting, campaigning, demonstrating, and rallying). Finally, a link that has been little appreciated, they are crucial to citizen contacting, a mode of political behavior that differs sharply from other forms of political participation.

Citizen contacting involves individuals who approach government officials or other powerful persons in order to obtain help for themselves, their families, associates, or larger social groups. Private, face-to-face, and almost always designed

to assist a particular and limited set of persons, it is conceptually distinct from other forms of political participation. Citizen contacting does not involve the selection of office holders, does not include public demonstrations of political strength, and does not require the formation and maintenance of ties among claimants, whether through political parties, interest groups, or other political associations. It hardly ever seeks resources for groups larger than neighborhoods. The intimate bonds, sporadic encounters, and narrow demands of citizen contacting set it apart from the more frequently studied forms of political participation associated with mass mobilization, such as elections, rallies, and demonstrations.

Examples of citizen contacting abound. In relatively industrialized countries, a variety of persons may be expected to seek governmental assistance: unemployed workers looking for jobs, veterans with questions about military benefits, social security recipients in search of lost checks, builders seeking government contracts, and property owners angling for tax abatements are obvious instances. In developing nations, the list of contactors would also include peasants looking for access to markets, new city dwellers trying to adjust to their new living environment, and parents trying to keep their sons out of the army or looking for recommendations to help their children into universities.

Cross-national studies point to the ubiquity of citizen contacting as a mode of political participation. Barnes and Kaase (1979) provide evidence of contacting in eight industrial democracies: Austria, Finland, Great Britain, Italy, the Netherlands, Switzerland, the United States, and West Germany. They find that between one-

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fourth and one-third of the respondents in the European democracies and one-half of the American respondents have contacted government officials and other politically powerful persons. In their study, they show that about the same portion of these societies have contacted as have attended political meetings, and that more persons have voted than have engaged in other forms of participation. Verba, Nie, and Kim (1978) have also provided systematic information on contacting in three of these countries, Austria, the Netherlands, and the United States, as well as three others, India, Japan, and Yugoslavia. They found that between one-tenth and one-third of those surveyed engaged in citizen contacting, fewer than the proportion who voted, took part in election campaigns, and who were members of communal associations. Given the results of these and other studies (see, for example, Cornelius, 1978), it is clear that contacting is a form of political participation found everywhere and engaged in by significant portions of each society.

The significance of contacting rests on more than its ubiquity. No matter how ephemeral any one encounter between citizen and government official, taken together the interactions affect the pattern and content of policymaking. The more widespread contacting is within a country, the more time government officials spend responding to narrow demands. The more successful contacting is, the more government policies center around the production of private goods and public goods with very limited sets of beneficiaries. Studies of urban politics in the United States have emphasized especially the relationship between the delivery of government services and citizen contacting (Crenson, 1983; Eisinger, 1976; Jones, 1981; Jones, Greenberg, Kaufman, & Drew, 1977). Analyses of national politics have shown that responding to the casework demands—contacting's label as seen from Congress—takes up increasing portions of legislators' activities (Fiorina, 1977, 1982).

The structural implications of contacting increase further where the encounters are elements of ongoing relationships between citizens and public officials, as in the case of political clienteles and political machines (Schmidt, Scott, Lande, & Guasti, 1977; Zuckerman, 1979). Here the effort to obtain government resources is part of an established exchange relationship that frequently serves as the basis of party ties and other political activities. In these circumstances, contacting becomes a primary mode of affecting policy decisions, and success in contacting demands membership in a political clientele. To the extent that political machines and clienteles abound in a polity, government policy is likely to be particularistic. Evidence from local and national politics

in India, Italy, and Japan provide much support for these hypotheses (Di Palma, 1977; Nicholson, 1978; Zuckerman, 1979).

These patterns raise several analytical questions: What accounts for the cross-national and within-nation variations in contacting? To what extent do factors that explain voting, demonstrating, rioting, and other forms of political mobilization also account for contacting? Are the economically well-off and the educated especially likely to seek government resources? Does activity in a political party increase the likelihood of contacting? Does the type of party or party system affect citizen contacting?

In this article we explore the social and political correlates of contacting by examining two analytical models. The first draws on variations in the distribution of social and economic resources to explain the likelihood of contacting. It takes its theoretical strength from the ability of hypotheses using these variables to explain respectable—if not especially high—proportions of the variations in other forms of political participation (See, for example, Barnes & Kaase, 1979; Brody, 1979; Milbrath & Goel, 1977; Nie, Powell, & Prewitt, 1969; Verba, Nie, & Kim, 1978). The second approach views political connections and obligations as distinct from social and economic resources and draws on variations in ties to political parties to explain incidences of contacting. We test these arguments with data from Verba et al.'s (1978) study of political participation. The diverse set of cases enables us to use a different systems logic of comparison and thus strengthens the generalizability of our results. We find that persons active in political parties and election campaigns, that is, those with political ties to those who can help and the political obligations to help others, are most likely to engage in citizen contacting. We use these findings to draw more general theoretical and normative conclusions.

Alternative Models of Contacting

The most widely used approach to the explanation of contacting relates this form of political participation directly to an individual's social and economic resources. Although Verba et al. partially remove contacting from their analysis, the logic of their argument implies an explanation of this form of political participation as well. Their model contends that there is a direct, strong relationship between the level of social and economic resources and political participation, except where political institutions intervene. Under what conditions do political institutions affect that relationship? They do so where political participation is tied to intergroup conflict and where the costs of individual participation are low. Thus, Verba et

al. predict and find a strong relationship between membership in and psychological identity with political parties and political participation obviating the effect of socioeconomic level with regard to voting, a weaker one with campaign activities, and weakest of all for participation in communal associations. The conditions associated with contacting are the reverse of those associated with voting: high personal cost and low intergroup conflict. Hence, the Verba et al. model implies that citizen contacting is a direct function of socioeconomic level. The higher the level of social and economic resources, the more likely a person is to approach a politically powerful individual for assistance. A high level of social and economic resources is associated with high "issue neutral motivations" to participate: efficacy, interest, and obligation. Hence, political institutions will have no impact on the level of contacting, when controlling for level of socioeconomic resources (Verba, 1978). Their model makes no distinction between the influence of social and economic resources on contacting for oneself and one's family and for broader social groups. Hence, the strong and linear relationship between social and economic resource level and contacting applies to all forms of contacting.

Most studies of this form of political participation omit the political institutions and personal incentives that might encourage citizen contacting. Indeed, Jones's (1981) work on Chicago is the only analysis of contacting to use a characteristic of the political structure as an explanatory variable. Jones demonstrates that the probability that individuals in a neighborhood will engage in contacting is directly affected by the strength of the Democratic party in the area. Because Jones views the party structure of Chicago as atypical, he denies that the conclusions derived from this analysis may be extended to other cases (p. 699). An alternative explanatory model of contacting would argue that, far from being atypical, the pattern found in Chicago exemplifies a widespread association between political parties and other political institutions and contacting.

Furthermore, the approaches drawing on social and economic variables take no account of whether the person considering contacting believes it likely that he will succeed in affecting the political influential or whether the potential contactor has personal incentives or political obligations to help others. They omit the likelihood that some individuals will be singled out for help in contacting the government. They ignore too the widespread common sense wisdom that personal political connections and ties bring people to knock on the doors of the powerful, not need or high social and economic resources.

Our approach draws on the asymmetric dis-

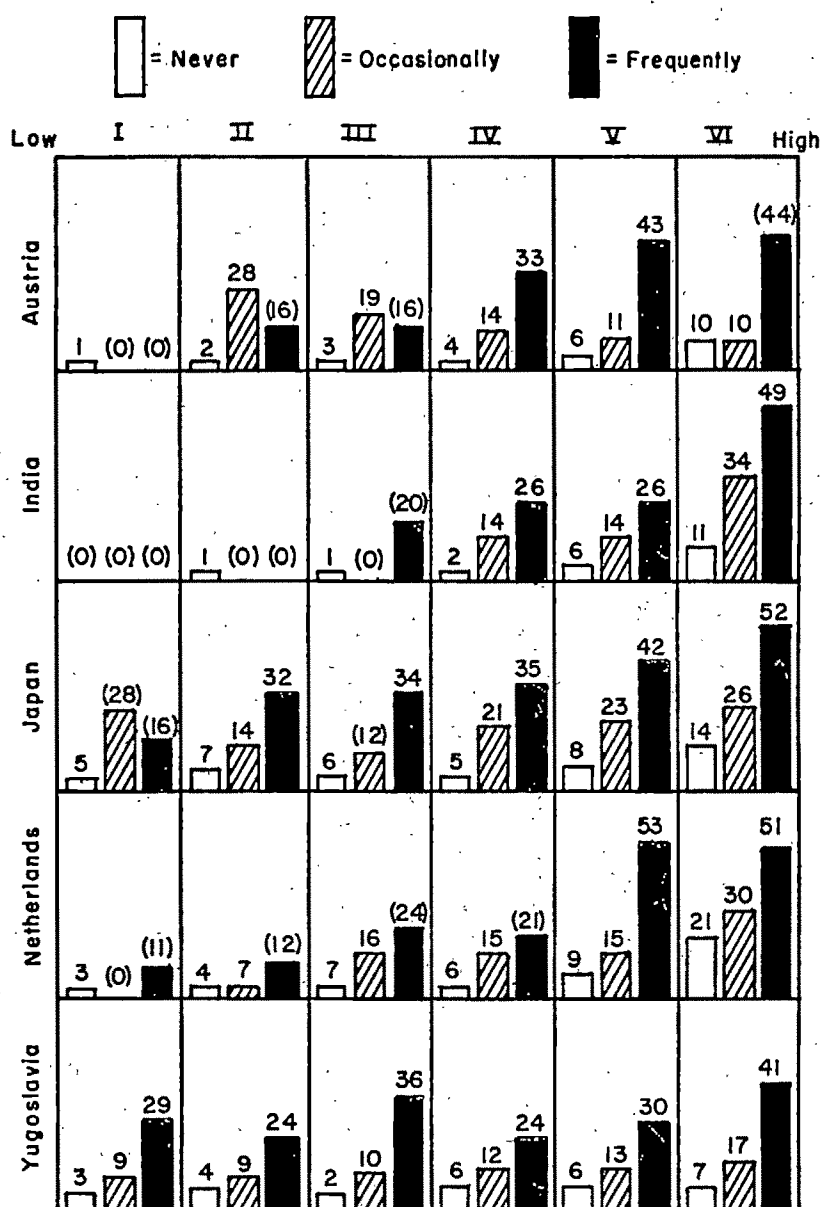
tribution of political contacts and obligations present in societies. Some individuals perceive it likely—and are so perceived by others—that their contacts with powerful persons will bear fruit. In peasant societies, landlords, lawyers, physicians, and school teachers fit this role. More generally, anyone with a direct personal bond to government officials qualifies. In addition, some individuals occupy roles and positions that obligate them to help others. Local campaign and party activists, whether ward heelers or local patrons in parties organized around political machines or others searching for votes, may be classified here. Two elements drive this approach to the analysis of contacting: personal ties to the government official which obligate the politically powerful person to the contactor, and political career or role responsibilities, which require the incumbents to contact power holders. This perspective views contacting as the result of a relationship, of an existing political bond, not a personal characteristic, such as education, income, efficacy, or need. It implies a set of alternative hypotheses that explain the likelihood of contacting: there is a direct relationship between political ties and contacting; the greater the number and strength of political ties and obligations that a person has, the more likely he is to approach a government official to help himself or others; and this relationship holds at all social and economic levels. Party activists are especially likely to engage in contacting because they have the political resources that induce positive responses from office holders. In addition, there is reason to argue that political ties have a greater impact on contacting to help people outside the immediate family. Political activists have the political connections that generally smooth the way to power holders and increase the probability of success. They also know that using those connections to help friends, neighbors, workmates, and others is likely to rebound to their own and their political associates' benefit and that not engaging in social contacting is likely to harm them. Hence, political ties will have a greater effect on the likelihood of contacting to help friends, neighbors, and other social groups than to assist the contactor himself or his family.

Data and Method

In order to test these models of citizen contacting in Nigeria, we did not analyze that country.) study conducted by Verba et al.¹ These surveys contain a wealth of information about contacting

¹For details about sample size and questionnaires in each country, see Verba and Nie (1972) and Verba, Nie, and Kim (1978).

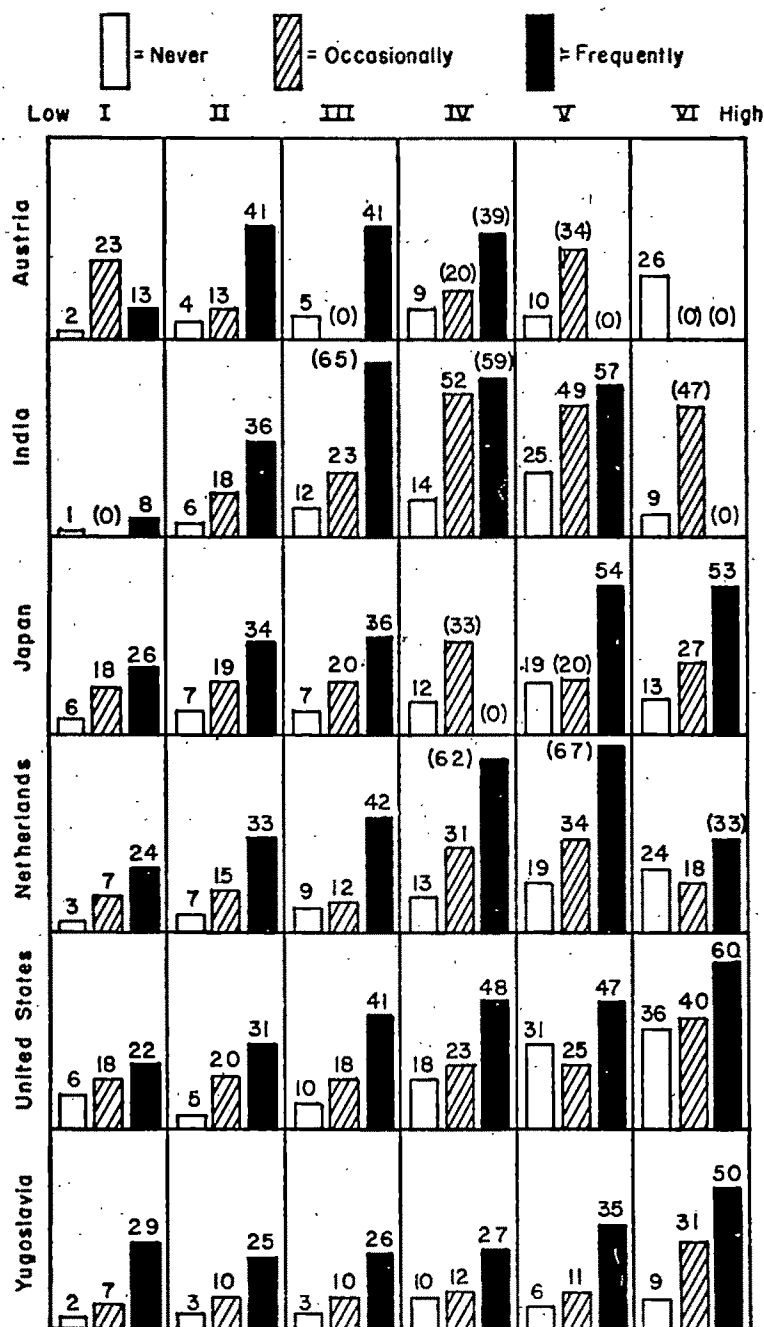
Figure 1. Proportion Who Do Social Contacting in Each Level of Socioeconomic Resources, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socioeconomic levels: Austria (II-VI), India (III-VI), Japan (I-VI), Netherlands (III-VI), and Yugoslavia (I-VI).

Figure 2. Proportion Who Do Social Contacting in Each Level of Education, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socioeconomic levels: Austria (I-III), India (II-IV), Japan (I-III, V, VI), Netherlands (I-IV), United States (I-V) and Yugoslavia (I-VI).

as well as other forms of participation. They permit us to distinguish among kinds of contacting and to relate them to activities of mass political mobilization. We investigated contacting in six of the countries included in the seven-nation study: Austria, India, Japan, Netherlands, United States, and Yugoslavia. (Owing to survey and data limitations and the paucity of citizen contacting in Nigeria, we did not analyze that country.) The political, economic, social, and cultural diversity of these six countries strengthens the theoretical power of the conclusions of our analysis.

Our study examines two dependent variables: particularized contacting and social contacting. The variables are derived from answers to survey questions about whether or not the respondent has contacted local or extralocal government officials or other powerful persons for help in addressing the needs of the respondent, his family, friends, neighborhood, workmates, community, or other social group, during the preceding two years. Particularized contacting combines local and extralocal efforts to help the contactor and his family. Social contacting refers to those who seek assistance for friends and others, whether locally or outside the local community. The labels particularized and social connote the nature of the beneficiaries, not the characteristics of the resources sought. The data show that both particularized and social contacting seek benefits for a distinct and narrow set of persons.

We have provided two sets of operationalizations of the key social and economic variables. The first is the measure of socioeconomic status that Verba et al. use in five of the six countries examined (only in the United States is this measure lacking). It ranks individuals by a combined measure of income and education that distinguishes six levels on a cross-culturally equivalent scale. The second measures the level of formal education completed, ranging from no schooling to university completion. This scale provides a measure for each society that may be applied to all six cases. In both operationalizations, the scales vary from the lowest to the highest.

The operationalization of political ties proved more difficult. The diverse set of political systems examined—a one-party Communist system, two multiparty democracies, two dominant party democracies, and a two-party democracy—introduced one source of problems. There are few indicators that both provide manageable tasks for survey analysis across the data sets and tap comparable phenomena in the different cases. The most intuitively plausible indicator, party membership, had to be excluded because the American and Japanese surveys contain no infor-

mation about it. Factors that locate persons with close personal ties to powerful individuals, such as those of family or friendship, were excluded because they amount to a very small portion of any society. Our measure examines the participation of individuals in the work of political parties. In the plural party systems, it examines activities in electoral campaigns by tapping the answers to the following questions: "Have you worked (frequently, occasionally, or never) in an election campaign in the last three years, such as distributing leaflets or playing an active role in campaign rallies?" In the case of Yugoslavia, we ascertained whether or not the respondent has been involved in the nominating procedures used to select government officials. The various measures tap the same underlying dimension of political connections and denote as well significant portions of each sample.

Our analysis relies primarily on a cross-tabulation technique which presents our findings in a clear and straightforward manner. It is especially appropriate because of the dichotomous nature of our contacting measures and because relatively few people engage in contacting. However, to strengthen our claims, we also utilized a multivariate approach. Those results, reported later in this article, generally matched our cross-tabulation findings.

The Analysis of Contacting

Our analysis begins by exploring contacting across the six levels of socioeconomic status. We first cross-tabulated socioeconomic resource level and social contacting. Although the magnitude of the relationship varies cross-nationally, the results show a direct relationship between the distribution of social and economic resources and the incidence of social contacting. These patterns are especially striking in the Netherlands, where the level of social contacting varies from 3% for those in the lowest socioeconomic categories to 25% for those in the highest, and in India, where the level varies from 0 to 16%. The relationship also holds in the other countries: Austria (from 1 to 11%); Japan (from 7 to 20%), and Yugoslavia (from 7 to 18%). In the United States, the comparison across income groups shows a similar relationship: 5% of those earning less than \$1000 per year have done social contacting, whereas 40% of those earning \$25,000 or more per year have engaged in this form of political participation. In all cases, the higher the level of socioeconomic resources, the greater the amount of social contacting.

In Table 2, we present the effect on social contacting of an alternative measure of socioeconomic status, education. Again, the contrasts be-

Table 1. Incidence of Social Contacting at Six Socioeconomic Levels (%)^a

	Low 1	2	3	4	5	High 6	Mean	N
Austria	1	4	4	7	9	11	6	1744
India	0	1	1	4	7	16	5	2554
Japan	7	11	10	12	15	20	13	2620
Netherlands	3	4	9	9	13	25	10	1526
Yugoslavia	7	8	10	11	14	18	12	2312

^aCell differences are statistically significant at the .05 level.

tween those at low and high levels are striking. The strength of the relationship varies from a six-fold increase (Netherlands and United States), to an eight-fold (Austria and Yugoslavia), and to a thirteen-fold (India) growth across educational levels. The highly educated are much more likely to contact government officials than are those with low levels of education. Furthermore, it is not just high education that adds to the likelihood of social contacting. Increases in social contacting occur with each additional level, high school, college, and postgraduate education.

There are various features of social contacting that might account for these relationships. By its very nature, this mode of participation involves awareness of others and their problems. Contacting government officials on behalf of friends, neighbors, and workmates presupposes interest in or familiarity with the plight of those in one's larger community. High levels of wealth, income, and education presumably increase both interest and familiarity, which in turn are likely to facilitate social contacting. In addition, socioeconomic status and education probably equip contactors with the financial and intellectual resources needed to request assistance from government officials.

The patterns between these explanatory variables and particularized contacting are much weaker and less clearcut. A comparison of Tables 3 and 4 shows that socioeconomic status and education have weak links to contacting for one's self and family. In Table 3, increasing levels of socioeconomic resources are associated with strongly increased particularized contacting only in India (from 7 to 28%) and do so to a much lesser extent in Austria (from 17 to 22%). In the other countries, the results show either no difference across socioeconomic levels or slight drops among those respondents at the highest categories. Table 4 documents similar patterns for educational groupings. India shows an increase from 9 to 32% across the levels of education. In none of the other countries is there a direct relationship. Particularized contacting is much less influenced by increasing amounts of income, wealth, and education than is social contacting.

What is especially striking about these results is the absence of any systematic patterns concerning particularized contacting. In the Netherlands, the relationship between the explanatory variables and contacting for self and family is best described by a flat line; in India, it rises directly with increasing levels of income, wealth, and educa-

Table 2. Incidence of Social Contacting at Six Levels of Education (%)^a

	Low 1	2	3	4	5	High 6	Mean
Austria	3	7	7	13	12	25	6
India	1	8 ^a	17	23	32	13	5
Japan	9 ^a	12	13	14	26	20	13
Netherlands	4	9	11	21	25	23 ^a	10
United States	11	15	21	30	37	51	20 ^b
Yugoslavia	4	8	9	14	14	30	11

^aCell differences are statistically significant at the .05 level.

^aThis cell merges two levels.

^bThe sample size is 2524.

Table 3. Incidence of particularized Contacting at Six Socioeconomic Levels (%)

	Low 1	2	3	4	5	High 6	Mean
Austria	17	22	22	21	18	22	20
India*	7	10	10	17	18	28	15
Japan	7	8	9	10	8	10	8
Netherlands	36	33	33	36	32	34	34
Yugoslavia*	18	24	22	25	22	14	21

*Cell differences are statistically significant at the .05 level.

tion. In the other countries, the two measures have different impacts on the dependent variable: sometimes a parabolic effect appears, sometimes a direct relationship, and sometimes no relationship at all.

This evidence does not support two widely cited hypotheses. Particularized contacting is not a function of the interaction between the need for government assistance and the possession of information on how to obtain that help (Brody, 1979; Jones et al., 1977). The expected parabolic curve, locating those most likely to engage in this mode of contacting at the middle levels of income, wealth, and education, appears only in Yugoslavia. In addition, there is little support for the claim that particularized contacting relates directly to level of socioeconomic resources or need (Sharp, 1982; Thomas, 1982).

These results may reflect the parochial nature of particularized contacting. Requests for government assistance on behalf of one's self or family are especially likely to reflect idiosyncratic needs, and this form of contacting may reflect the essentially random nature of these incentives. It may also respond to the relative ease of contacting government officials and powerful persons. The well-developed Dutch welfare state makes seeking government aid equally easy for everyone; those at the lowest levels of income, wealth, and formal

education do particularized contacting more frequently than those in all the categories of the other political systems. By itself, need has no influence on the likelihood of seeking government aid for one's self and one's family. No group has a more generalized need for assistance than the poor and uneducated in India, yet no group is less likely to engage in particularized contacting. Both points reinforce the claims that this mode of contacting is directly affected by the idiosyncratic distribution of personal needs and the extent to which political institutions foster particularized contacting.

In Tables 5 and 6, we cross-tabulate political campaign work and the two contacting variables. Table 5 reports a strong relationship between the frequency of campaign work and social contacting. In all political systems, those who frequently engage in this form of political participation are much more likely to contact government officials for help than those who are not politically active. For example, only 5% of the Austrians who have never campaigned contacted officials on behalf of others, whereas 32% of the frequent campaigners did. Similarly, 3% of the Indians who are politically inactive contacted officials, whereas 34% of the actives did. This pattern is repeated in all six countries, which leads to the striking conclusion that one-third of those who frequently

Table 4. Incidence of Particularized Contacting at Six Levels of Education (%)

	Low 1	2	3	4	5	High 6	Mean
Austria	20	18	23	24	25	12	20
India*	9	23 ^a	29	37	35	32	15
Japan	5	8	9	10	9	11	8
Netherlands	33	33	36	36	35	37	34
United States*	8 ^b	9	12	13	14	10	10
Yugoslavia*	16	21	25	22	16	18	21

*Cell differences are statistically significant at the .05 level.

^aThis cell merges three levels of education.

^bThis cell merges two levels of education.

Table 5. Incidence of Social Contacting at Three Levels of Campaign Work (%)*

	Never	Occasionally	Frequently	Mean
Austria	5	14	32	6
India	3	22	34	5
Japan	7	20	37	13
Netherlands	8	16	37	11
United States	9	20	39	20
Yugoslavia	5	12	31	11

*Cell differences are statistically significant at the .05 level.

perform campaign work also engage in social contacting. Political activities strongly increase the propensity for social contacting in all political systems.

This relationship holds for particularized contacting as well, although the magnitude is not as strong. Table 6 shows a clear linear relationship between political contribution and particularized contacting. In India, only 13% of the inactives contact government officials for themselves and their families, compared to 47% of those active in election campaigns. In the United States and Japan, the likelihood of particularized contacting doubles (from 8 to 15% and from 6 to 17%) as one moves along the measure of campaign activity. Because this form of contacting generally responds to idiosyncratic circumstances and is likely, therefore, to have a random quality, the link between political networking and particularized contacting is especially striking.

Political ties facilitate contacting in different ways. Political activities help the formation of connections that open political doors in all political systems. They also increase a person's sense that contacting will succeed in remedying social or family problems. Finally, they are likely to increase the incentives to contact government officials by lowering the costs of the activities and raising the direct personal benefits. Political networking is important both for social and particularized contacting.

The evidence we have presented to this point describes two-way cross-tabulations. We have found that variations in socioeconomic resources are related to social but not particularized contacting and that political networking is related to both types of contacting. Important questions remain: Are political connections equally important for all people? More precisely, are they as important for those with high levels as for those with low levels of income, wealth, and education? How will the relationship between socioeconomic resources and contacting be affected when one controls for political work? Will the original patterns remain?

In order to address these questions, we cross-tabulated the measures of socioeconomic resources, education, political work, and the two kinds of contacting. Figures 1 and 2 show three-way cross-tabulations for social contacting, with several interesting results: First, political work consistently exercises a powerful impact on this form of contacting. In all political systems there is a strong relationship between the frequency of engaging in campaign activities and the likelihood of social contacting. Second, these effects are present in almost every category. Third, the boost provided by political activities is especially critical for individuals at low levels of socioeconomic resources. Without political activities, their contacting would be minimal (less than 5%). Fourth, there are interactive effects between political work

Table 6. Incidence of Particularized Contacting at Three Levels of Campaign Work (%)*

	Never	Occasionally	Frequently	Mean
Austria	20	28	28	20
India	13	41	47	15
Japan	6	13	17	8
Netherlands	33	35	49	34
United States	8	11	15	10
Yugoslavia	16	27	28	21

*Cell differences are statistically significant at the .05 level.

and level of socioeconomic resources and education. Persons most likely to engage in contacting are those who are politically active *and* financially well-off or well-educated. Approximately half these people engage in social contacting. These patterns raise important normative issues, which we discuss in the conclusion.

The powerful effect of campaign activities on social contacting at various levels of socioeconomic resources and education is demonstrated even more clearly in the analysis of social contacting at the local level, the most frequent locus of this mode of political participation.² Several patterns are consistently present: Persons who have the lowest levels of education and who have taken part in campaign work are more likely to contact than are those at the highest levels of education but with no campaign experience. Those at the lowest socioeconomic levels who have campaigned contact more frequently than those at the highest levels who have not done election work. In those categories of socioeconomic resources and education with the highest rates of contacting, adding campaign work strongly increases the frequency of contacting.

Some examples from the data illustrate these generalizations. In the United States, almost everyone who engages in social contacting at the local level is a campaign worker. In the two income categories most likely to engage in social contacting, 30% do so. Every contactor in these categories has worked in an election campaign. In the income category containing the next highest level of this form of political participation, 92% have worked in elections. In the United States, 82% of the persons who contacted local officials and powerful persons to help their friends, neighbors, workmates, or other social groups have taken part in election campaigns. In this political system, electoral connections appear to be a necessary condition for social contacting at the local level.

The effect of education on the likelihood of social contacting at the local level is most striking in India. Here, 65% (1642 respondents) of the sample were illiterate. Of these, only one person has engaged in local contacting for a social group, and he has also worked in an election campaign. Among Indians with some formal education, however, political work has a strong influence on the likelihood of social contacting. In the education categories containing the highest proportion of social contactors, adding campaign work doubles the frequency of contacting to 50%.

Among those who actually have engaged in social contacting, nearly 90% were campaign workers.

The analysis of social contacting at the local level demonstrates the powerful impact of political work across all categories of socioeconomic and economic resources. In this most frequent form of social contacting, political contacts open doors and provide the incentives for this kind of political participation.

Figures 3 and 4 present three-way cross-tabulations for particularized contacting. Again, the effect of political work here is not as strong as it is on social contacting. Political work has an effect in some political systems (India, Japan, and the Netherlands) and on many socioeconomic groupings (usually those at the lowest levels). There is some limited evidence of a parabolic effect in India, where the middle categories have the highest rates, even when controlling for political work. There is no evidence of a parabolic effect, however, in any of the other countries. Political work has the greatest impact on particularized contacting of all the explanatory variables that we have examined.

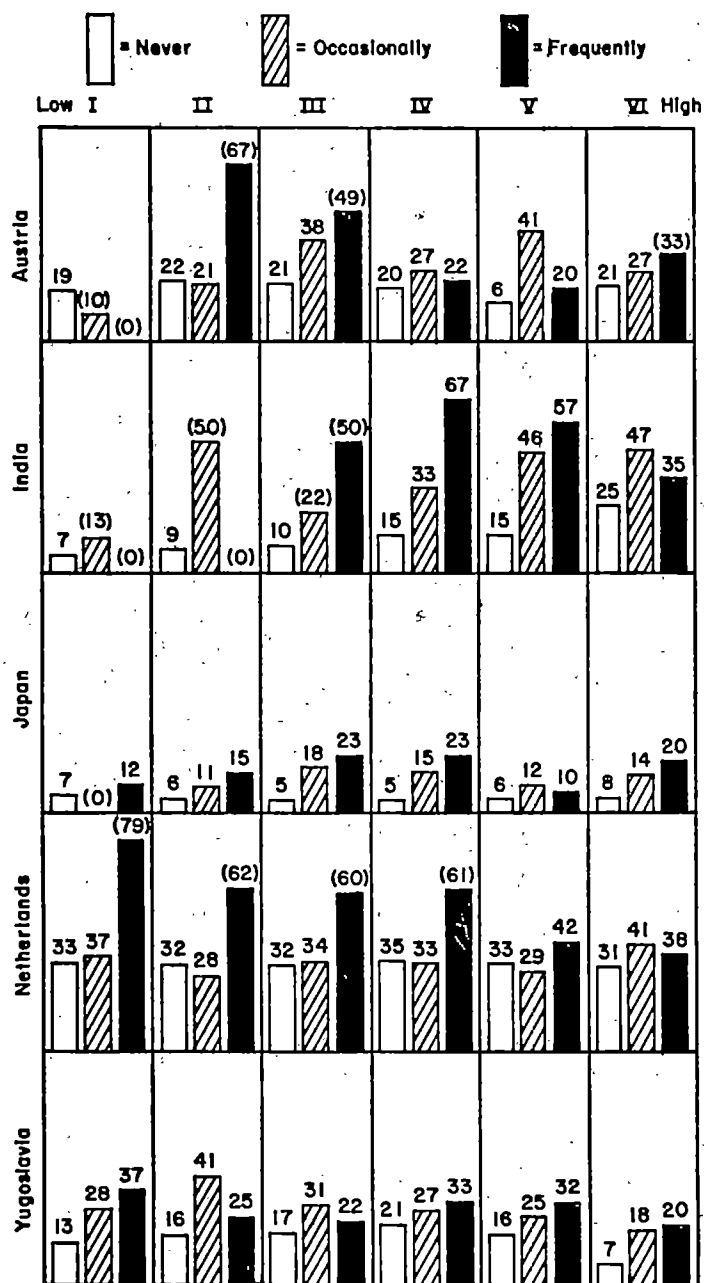
In addition, the power of the political variables is even stronger in the analysis of local contacting. Here too, those at the lowest socioeconomic levels who have taken part in election campaigns are more likely to contact local government officials for themselves and their families than are those at the highest levels, but who have no campaign experience. Similarly, those with the least formal education who have campaign experience are more likely to engage in contacting than those at the highest levels of education who have no campaign experience. The frequency of contacting among persons in those categories of wealth, income, and education most likely to contact is greater where campaign activity is greater.

Finally, we conducted a regression analysis of contacting patterns to determine whether or not the earlier patterns held true.³ We used particularized and social contacting as dependent variables and evaluated the relative influences of socioeconomic resource level, education, and campaign activity. Three conclusions stand out. First, like others who have studied citizen contacting (Barnes & Kaase, 1979; Nie et al., 1978), we found that our independent variables explained a relatively small percentage of the variance in contacting activity. Although there was variation from country to country, the general range was from 3 to 15% of the variance. Second, our model generally worked better as explanations of social than particularized contacting. This finding is

²The Yugoslav data do not distinguish between local and extralocal contacting.

³We also experimented with a log-linear approach and found similar results.

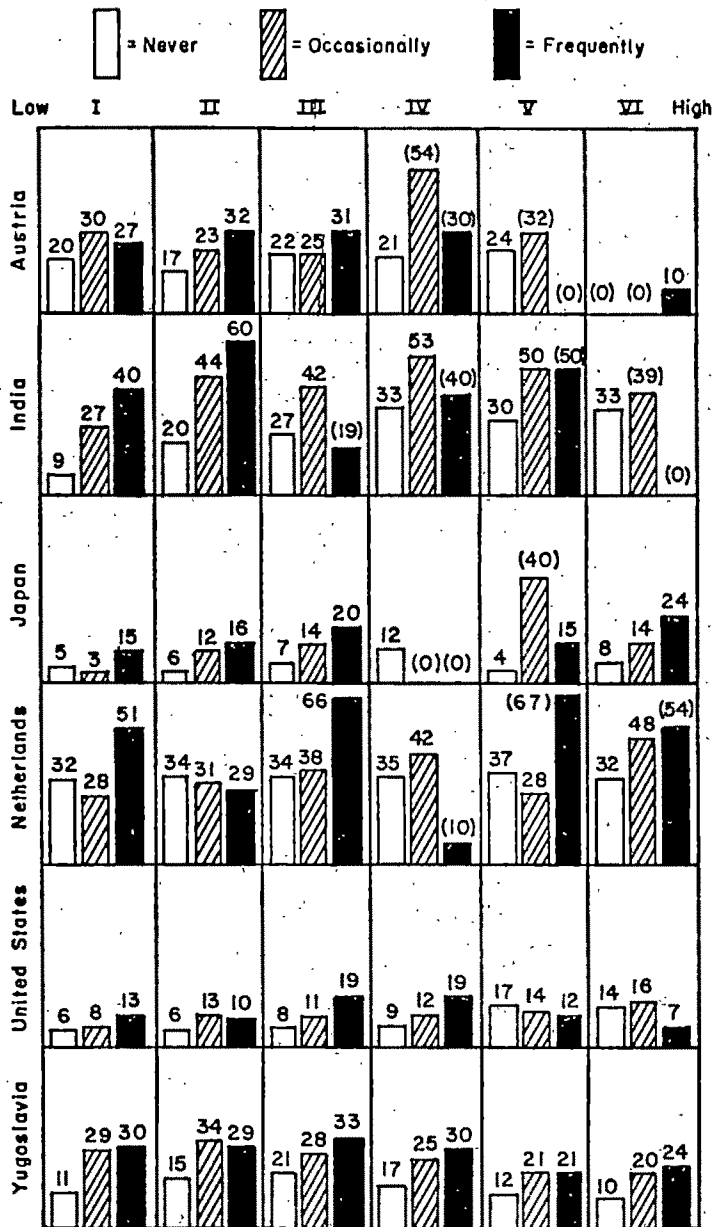
Figure 3. Proportion Who Do Particularized Contacting in Each Level of Socioeconomic Resources, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socioeconomic levels: Austria (II-V), India (II-VI), Japan (II-III, IV, VI), Netherlands (I) and Yugoslavia (I-VI).

Figure 4. Proportion Who Do Particularized Contacting in Each Level of Education, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance at the .05 level for the following socioeconomic levels: India (I, II), Japan (II, III, V), Netherlands (III), United States (II, III), and Yugoslavia (I-III).

consistent with our cross-tabulation results and provides additional evidence of the idiosyncratic nature of particularized contacting. Third; the regression results confirm the greater explanatory power of political over socioeconomic variables. Participation in political activities such as campaign work accounted for almost three-quarters of the variance we were able to explain. The only exception to this pattern was India, where the powerful limiting effect of illiteracy made education account for the largest share of the variance.

In sum, our analysis indicates that the political networking model helps to explain variations in the level of contacting. Political activities, and the advantages they bring (connections, interest, and a sense of efficacy), facilitate citizen contacting of government officials. People who are politically active are much more likely to contact officials than those who are not active. Although there is some variation from country to country, these patterns appear consistently in a diverse set of political systems and under a variety of methodological approaches.

Political Networking: A Party Link

Our measure of political ties focuses on campaign activities without directly exploring the impact of political parties, the institution most involved in election campaigns. The absence of a measure of party membership in all six countries necessitated this less-than-completely satisfactory operationalization. Does including a direct measure of party ties increase the effect of political networks on social and particularized contacting? The data for India, Austria, Yugoslavia, and the Netherlands provide information on party membership that shows that campaign activists who are party members are still more likely to engage in both forms of contacting than those campaigners who are not tied to a political party. The relationship is especially strong, once again, with regard to social contacting.

Table 7 reports the variable impact of party and campaign ties on social contacting. The pattern is the same in all the cases. Approximately half the persons who are party members and frequent campaigners do social contacting. Each party and campaign factor has an added impact on this mode of contacting. In the Netherlands, party members who are frequent campaigners are five times more likely than other citizens to engage in social contacting; in Yugoslavia, they are nearly four times more likely; in Austria, they are seven times more likely, and in India, they are nearly ten times more likely.

In every country we studied, nearly half of all the people who do social contacting are party members, campaigners, or both. In the Netherlands, persons with these political ties are exactly 50% of the social contactors (26% of the sample); in Yugoslavia, they are 83% (49% of the total); in Austria, they are 51% (30% of the sample), and in India, they are 45% of those who do social contacting (and 5% of the total). The patterns are simple, clear, and consistent; there is a strong, direct relationship between political ties and social contacting.

The effect of party ties and political connections on particularized contacting is present, but not as strong. Table 8 indicates that in the Netherlands and India, half the party members who are frequent campaigners engage in particularized contacting. The proportion is considerably lower in Yugoslavia and Austria. In India and the Netherlands, these political ties raise the probability of particularized contacting to well above the national mean. The proportion that party members and campaigners represent among those who do this form of contacting varies across the political systems: In the Netherlands, they are 29% (27% of the sample); in Austria, they are 37% (30% of the total); in Yugoslavia, 61% (44% of the total), and in India, 51% (5% of the sample). As we noted in the analysis of campaign work, only in India do political ties have equally powerful effects on both forms of contacting.

Table 7. Incidence of Social Contacting at Levels of Party Membership and Campaign Work (%)

	Austria	India	Netherlands	Yugoslavia
Party members and frequent campaigners ^a	42	47	53	44
Frequent campaigners not party members	32	35	37	24
All party members	11	22	27	28
Party members no campaign work	6	14	20	15
Mean	6	5	10	12

^aIndividuals who have taken part in three or more election campaigns.

Table 8. Incidence of Particularized Contacting at Levels of Party Membership and Campaign Work (%)

	Austria	India	Netherlands	Yugoslavia
Party members and frequent campaigners ^a	28	56	50	25
Frequent campaigners not party members	28	47	49	29
All party members	28	44	38	24
Party members no campaign work	20	38	36	18
Mean	20	15	34	21

^aIndividuals who have taken part in three or more election campaigns.

Conclusions

Ties to political parties—as members and campaign activists—strongly affect the likelihood of contacting. This variable maintains its effect at all levels of social and economic resources; it makes the poor and uneducated more likely to contact than those who do not campaign; it further increases the likelihood of the wealthy and well educated to engage in contacting. Without political ties, the distribution of social contacting would look much like the distribution of socioeconomic resources, and the distribution of particularized contacting would have no form at all, reflecting the random nature of personal needs. Efforts to explain the likelihood of contacting need to include the distribution of political connections in the analysis.

Approximately half of those who take part in election campaigns and who are members of political parties knock on the doors of the politically powerful for governmental assistance. In turn, more than half of those who do the knocking are party and campaign activists. These connections go a long way in explaining who is likely to engage in contacting. These findings appear consistently in a politically, culturally, socially and economically diverse set of countries: Austria, India, Japan, the Netherlands, the United States, and Yugoslavia. They are, therefore, highly likely to appear elsewhere as well.

These findings have important implications. Political connections, especially to parties, emerge as the major structural basis for overcoming the limitations on political action imposed by poverty and ignorance (Schattschneider, 1960). Efforts to increase citizen demands on governments grow and are most effective when linked to political parties (Jones, 1981). If the place of parties has declined in the United States since these data were collected, then we would expect a concomitant fall in the level of social contacting (Eldersveld, 1982).

At the same time, party ties are by their very nature exclusive. They help only those who have the right political connections. The power of political clienteles in Indian and Japanese politics and policymaking is underlined by our finding that political connections have an especially powerful effect on all forms of contacting in these countries. The Netherlands provides an important counterexample with regard to particularized contacting. Here, the very high relative level of particularized contacting reflects the availability of government agencies that facilitate it. In the Netherlands, government structure weakens the exclusive quality of political connections as a determinant of particularized contacting. In all instances, political structures and connections directly and strongly influence the likelihood of contacting.

The variables that affect the relationship between political ties and contacting reflect neither the type of party system present nor the level of interparty competition. Similarly, the kind of party, as ascertained by organizational structure or ideology, has very little impact on levels of contacting within or among the political systems. Party ties and campaign activity have a direct and powerful effect on contacting, especially social contacting, regardless of the kind of party or party system.

The tendency for political ties to have a greater impact on social than particularized contacting raises a final general point. The differential effect reflects the additional benefits and costs faced by political activists, who have neighbors, friends, and workmates in need of governmental assistance. To help—to engage in social contacting—fulfills the expectations of these colleagues and cements the political bonds that bring votes to the activist's political allies. Not to help risks the loss of the political support of one's friends, neighbors, and workmates, and jeopardizes the activist's political base. The party and campaign activist is the focal point for the contacting efforts

of the people around him. Because he has the political connections and the political obligations, they turn to him for political assistance. This connection becomes the structural basis of the political ties that account for much of the social contacting that exists throughout the world.

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A Theoretical Analysis of the "Green Lobby"

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The purpose of this article is to develop a theoretical framework for analyzing why individuals support private environmental "public interest" groups. The model attempts to integrate past contributions which have argued that these decisions could be explained by one of several factors, including: bounded rationality and imperfect information (Moe, 1980); the features of the public goods provided (or influenced by) these groups (Mitchell, 1979); the existence of a disequilibrium in households' demands for public goods (Weisbrod, 1977); or as a response to contract failures (Hansmann, 1980). The article uses a theoretical framework originally developed to explain individuals' decisions to join private clubs and specifies the conditions for the efficient provision of access to different types of private, nonprofit groups. By describing the optimal access conditions as if individuals could be coordinated to assure this efficient outcome, the model provides insights into the benefits and costs associated with membership in the environmental groups in practice.

This article develops a theoretical framework for describing why individuals support private environmental public interest groups. Today these groups are increasingly regarded as an effective lobbying force in promoting environmental causes. Membership in environmental groups currently exceeds five million. Their collective annual budget was approximately \$85 million in 1982.¹ Moreover, a recent article in a popular business magazine on the so-called green lobby ranks its clout with that displayed by the right-to-life movement and the National Rifle Association.² Despite this increasing presence in environmental politics,

analysis of the reasons why individuals support these organizations remains quite limited. Early political science research adopted a pluralist explanation of interest groups, arguing that individuals choose to join such groups because they supported the group's goals. Olson's (1965) analysis of collective behavior provided a significant challenge to this approach. Rather than acting to realize common interests, Olson's framework implies that for individuals to join such groups, they must receive some type of selective incentives (i.e., gains that are private or subject to some form of exclusion).³

Three recent developments have contributed to the process of amending these views: 1) Moe's (1980) recent conceptual analysis of group behavior has argued that bounded rationality and imperfect information have important implications for description of group membership decisions and group activities in general; 2) Mitchell's (1979) conceptual and empirical analysis of the

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¹These statistics are taken from Symonds (1982).

²More specifically, Symonds (1982, p. 137) noted that: "Since President Reagan and the first Republican-controlled Senate in a generation took power in Washington, many of the liberal ideas and programs that flourished in the Sixties and Seventies have faltered. One that hasn't is the environmental movement, which has not only hung on to its public support but become so much stronger and more sophisticated that it deserves being ranked with the National Rifle Association and the right-to-life movement as a superlobby."

³Mancur Olson's (1965) analysis of the economic incentives for collective action suggests that a rational, self-interested, economic agent must be given specific, private (or in his terms, "selective") inducements to join a group. The provision of a public good (directly or indirectly) as a result of the group's activities, without the ability to exclude nonmembers from its enjoyment, will not be sufficient motivation to sustain a large pressure group.

These groups have also been described as pressure groups in the literature in political science and sociology dealing with their activities. See Castles (1967) and Truman (1958) as examples of the early literature and Moe (1980) for a more recent treatment of the political influences to group behavior.

motivations for individual membership in environmental groups provides the rudiments of an alternative explanation of the growth of these organizations; and 3) advances in modeling of private, nonprofit organizations have provided insights that can be used to enhance our understanding of an individual's motives for supporting environmental groups (notable examples include the work of Weisbrod (1977), and Hansmann (1980, 1981)).

The present model attempts to integrate these earlier contributions with an amended form of the theoretical model used to describe individuals' membership decisions for private clubs designed to provide local public (or collective) goods.⁴ More specifically, it describes the conditions for an efficient provision of access to different types of private, nonprofit groups. While the framework describes an optimal plan for access conditions as if the decisions of individuals could be coordinated to assure the efficient outcome, it does nonetheless provide insights into the relevant tradeoffs between the benefits and costs associated with membership in these groups in the real world, where the conditions governing access to each type of organization are not the result of an optimal plan.

This article is developed in four sections. The first of these reviews the relevant aspects of past analyses of the motivations for the formation of nonprofit groups and for their behavioral decisions. This includes a summary of both Moe's analysis of group behavior and Mitchell's specific survey findings on the reasons why individuals join environmental groups. In addition I consider the role of Weisbrod's (1977) and Hansmann's (1980) analyses of nonprofit organizations, together with other related evaluations of this sector (notably Downing and Brady (1981), Holtmann (1983), and James (1983)). The second section outlines a generalized model of club membership that recognizes the range of choices available to the individual in satisfying his or her demands for environmental amenities. For the purposes of my analysis these are represented by a society-wide environmental collective good, a privately available substitute good, and the services provided by

two distinct types of nonprofit organizations. The first offers a club-specific service which is a collective good to members and capable of being denied to nonmembers. The second is a lobby; it offers members the prospects for influencing the level of provision of the society-wide collective good that is perceived to be available to its members.

As a rule, public interest groups rely on donations to support their activities. For example, Downing and Brady (1981) report that 26% of the total revenue of the Sierra Club in 1979 was derived from contributions.⁵ Membership fees in the same year accounted for 34% of revenue. Hansmann (1981) has suggested that for many nonprofit organizations, the combination of fees for membership (or services) and donations can be interpreted as offering the prospects for voluntary price discrimination. Therefore, in the fourth section I discuss the implications of donations as a means of support for environmental lobbies. The last section summarizes the results of this analysis and discusses their implications for understanding the political economy of environmental groups.

The Rationale for Private Public Interest Groups

Nearly all of the current paradigms offered as descriptions of group behavior find their origins in Mancur Olson's (1965) work. Olson's analysis suggested that such groups provide members with two types of goods (or services): a private good and a collective good. However, in his framework it was suggested that this public good had to be largely a secondary consideration to the membership decision, when the ability to prevent other individuals from having access to it was not possible. Indeed, Olson's argument indicated that individuals will tend to act as free riders, seeking to enjoy the benefits that the organization provided without sharing in its costs. This tendency was enhanced within large organizations; that is, there might be large costs for large groups to provide public goods (because of the number of individuals involved), but we can generally expect to find that each individual's share of the costs of provision would be small. As a result, the individual can easily interpret this small share as un-

⁴The term "local" implies that there are boundaries to the public character of the good or service that permit individuals to be excluded from access. In the case of a pure public good, each individual's consumption of the good would not impinge on the ability of others to consume it, and there would be no ability to exclude these individuals from access.

As a rule, this term is used for municipal services, which, it is argued, have certain public good attributes and also provide some basis for exclusion.

⁵This estimate is substantially greater than the crude estimates derived as part of the discussion in the fourth section. Comparisons of Mitchell's per-member payments with posted membership fees would imply donations of only 1% of total payments. The statistical information on these groups is quite limited. The Downing-Brady data are likely to be more accurate indicators of donations. Unfortunately comparable data were not available for the other groups.

important to the continued provision of the public good. Olson referred to this difficulty as the inconsequentiality problem. The existence of these types of judgments on the part of individuals serves to provide reinforcement to the free rider tendency through a tyranny of small decisions.⁴ Accordingly, in Olson's framework group, actions leading to the provision of some public good as an indirect by-product of their activities could be expected only where there was an ability to coerce individuals to join the group, or sufficient individual private services were provided to each individual to offset the costs of membership.

Although the general conclusions of Olson's analysis are now generally accepted, there have been important initiatives to amend the framework to admit a wider range of goals than those implied by a purely economic perspective. In many respects these efforts seek a middle ground between the early pluralist explanations of group formation and the rational economic calculus embodied in the Olson model.

This research has considered several issues. The issue most important to these objectives involves the role of group size in its behavioral decisions and the implications of the idealized assumptions of the Olson model for its conclusions. Considering the first of these, Hardin (1977) has suggested that altruism and a concern for group goals are possible, but only within small, intimate groups. Indeed, consistent with Olson's model, he assigns a limited role to large groups in responding to such goals, noting that:

In large groups social policy institutions necessarily must be guided by what I have called the Cardinal Rule of Policy: *Never ask a person to act against his own self-interest.* It is within the limitations of this rule that we must seek to create our future. . . . In the intimacy of small groups altruism may be substantial and important; in large groups enlightened egoism is the most powerful motive. (Hardin, 1977, p. 27)

Olson admitted this possibility by recognizing

The concept of lobbying activity is being used rather loosely in this discussion. Legal statutes governing the behavior of nonprofit organizations limit the roles they can play in influencing political decisions. Thus, the reference to lobbying activities is not in the terms required for tax-exempt status; rather it is intended to refer to those sanctioned actions that direct public attention to environmental issues, commentaries provided by environmental groups during prescribed public review and information sessions, and other recognized open-access forums. Although such meetings are, in principle, equally accessible to individuals, the available information and costs generally preclude individual action and favor group responses.

that small groups could, in fact, provide empirically important levels of a collective good. Although some environmental groups have been small, their recent political successes have been largely the result of the coalition of these groups in responding to national environmental issues—actions that move in the opposite direction to that implied by these group size arguments.

The second line of research extending Olson's work focuses on his assumptions of perfect information and rational economic behavior. Moe's conceptual and empirical analyses of a variety of groups provided strong support for the importance of economic considerations as a dominant but not the exclusive explanation of behavior. He concluded his empirical appraisal noting that:

The overall impression conveyed by the interest group studies is simply that the average member ascribes more importance to nonpolitical than to political inducements and that he primarily seeks economic gain rather than purposive or solidarity incentives. (Moe, 1980, p. 200)

However, other motives, including altruism, status, and prestige do play some role as secondary considerations in explaining individual behavior.

In many respects Mitchell's (1979) arguments can be treated as an adaptation of these views, recognizing the specific features of environmental groups; that is, he has observed that it is possible to find public goods a major motivating force underlying collective behavior. Indeed, based on a survey of the members of five major environmental and conservation groups (i.e., the Environmental Defense Fund, Environmental Action, National Wildlife Federation, Sierra Club, and The Wilderness Society), he found that selective inducements have little bearing on individuals' decisions to join these organizations. Rather, the objectives specified as being important reasons for membership were largely associated with each individual's perception of a public good that he or she realized as a result of the group's actions. Mitchell's survey respondents were asked to rate items that identify their motivations for joining each group according to their importance on a five-point scale and then to rank the three most important reasons. The majority of the respondents in each of the five organizations sampled identified public good motivations as important reasons for belonging to the group. Table 1 summarizes some of the Mitchell findings. It reports by group the percentage of respondents identifying as important those motivations that Mitchell considered related to the public-good outputs of these groups. These concerns were uniformly more important to members than the private good

outputs of the respective organizations. The most significant of the missing elements in Olson's model, which prevents it from dealing with these cases, is the failure to distinguish responses according to the character of the public good or service provided by these groups. In some respects (as I discuss below), his proposed amendment parallels Hansmann's treatment of the features of goods and services that make them more suitable for being provided by nonprofit organizations.

Three considerations influence an individual's membership decision in Mitchell's framework—the cost of joining, the benefits of joining, and *the costs of not joining*. The first two of these motives are, of course, recognized in Olson's analysis. The

new element in Mitchell's framework is the recognition that the features of some goods may well lead the costs of not joining the group to be a strong motivation. Or, equivalently as Mitchell observed:

Drawing on qualitative evidence, including the groups' carefully pretested mail appeals, I argue that under certain conditions a utility for environmental goods will motivate member contributions and that these contributions are compatible with behavior of the egoistical, rational, utility maximizing kind because the cost is low, the potential cost of not contributing [or joining] is high and the individual has imperfect information about the effectiveness of his or her con-

Table 1. Mitchell Survey Findings on Motivations for Joining Environmental Groups^a

Motivation ^b	The Wilderness %	Environmental Defense Fund %	Sierra Club %	Environmental Action %	National Wildlife Federation %
If I don't act now to preserve the environment, things will get much worse.	68	68	64	43	57
If the (<i>relevant organization</i>) achieves its goals, my life and my children's lives will directly benefit.	53	50	56	50	55
My contribution to the (<i>relevant organization</i>) is helping to influence government action on conservation/environmental problems.	43	58	42	—	—
Solving environmental problems is so important that I try to support any effort aimed at that goal.	39	46	30	43	—
N	623	630	661	705	509

^aThese results report the percentage of respondents who rated the statement given as an important motivation to their joining the group which was the highest rating on a five-point scale. These results are summarized from Table 15 in Mitchell (1979).

^bThe remaining motivations identified for respondents to explain their membership in these groups were:

1. I enjoy the (*relevant organization's*) magazine and/or other benefits of membership, such as outings, very much.
2. Some aspects of my life are threatened by environmental or conservation problems.
3. As a result of the encouragement of my friends.
4. I personally gain much from the information I receive from the (*relevant organization*).
5. Many knowledgeable and influential people support the (*relevant organization*).
6. Without contributions like mine, the (*relevant organization*) would be unable to work for improved environmental quality.
7. If I dropped my membership in the (*relevant organization*), I would find it difficult to live with myself.
8. Belonging to the (*relevant organization*) makes me feel really good about what I am doing with my life.
9. My spouse belongs.

tribution in obtaining the good or preventing the bad. (Mitchell, 1979, p. 100, bracketed term added.)

An alternative interpretation of the behavior of environmental groups can be derived using models of the behavior of nonprofit institutions. Two of these efforts seem to have the most potential for transfer to the study of the behavior of environmental groups. The first is Weisbrod's (1977) disequilibrium model for the emergence of nonprofit institutions, and the second is Hansmann's theory based on contract failure.

Consider the Weisbrod framework. The public sector (either national or local) is assumed to provide some level of a collective good to all individuals within its jurisdiction. The quantity and quality provided of this good is assumed to be the result of a political voting process. Further, the model maintains that the individuals served have different utility functions and therefore do not value the public good equally. Although Weisbrod acknowledged the possibility of a variety of financial systems for paying the costs of providing this good, he argued that nearly all of them do not permit every consumer to equate the share of the tax (and thus the implicit price) that he or she pays for these services to the marginal benefit received from them (i.e., it precludes use of the Lindahl prices). To illustrate the difficulties this separation of quantity and price decisions poses, we need only consider a simple analysis of different individuals' demands for the public good. For example, suppose that Figure 1 describes three individuals' demand functions for public good, Q . To keep the analysis simple, assume each individual pays an equal contribution to the cost of the public good and that contribution (or price) is OT . All individuals receive the same quantity of the public good, OQ , by definition. As the figure illustrates, I have assumed that individuals are heterogeneous. Therefore each individual will respond differently to the quantity of the public good provided and to the perceived price paid. To understand these reactions, consider the second individual's demand, as given by D_2 ; this individual finds the government's level of provision and his perceived price completely consistent with his willingness to pay. Thus, he (or she) would not want to change either. The same judgment cannot, of course, be applied for individuals one and three.

For individual one the level of the good provided exceeds his demand at any price (including zero) and the perceived price would lead him to demand none of the good under private market provision. By contrast, individual three is undersatisfied. At a perceived price of OT , this individual would demand RV additional units of the

good. Thus, for both of these individuals, an exogenous determination of the quantity and price has led to dissatisfaction (for different reasons).

Weisbrod's explanation for the existence of nonprofit institutions considers them to be the result of the attempts by those individuals with excess demand for specific public goods to adjust the available level of public goods (or substitutes for them) in order to bring their desires into balance with the level of the good or service provided. Of course, these adjustments are assumed to take place within a framework that recognizes the incremental costs of providing alternatives. More specifically, his "output hypothesis" proposed that:

a class of voluntary organizations will come into existence as *extragovernmental providers of collective consumption goods*. These organizations will "supplement" the public provision (which can be zero) and provide an alternative to the private-sector provision of the private-good substitutes for collective goods. . . . The amount of collective good provision by the voluntary sector as compared with the public sector depends on the degree to which the public sector is able to satisfy the diverse demands of its constituents. (Weisbrod, 1977, pp. 59-61)

Unfortunately, this hypothesis did not explain the emergence of nonprofit over for-profit organizations for those collective goods that do permit selective exclusion. It also does not seem to deal with those activities by pressure groups seeking to influence the governmentally supplied level of the public good. The first of these limitations is addressed in Hansmann's analysis of nonprofit organizations. He argued that:

Nonprofits tend to produce particular services, those characterized by "contract failure," because consumers prefer to deal with nonprofits in purchasing those services. This preference . . . is based upon a feeling that nonprofits can be trusted not to exploit the advantage over the consumer from contract failure. This trust derives its rational basis from the nondistribution constraint that characterizes the nonprofit form. (Hansmann, 1980, p. 896).

In Hansmann's framework the collective or public nature of the good is only one of the reasons for "contract failure." He identified four others, including cases with:

- 1) a separation between the purchaser and recipient of a private service that makes it difficult for the purchaser to have assurance that the service was provided at the level and quality purchased;
- 2) goods where fixed costs account for a substantial fraction of costs of operation; there are in-

Figure 1.

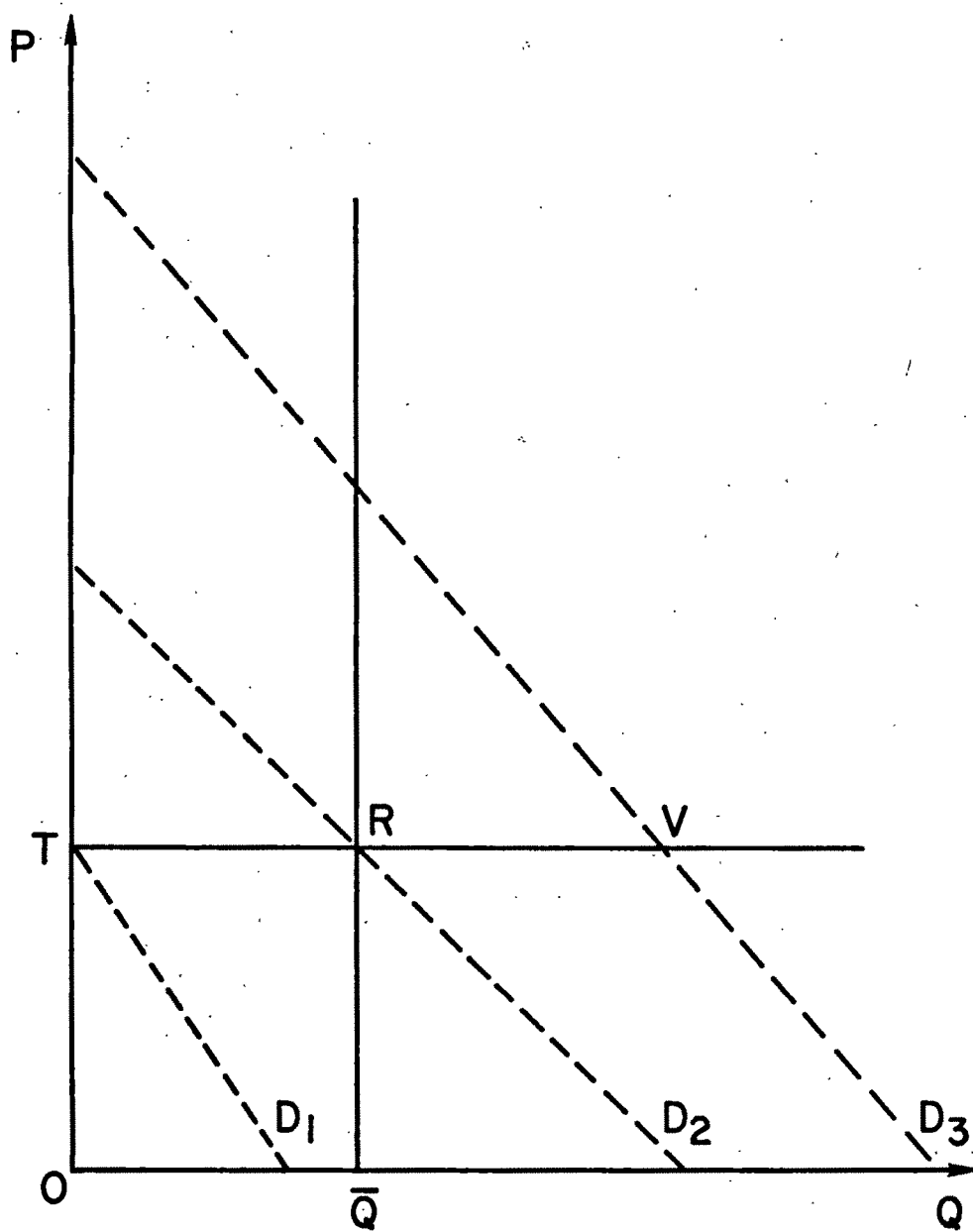


Figure 1

divisibilities in the production process; and the market for the good or service may be so limited that there is generally excess supply (good examples can be found in most of the performing arts). In these cases Hansmann argued that voluntary price discrimination through individual donations is possible. However, this is only true because the nonprofit form is regarded as offering a greater assurance that the donations will be used to continue the service;⁷

3) market imperfections limiting access to certain goods or services, such as loans. For example, legal and practical imperfections may limit an individual's access to loans for higher education and training; and

4) the delivery of complex personal services where the buyer must trust the seller. Services are provided at the discretion of the seller, but the buyer has no technical ability to monitor the outputs.

For my purposes none of these three explanations for nonprofit activities (i.e., Mitchell and Moe, Weisbrod, or Hansmann) provides a fully adequate description of the issues involved with an individual's choice to participate in an environmental public interest group, because each considers the decisions of individuals in isolation from the available alternatives.

Consider, for example, the alternatives before the representative individual who I shall assume is not satisfied with the level of a government-provided public good or service. It is sensible, following Weisbrod, to consider the modes of adjustment available to this individual. These adjustments might involve the acquisition of a private good (or goods) that substitute for the public good. Alternatively, the individual may join a group that seeks to provide a substitute public good whose services are confined to the members of the group (the Weisbrod conception of the emergence of nonprofits). Finally, it is also possible for the individual to attempt to influence the level of government provision through membership in a group that lobbies to increase the provision of the public good of interest. However, in this case the membership alone does not insure adjustment in the amount of the government-provided public good. Rather, the outcome will depend on the number of members in the group (as well as other considerations that may be outside the individual's control). The group may increase the public good provided to its members

or, equally plausible, may prevent it from being reduced which, based on Mitchell's cost of not contributing, may be equally important.⁸

In many cases, the individual does not have to rely on just one of these options but can consider undertaking a subset or, indeed, all of them. Of course it should also be acknowledged that the last of these alternatives is a simple caricature of the environmental organizations available in the real world. They have different histories and constituencies. Some trace their origins to the turn of the century (e.g., the Sierra Club in 1892 and the National Audubon Society in 1905), whereas others arose as litigators in the late sixties and early seventies. Of the 13 environmental groups considered by Mitchell, all had some form of magazine, but only one had an operational outings program. Their orientations toward political activity ranged from the fairly conservative stance of the National Wildlife Federation to the more activist orientation of the Sierra Club. These groups also span other dimensions, including special regional or issue-related interests such as the Defenders of Wildlife, whose focus is on protection of endangered species. Thus not all environmental groups can be expected to fit within the confines of a single type of group. It is important, therefore, to consider the potential role of different types of groups in providing an efficient response to individuals' demands for collective goods. This is clearly consistent with what we observe in practice and with past efforts to model group behavior.⁹

⁸Symonds (1982) clearly suggested that the environmental groups were responsible for preventing the relaxation of environmental regulations and a more pro-business policy. He cited three examples of the groups' influence: (a) the extension of the Endangered Species Act for three years without significant changes; (b) the defeat of former Interior Secretary Watt's attempt to permit some oil and gas leasing in wilderness areas; and (c) the National Clean Air Coalition's success (to this date) in preventing major changes in the Clean Air Act.

⁹Indeed, Chamberlin (1974) explicitly called for such efforts as one step in the development of criteria to evaluate group action. He noted that: "Most of the work making use of the theory of collective goods has thus far dealt with the actions of a single group . . . an important next step in applying this theory to the study of political phenomena would be the consideration of the behavior of several groups in competition" (p. 715). My approach is not fully responsive to this suggestion, because I consider the prospects for a coordinated role of different groups in efficiently meeting individuals' demands for collective goods.

A next step, beyond the scope of this study, would involve consideration of how the private actions of groups would relate to this idealized coordinated solution.

⁷For further discussion of this point see Hansmann (1980, pp. 854-859).

Environmental Public Interest Groups as Clubs

Sandler and Tschirhart (1980) define a club as "a voluntary group deriving mutual benefit from sharing one or more of the following: production costs, the members' characteristics, or a good characterized by excludable benefits" (p. 1482). I propose to use the theory of clubs as a modeling framework for describing the ideal conditions of access to environmental groups; that is, based on how these groups respond to individuals' demands, either directly, through environmental services that substitute for a government-provided public good, or indirectly through lobbying, the model describes the ideal levels of their activities (including the size of their memberships). These conditions are derived under the assumption that the resource allocation decisions are intended to structure the terms for access to these institutions, not to assign specific individuals to specific groups.¹⁰ My model should be distinguished from the Hansmann (1981), Holtmann (1983), and James (1983) positive models of the behavior of nonprofit institutions. In these studies, each author sought to model the decision making of nonprofits, rather than the reasons why individuals would join or support these groups. Holtmann (1983), for example, describes nonprofit organizations as a response to situations where market demand is random. In these cases it may be possible to offer the commodities involved under different rationing systems where the quantity demanded exceeds that supplied at the market price. James, by contrast, considered the use of profit-making activities by nonprofit institutions as one means for supporting (i.e., cross-subsidizing) their primary, nonprofit activities.¹¹

The basic structure of my model follows Hill-

man's and Swan's (1983) recent description of participation rules for clubs. There are, however, several important amendments to the framework they utilize: 1) my description of the production activities necessary for the club-produced public good uses a cost function at the individual club level. This specification is therefore independent of the production transformation function ($F(Z, Y)$) that describes the feasible combinations of the society-wide public good and the private numeraire good available to society;¹² 2) the definition of clubs includes a specification of the membership fees or cost-sharing rules. This approach permits the clubs to reflect the non-distribution constraint (i.e., no profit) that is characteristic of these groups;¹³ 3) the public interest club or lobby does not provide a tangible output. It can be interpreted as a mechanism for enhancing one's ability to petition for increases in the society-wide public good. Thus the output of the club is an increase in the likelihood of changing the level of this public good. Two versions of the model are considered. In one, these increments in the public good are enjoyed exclusively by the club members, whereas in the second everyone receives them; and 4) access conditions are used to define the efficient provision of different types of clubs to individuals.¹⁴ These conditions

¹⁰This approach avoids the difficulties associated with formulating models of the production technology that maintain consistency between the microlevel, club-specific description of the production activities and the economywide summary of a set of clubs' activities. The club results discussed recently by Boadway (1980) are incorrect because they fail to treat the micro and aggregate technologies consistently. See Berglas (1981) and Smith (1980) for further details.

¹¹Sandler and Tschirhart (1980) object to this characterization on the grounds that it imposes a second-best constraint on the objective function for a Pareto-optimal allocation of resources. They observed that the presence of this constraint leads to the disparity in findings on the implications of membership heterogeneity for the efficiency of segregation in clubs. Of course, their observations are within a model that allows members to utilize the club good by different amounts. Nonetheless, their overall conclusion would imply differential charges for different individuals based on their respective valuations of the club good. In their judgment the constraint would only be warranted if the monitoring costs were prohibitive. This position fails to recognize the importance of institutional constraints for the description of efficient resource allocation.

¹²In addition I have assumed club members receive equal quantities of the club-produced good and therefore do not have discretion in their intensity of use, as in Oakland's (1972) model. Sandler and Tschirhart (1980, pp. 1487-1488) provide a discussion of the advantages of the Oakland framework. It was not adopted here, because the present model considered three types of sub-

¹⁰See Hillman and Swan (1983, pp. 59-64) for further discussion of this distinction.

¹¹The contract research activities of some of the nonprofit environmental groups might be viewed in an analogous way; that is, some of these groups have undertaken contract-supported environmental research. These activities might be interpreted as secondary to their overall objectives of promoting governmental activities to maintain and enhance environmental quality and to preserve natural environments. Nonetheless, by defraying some of the organizational overhead, these contracts may serve to cross-subsidize their primary activities.

Research contracts should be distinguished from grants designed to support environmental groups' activities. Generally such funding is relatively free of requirements and would be capable of supporting the groups' primary goals. For a discussion of the importance of grants to the development of environmental groups, see Downing and Brady (1981).

can be interpreted either as offering membership access probabilities, based on an *ex ante* Pareto efficient lottery, or as the membership rules that would be implied by an equally weighted Benthamite additive social welfare function.

The model distinguishes four types of good: a society-wide public good, Z , which is available to all individuals regardless of their club membership; a private numeraire good, y ; and two types of substitutes for the society-wide public good—a club-provided public good, x , available only to members, and a private substitute good, w . This configuration was selected in an attempt to reflect the adjustment process described by Weisbrod as providing the motivations for the formation of nonprofit organizations.

It should be acknowledged, however, that by maintaining that each individual can enter one club I implicitly assume individuals make mutually exclusive choices about the organizations they will support. Thus, the portfolio of jointly undertaken activities that the model can consider involves all possible adjustments from the perspective of society as a whole.

Individuals are assumed to have their preferences described with well-behaved (i.e., differentiable, monotonic, quasi-concave) utility functions ($U^i(\cdot)$).¹⁵ The overall population, N , is fixed and the membership conditions describe the resource allocation necessary to offer each member of the population the same lottery with fixed odds of membership in each of three different groups—the club providing an excludable, environmental public good, the lobby intended to influence the level of government-provided public good (i.e., from Z to $Z+A$), and the general public (i.e., those not affiliated with either group). This system provides equal *ex ante* treatment for each

stitutes for the economywide public good. An Oakland club good was largely equivalent to our private good. The more conventional club membership public good provided a better contrast to the private good and to the services provided the “environmental lobby.”

¹⁵To simplify the analysis I have assumed identical individuals. If I were to consider the case of nonidentical individuals, then a ranking of individuals according to their respective preferences for each type of club would be necessary. Such a framework would be analogous to the approach Sandler and Tschirhart (1980) used to provide a general definition of the provision and membership conditions. However, in their case, the ranking was based on preference for the club good. With three states of assignment of individuals, it is not clear any consistent ranking could be derived. Thus, there might be the need to consider a variety of permutations of individuals to describe the Pareto efficient criteria. The assumption of identical individuals eliminates this problem.

member of the population in the sense that no other lottery could improve the expected utility of some member of the population without decreasing the expected utility of some other member. From an individual's perspective it is clear that one either joins or does not join a group. Individuals do not select lotteries. However, when we consider how to coordinate efficiently the activities of a set of groups as if they could be planned to realize an efficient allocation of resources, then the lottery concept provides one means of describing the conditions of access to each group that would be implied by an efficient plan. As Hillman and Swan explain, both this *ex ante* interpretation and an *ex post* explanation of the criteria as the result of selecting membership conditions to maximize social welfare are possible. This follows from the linearity of the specified social welfare function (see Ulph, 1982, pp. 267-268). In what follows I adopt the first of these interpretations. Regardless of their membership condition, each individual is assumed to have access to a certain allocation of the society-wide public good, Z . In addition, he is assumed to be able to “purchase” the private substitute good, w , at a fixed price (in terms of the numeraire good, y) of r .

Equation (1) states the objective function for describing the Pareto optimal allocation.

$$L = N EU + \gamma F(Z, Y) + \theta [Y - A - N_1 y_l - N_2 \bar{y}_l - (N - N_1 - N_2) \bar{y}_l] \quad (1)$$

where:

$$\begin{aligned} EU = & \frac{N_1}{N} U^l(y_l - \frac{c(x, N_1)}{N_1} - r w_l, h^l(x, Z, w_l)) \\ & + \frac{N_2}{N} [P(N_2) U^l(\bar{y}_l - \frac{C(N_2)}{N_2} - r w_l, \\ & h^l(O, Z + A, w_l)) + (1 - P(N_2)) U^l \\ & (\bar{y}_l - \frac{C(N_2)}{N_2} - r w_l, h^l(O, Z, w_l))] \\ & + \frac{(N - N_1 - N_2)}{N} U^l(\bar{y}_l - r w_l, h^l(O, Z, w_l)). \end{aligned}$$

EU describes the expected utility to be realized by the representative individual if society selects a resource allocation that provides N_1/N odds of being a member of a club providing the excludable public good, x , N_2/N odds of being a member of the environmental lobby, and $(N - N_1 - N_2)/N$ odds of being a member of neither. Within each group I have also defined the rules governing the

payments for membership and the corresponding production of each group's services.

The functions $c(x, N_1)$ and $C(N_2)$ describe the costs of operating each of the two kinds of clubs. The first kind produces a public good substitute for Z . The specification selected for the cost function is consistent with many other formulations of the club model at a decentralized level. (See McGuire, 1974, and Berglas, 1976, as examples.) Economic analyses of the club membership and collective good provision decisions have provided two different descriptions of the technologies used by clubs for these activities. When concern has been with the number and types of clubs, the description has generally used a production transformation function to describe all of their activities together (with the number of clubs included as an argument in this function). By contrast, when the focus is on the activities of a predefined number of clubs, the technology is often described using individual cost (or production) functions for each club. There has been some controversy over the consistency between the two descriptions in the recent literature.¹⁶ These concerns need not interfere with our analysis because a microlevel description is necessary to describe the specific institutional features of each group. That is, the cost-sharing arrangement is specifically imposed in each case as a vehicle for reflecting the importance of the nondistribution constraint.¹⁷

The three substitute goods are assumed to enter each individual's utility function within a weakly separable subfunction, $h^i(\cdot)$. This formulation is one way of maintaining that the three goods play a comparable role in contributing to individual utility. They do not exhibit equal marginal utilities, but do bear comparable associations with other goods that enter the individual's utility function. Moreover, it is consistent with the assumption of budget decomposability that im-

plicitly underlies Weisbrod's discussion of non-profit institutions. My analysis will consider differences in the treatment of club members. With the first club, I assume that only the N_1 members of the club receive x . All individuals have access to the society-wide public good, Z , and can purchase the private good substitute, w_i .

The second club does not provide a certain product (or service) to its members. Rather, it offers some prospect of enhancing the level of the society-wide public good they receive by a fixed amount, A . This increase comes at a cost born by society as a whole in terms of a reduced total amount of the private numeraire good, Y , for all. Although the numeraire good is always reduced by A , it is not clear the allocation of A will come to the club members. Thus, there is a probability, $P(N_2)$ of receiving the good.¹⁸ This is bounded between zero and one, and a concave, monotonic, increasing function of N_2 , the number of club members. I consider two formulations of our model—one in which only club members receive A and another in which all of society receives A .

As I noted earlier, $F(Z, Y)$ describes society's transformation function for the private numeraire good in total and the society-wide public good. A tilde (\sim) and a bar ($\bar{\cdot}$) are used for notational convenience to identify individual utility levels by club type and to specify individual selections of y_i . This format serves to highlight that individuals in each club may not receive the same allocations of y . γ and θ are all Lagrangian multipliers.

If I consider the first interpretation of the lobby, differentiate equation (1) with respect to x , y_i , w_i , N_1 and N_2 , and rearrange the resulting first-order conditions, I can specify provision conditions for x , w_i , and Z as well as conditions for the access terms for each of the two types of clubs. These are presented in equations (2) through (6), with MRS designating the marginal rate of substitution, and $EMRS$ the expected marginal rate of substitution. That is:

$$MRS_{xy}^i = \frac{\left(\frac{\partial U^i}{\partial x}\right)}{\left(\frac{\partial U^i}{\partial y_i}\right)}$$

¹⁶Further discussion of this issue is available in Berglas (1981) and Smith (1980).

¹⁷Sandler and Tschirhart (1980) have characterized such constraints as converting the Pareto-optimal club framework to a second-best format. This characterization is somewhat misleading. Although it is true that replacing either of these constraints with one reflecting an individual's specific costs to the club (under the assumption of varying intensities of use) can lead to higher levels of welfare for some individuals without reducing the utilities of others, it fails to recognize that each specification for the payment constraint describes an alternative institutional framework. Institutions limit the feasible allocations of goods and services and should be reflected in the description of the conditions of the politically feasible, economically efficient resource allocations. The use of these cost-sharing constraints in this model offers one simple means of characterizing the nondistribution constraint of nonprofit organizations.

¹⁸This is somewhat similar to a framework adopted by Austen-Smith (1981) in describing an individual's motives for joining a pressure group. In his framework, membership can serve to add a constant amount to the individual's utility function; that is, if the group is successful, the individual utility is $U_i + A$ and if not successful, it is U_i . His analysis is in terms of a "within club" framework and considers an individual's allocation of time to club activities in the presence of uncertainty as to the success of the group's activities.

$$\text{and } EMRS_{wy}^i = P(N_2) \frac{\frac{\partial U^i}{\partial w_i}}{\frac{\partial U^i}{\partial y_i}} + (1 - P(N_2)) \frac{\frac{\partial U^i}{\partial w_i}}{\frac{\partial U^i}{\partial \bar{y}_i}}$$

$$\sum_{i=1}^{N_1} MRS_{xy}^i = N_1 MRS_{xy} = \frac{\partial c}{\partial x} \quad (2)$$

(provision of x , because all individuals are assumed identical)

$$MRS_{wy}^i = EMRS_{wy}^i = r \text{ for } i = 1, 2, \dots, N. \quad (3)$$

(provision of w)

$$N_1 MRS_{Zy}^i + N_2 EMRS_{Z\bar{y}}^i + (N - N_1 - N_2) MRS_{Z\bar{y}}^i = MRT_{ZY}. \quad (4)$$

(provision of Z)

$$\frac{U^i}{\frac{\partial U^i}{\partial y_i}} - \frac{\bar{U}^i}{\frac{\partial \bar{U}^i}{\partial \bar{y}_i}} + (\bar{y}_i - y_i) = \frac{\partial c}{\partial N_1} - \frac{c(x, N_1)}{N_1}. \quad (5)$$

(membership in the first club—the environmental group)

$$\frac{E(\bar{U}^i)}{E(\frac{\partial \bar{U}^i}{\partial \bar{y}_i})} - \frac{\bar{U}^i}{\frac{\partial \bar{U}^i}{\partial \bar{y}_i}} + (\bar{y}_i - \bar{y}_i) + N_2 \frac{dP}{dN_2} \frac{(U(\cdot, Z+A) - U^i(\cdot, Z))}{E(\frac{\partial \bar{U}^i}{\partial \bar{y}_i})} = \frac{dC}{dN_2} - \frac{C(N_2)}{N_2}. \quad (6)$$

(membership in the second club—the environmental lobby)

where $\bar{U}^i(\cdot, Z+A)$ is a shorthand expression for

$$U^i(y_i - \frac{C(N_2)}{N_2} - rw_i, h^i(O, Z+A, w_i))$$

and $\bar{U}^i(\cdot, Z)$ corresponds to

$$U^i(y_i - \frac{C(N_2)}{N_2} - rw_i, h^i(O, Z, w_i)).$$

To simplify these expressions I have adopted some notation conventions. First, a bar ($\bar{\cdot}$) or tilda (\sim) over y_i or U^i is used throughout to designate the individual's status (i.e., no bar or tilda indicates a member of the first club, a tilda indicates membership in the lobby, and a bar membership in neither).

The most interesting aspect of the model's insights arises with the membership conditions. Equations (2) through (4) provide the conditions for optimal provision levels of the club public good (x), the private substitute (w), and the society-wide public good (Z), respectively. Equation (2) indicates that the level of x provided should be such that the aggregate marginal benefit for club members ($N_1 MRS_{xy}$) equals the marginal cost of producing x (measured in units of the private numeraire— y). This is the standard result from club theory (see Berglas, 1976; Buchanan, 1962; and Sandler & Tschirhart, 1980, as examples). Equation (3) indicates that individuals, regardless of their membership in one of the clubs, will acquire the private good, which functions as a substitute for the public goods (i.e., Z and x) until the marginal benefit (or willingness to pay) measured in terms of the numeraire y equals the marginal cost (also in terms of y). This arises because each individual faces the same access condition for the clubs (and the price of w_i is constant across membership states), the "certain" marginal benefits from w for members of the first club or nonmembers or both will equal the expected marginal benefit realized by members in the lobby.

In equation (4) there is the now familiar Samuelsonian condition for the level of provision of the economywide public good, Z ; that is, the individual demands for collective good are summed vertically, and the result is equal to the marginal cost of providing the public good (MRT_{ZY}) in order for an efficient level of Z . My formulation implicitly accepts the Diamond (1967) and Sandmo (1972) approaches to defining *ex ante* Pareto efficient allocations, and in this case treats expected and actual marginal rates of substitution in equivalent terms, which means that society is adopting a risk-neutral attitude. That is, equation (4) specifies an efficiency rule that treats the expected marginal rates of substitution (which are comparable to inverse demands functions) as the certainty equivalent demands.

My membership conditions are quite different from all earlier discussions of club membership rules, including Sandler and Tschirhart (1980), Hillman and Swan (1983), and earlier work by Helpman and Hillman (1977). The differences arise from two sources: the prospects for membership in either of two clubs, each with a different type of service to offer; and an explicit intro-

duction of the institutional arrangements governing each individual's membership fee. The access conditions for these clubs use the nonmembership state as a benchmark. Thus, the benefit (from society's perspective) to increasing the *ex ante* prospects for membership in the first club is the incremental utility as a result of membership (measured in terms of y) plus the change in the allocation of the private numeraire good required to assure that

$$\frac{\partial U^I}{\partial y_I} = E\left(\frac{\partial U^I}{\partial y_I}\right) = \frac{\partial U^I}{\partial y_I},^{19}$$

Given our predefined payment scheme, the cost to society is determined by the cost imposed on each club by an additional member (i.e., $\frac{dC}{dN_2}$ or $\frac{\partial c}{\partial N_1}$)

in comparison to the membership fee or average cost. In effect, social efficiency requires I judge the costs of providing membership according to whether additional members would pay for the costs they impose on the group. It is important to note that I do not require that they pay the full incremental costs of their membership. Rather, efficient access conditions recognize that the benefits these members realize directly or, in the case of the lottery, confer on others indirectly, must also be considered.

The environmental lobby (i.e., the second club) includes two types of benefits associated with membership: the incremental utility measured as the expected utility from lobbying activities, and the incremental benefits associated with each member's contribution to the probability of successful lobbying activity. In other words, because the probability of realizing an increment to Z from lobbying was expressed as a function of the size of the lobby, each additional member raises the likelihood of success, and all existing members gain as a result. Their gain is the incremental utility provided by an increase from Z to $Z + A$ weighted by the increased likelihood of success as a result of the additional member, $\frac{dP}{dN_2}$. These incremental

benefits, including the external benefits provided by a new member to all existing members, are equated to the excess of marginal operating costs over the membership fee (all measured in terms of the numeraire good y).

This description of the lobby implicitly main-

tains that there is a mechanism to assure that the increment (A) to the public good (Z) as a result of lobbying is realized only by members of the lobbying group. If I drop this assumption, the benefits of lobbying activity are spread more widely; that is, additional terms reflecting the increment to utility (measured in units of y) for all individuals would be counted among the benefits. Of course, these are present in this description because we are examining a socially optimal description of the club access conditions. In a description of the private incentives for lobbying activities, these would be externalities (and one would conclude a sub-optimal level of the activity was undertaken).

How do these results relate to describing an individual's decisions for joining an environmental lobbying group or other nonprofit, public interest group providing goods that substitute for government-provided public goods? They suggest that on efficiency grounds (recognizing the potential role of institutional constraints), these activities should be undertaken. Thus, the private actions we observe in supporting environmental lobbies can be explained in a rational economic framework. In addition, they indicate that with some modification rational behavior need not preclude collective action. However, it should be acknowledged that in both of the clubs described here, some form of exclusion or influence is assumed to be present. For the group providing x , the club is able to restrict services to those individuals joining the club. By contrast with the environmental lobby, the ability to influence the provision of Z is assumed to reside with the lobby. This influence is similar to self-protection activities discussed in the Ehrlich-Becker (1972) analysis of insurance. My analysis of this case suggests that exclusion is not essential, but the ability to exercise either exclusion or influence is. Of course, it should be acknowledged that without real or perceived exclusion, the incentives for an individual to join the lobby would be greatly diminished. They may not be zero. As the marginal conditions suggest (considering only the terms associated with one individual), the incentives depend on the quality of the available substitutes (i.e., x and w_I), and the respective costs of each to the individual.

Any judgment about the importance of these modifications to Olson's framework depends on whether they are plausible descriptions of why individuals join the groups. Does an individual's residential location or some other aspect of the constraints to his (or her) allocation decisions (such as ability to take vacation time) permit him to take greater advantage of increments to the society-wide public good? Is membership in a lobby the most cost-effective way of assuring an increase in the supply of a public good regardless of whether the increase is also made available to

¹⁹See Hillman and Swan (1983, pp. 61-63) for a discussion of the implications of this condition in models where club participation is uncertain and a parallel requirement when it is certain.

others? This possibility offers an alternative to Moe's argument that some collective goods may actually generate their own selective incentives because of the unique role of purposive incentives (i.e., benefits derived from the support and pursuits of worthwhile collective goods). In other words, we do not need to assume some form of altruism to explain membership in environmental lobbies.

Finally, it is also possible to argue (within a model that allows for heterogeneous individuals) that some individuals' valuation of environmental services may not be exclusively associated with their use of these services. There has been increasing recognition of the importance of existence values in the aggregate benefits associated with unique natural environments (see Schulze, Brookshire, Walther, & Kelley (1981)).²⁰ Consequently, one might also interpret the membership in environmental lobbies as composed of individuals who realize existence values in excess of that associated with the direct, user-oriented level of the collective good (Z). In such a framework, the increment (A) might be interpreted as a reflection of these nonuser services.

These efficiency conditions also suggest that several types of groups can exist simultaneously (even under the restrictive assumption of mutually exclusive membership patterns): Moreover, the existence of either society-wide public or private goods that substitute for the services provided by the club does not, in itself, preclude the existence of an environmental public interest group as an institutional mechanism for improving the allocation of resources. An important distinction between an *ex ante* Pareto-optimal formulation of the role of environmental groups and one based on a positive model of individual decision making arises with the character of the information available to the individual. For example, both the Mitchell (1979) and the Moe (1980) explanations of the economic rationale for an individual joining a public interest group rely on an assumption of incomplete information. In Mitchell's case this was associated with a rather large discrepancy between $\bar{U}^i(\cdot, Z + A)$ and $\bar{U}^i(\cdot, Z)$, as well as the perception that each individual's membership can influence the realization of the group's objec-

tives (i.e., $\frac{dP}{dN_2}$ is perceived as significantly different from zero). My analysis maintained that these relationships are known and constrain the *ex ante* resource allocation decisions. In the real world, one can expect a diversity of information and perceptions; this may further enhance the prospects for actual membership decisions.

Voluntary Price Discrimination and Clubs

The model described in the previous section ignores one important aspect of individuals' relationships to nonprofit groups. In many cases, individuals do not simply join the group and pay their membership fee. There can be substantial voluntary contributions of time and monetary resources in support of these groups.²¹ Hansmann (1981) describes these responses as a form of voluntary price discrimination made possible because individuals trust the organization to use donations to support the goods or services provided. For some environmental groups, donations can be an important component of their income. Mitchell (1979) reports, based on personal interviews with the membership directors of environmental groups, that between 17 and 49% of the members of the five groups with available data made contributions above the ordinary membership fee.²² Nonetheless, it is difficult to gauge directly the magnitude of donations as a fraction of any group's total income. Mitchell's (1980) subsequent analysis of the five other environmental groups (including three of the groups reporting the proportion of the membership donating in excess of dues) reported the membership fee and income from members (including the fee and member donations). Table 2 uses this information together with current membership levels to calculate an approximate estimate of the total donations. These crude estimates are likely to understate actual donations. Nonetheless, they indicate donations are an appreciable component of each group's operating budget, ranging from \$70,000

²¹This was the primary focus of Austen-Smith's (1981) model.

²²Mitchell (1979) also reports information on the percentage of the membership of selected environmental groups that made contributions higher than the basic membership fee. His information was based on personal interviews with the membership directors of several of the organizations. They include three of the groups reported in Table 2: Environmental Defense Fund, 32.0%; Sierra Club, 2.4%; Wilderness Society, 17.0%.

For the Sierra Club he reported the percentage of the membership donating \$50 or more in addition to paying their dues. See Mitchell (1979, pp. 107-110) for further discussion.

²⁰Existence value arises when an individual gains utility from the presence of a good, generally a unique natural endowment, even though there is no direct consumption of its services. The theoretical implications of this concept have not been fully developed in the literature. Some economists have described them as a form of either vicarious consumption of or stewardship motives for the environmental resources.

per year for the Sierra Club (which is substantially less than the donations reported by Downing & Brady, 1981) to \$5.2 million for the National Wildlife Federation. Thus, it seems reasonable to consider the role of donations in an *ex ante* Pareto framework.

I will consider the case where donations are made to the environmental lobby group; moreover, I will assume that aggregate donations increase the probability of realizing the predefined increment to the economywide public good (A). This specification implies that $P(N_2)$ in equation

(1) would be replaced by $\bar{P}(N_2, D)$ where $D = \sum_{i=1}^{N_2} d_i$

with d_i representing each individual's donation expressed in terms of y_i^f . Of course, each individual's y_i would also be reduced by d_i to reflect the voluntary contribution to support the club in excess of the constant membership fee— $C(N_2)/N_2$. Within the *ex ante* Pareto decision-making framework each individual would be assumed to donate to the club until his expected loss from the contribution equaled the aggregate expected improvement for all members resulting from the effects of the aggregate donations on $\bar{P}(n, D)$.²¹

²¹If I assumed that the increment A was received by everyone regardless of whether or not they were members of the lobby, I simply add terms to the left side of equation (7) reflecting the utility increments for these groups and their membership levels.

$$\frac{N_2 \frac{\partial \bar{P}}{\partial D} (\bar{U}^i(\cdot, Z+A) - \bar{U}^i(\cdot, Z))}{E\left(\frac{\partial \bar{U}^i}{\partial y_i}\right)} = 1. \quad (7)$$

In addition, in equation (6) the change in the private good to equalize marginal utilities of the numeraire good across alternative states will include an adjustment for an individual's donations to the lobby (i.e., $\bar{y}_i - \tilde{y}_i$ will be replaced by $\bar{y}_i - \tilde{y}_i - d_i$).

The marginal cost of a donation is one, because donations are expressed in terms of y , and each unit donated is assumed to be completely allocated to lobbying activities. If, by contrast, I assumed a fraction of total donations, say α ($0 < \alpha < 1$) would be allocated to lobbying activities, then the expected incremental gains (to society) in equation (7) would equal $1/\alpha$.

Of course, what is important from the perspective of explaining membership decisions is how the individual will perceive his or her benefit resulting from a donation. The Pareto criterion internalizes the expected increment to all members' utilities resulting from an increase in the societywide public good to $Z + A$. To the extent an individual's perception is limited to personal (or perhaps family members') utility increments, donations will be less than the efficient levels. However, they will not necessarily be zero. This is Hansmann's point in discussing the prospects for voluntary price discrimination.

Table 2. A Comparison of Membership Fees and Total Contributions^a

	Membership Fee	Average Amount Received from Members	Membership in 1982 ^b (millions)	Estimated Annual Donations (millions)
National Wildlife Federation ^c	\$ 7.50	\$16.24	4.20	\$5.21
Environmental Defense Fund	15.00	17.56	0.05	0.13
Wilderness Society	15.00	17.36	0.07	0.17
Sierra Club	20.00	20.22	0.31	0.07
Environmental Action	15.00	15.00 ^d	0.03	

^aThese estimates are based on 1978 membership fees reported in Mitchell (1980, p. 50) and contribution information for 1977, 1978, and 1979 reported in Mitchell's Table 10. The calculations assume that the membership fee did not change over the period.

^bThese membership estimates are taken from Symonds (1982).

^cThe distinction between membership fee and the average amount received from members is somewhat misleading in this case. The \$7.50 fee provides membership in the National Wildlife Federation and the *National Wildlife Magazine*. A \$15.00 fee provides membership, the *National Wildlife Magazine*, and the *International Wildlife Magazine*. I have used the higher of these two figures to provide a conservative estimate of the total donations. I am grateful to John Tschirhart for calling this point to my attention.

^dThe estimated average amount received was somewhat less than the membership fee. Presumably this difference was the result of using 1977 income from membership and the 1978 membership fee.

Implications

This article has explored the possibility of using a society-wide Pareto optimal description of club membership and provision conditions to evaluate the role of public interest groups in promoting an efficient allocation of resources. The model used for this purpose extended the *ex ante* Pareto conditions recently presented by Hillman and Swan (1983) by considering the definition of the conditions of access to each of three possible groups. The use of access to membership provided the basis for our *ex ante* analysis of the membership conditions. Two types of clubs were introduced, one providing a public good to its members, and the second (intended to represent the environmental lobby) attempting to influence the level of the society-wide (environmental) public good provided to its membership (as well as more generally). This characterization of the efficient assignment and provision conditions allows the model to consider aspects of the Weisbrod, Mitchell, Moe, and Hansmann analyses of the reasons for and role of nonprofit organizations.

In a genuine sense the model described here attempts to formalize and extend Moe's (1980) general analysis of groups and Mitchell's (1979) more specific evaluation of individuals' membership decisions in environmental groups. The results clearly support a rational explanation of the role of those groups when there exists either a basis for influencing the provision of a public good or providing some substitute excludable good. Indeed, the conditions for an efficient allocation of resources admit the possibility for the existence of different types of clubs, providing goods or services that substitute for or offer the prospect of enhancement in the supply of an economy-wide public good. Thus, the model does not invalidate Olson's general argument, which relies on the inability of groups to exclude nonmembers from enjoying their collective services. Rather, this model recognizes the exclusion is only one of the forms selective incentives can take.

The framework also suggests that donations as a form of voluntary price discrimination can be considered a part of an efficient resource allocation, when these donations influence the probability of success of the environmental lobby. My conditions are also consistent with Hansmann's judgments about what can be expected from donations in a framework structured to describe an individual's choices. In this case, the individual's level of donations will not necessarily be zero; rather it will depend on that individual's perception of the effects of his contribution for the success of the group. This finding is clearly consistent with Moe's (1980) arguments emphasizing the importance of limited information and

bounded rationality in explaining group behavior. It is also compatible with Mitchell's (1979) arguments for membership in environmental groups and Austen-Smith's (1981) evaluation of the allocation of individual effort to pressure groups.

Nonetheless, the model remains quite limited. The characterization of uncertainty used in the outputs of the environmental lobby was rather simple. Individuals were assumed to be homogeneous and to be members of only one club at a time. Finally, I did not attempt to explore the relationship between decentralized models of club behavior and this economy-wide, centralized assignment framework. Each of these limitations suggests possible areas for improvement and extension. Nevertheless, the analysis does suggest that Mitchell's arguments for membership in collective groups as a rational decision can be supported and extended within a generalized club-theoretic framework.

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Provision of Public Goods and the MCS Experimental Paradigm

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Van de Kragt, Orbell, and Dawes (1983) reported the results of a series of experiments in which n subjects each receive a monetary endowment and then may choose privately whether to contribute it to a monetary public good; the good is supplied if a prespecified number $m < n$ of contributions or more is made. Decision policies maximizing expected value are derived for this experimental paradigm under three different assumptions about the expectations each individual has about the decisions of the other $n - 1$ members: A homogeneity assumption postulating that each other member contributes with fixed probability p , a heterogeneity assumption postulating that the $n - 1$ p 's are independently selected from a subjective probability distribution, and a partial homogeneity assumption postulating that the $n - 1$ members are partitioned into distinct subsets with the same p for all members of each subset. The theoretical and social implications of these assumptions are briefly discussed.

Public goods are defined by two properties, namely, jointness of supply and impossibility of exclusion (Barry & Hardin, 1982). A basic prediction concerning public goods, derived from a straightforward extension of the standard economic model of rational choice, is that they will be underprovided relative to demand or, in the extreme case, will not be provided at all. "Rational and selfish individuals will recognize the opportunity to 'free ride' on the contributions of others which public goods offer and, secure in the knowledge that they can share in the good once it is provided, will withhold or severely curtail their own contributions" (van de Kragt, Orbell, & Dawes, 1983, p. 112). To explain the fact that people demonstrably do contribute toward public goods, it has been natural to replace the model of narrow rationality defined for the case of self-interested motivation by a more general model that prescribes maximization of expected utility in which the utility is a function of the payoffs (contributions and benefits) associated with each decision as well as other individual and social consequences (e.g., satisfying one's conscience, contribution to the welfare of others).

Apparently motivated by the work of Campbell (1975) and Hardin (1968, 1977), who have viewed

with skepticism or mistrust solutions to social problems founded on altruism, conscience, and social norms as explanatory constructs, van de Kragt et al. posed the following question: "Under what circumstances will individuals who are *not* constrained by a concern for the welfare of others nevertheless act so as to provide valued public goods for the groups of which they are members?" (1983, p. 113). Rather than attempting to answer this question by theoretical arguments, van de Kragt et al. proposed and employed a simple and attractive experimental paradigm which, if further extended, may successfully compete with the familiar n -person Prisoner's Dilemma paradigm. They reported the results of a series of small group experiments in which seven subjects each receive a fixed monetary endowment and then may choose privately whether to contribute the endowment to a monetary public good, and in which the good is supplied if a prespecified number of contributions or more are made. Communication among the subjects was the major independent variable in these experiments. Given the opportunity to discuss the problem before making their decisions privately, the subjects organized themselves by specifying the contributors in a way that always resulted in an efficient provision of the public good. But even when communication was prohibited and feedback was not given, the public good was provided 65% of the time, and when provided, inefficiency resulted more than 50% of the time because of over-provision.

My concern in the present article is with the traditional, no-communication, single-trial step-good collective action with binary contributions (Hardin, 1982). If the benefit to the group increases as the number of contributors grows, with

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no minimal contributing set (MCS), the provision of public goods may be modeled as an n -person Prisoner's Dilemma game in which the (pure) strategy not to contribute is unconditionally best (Hardin, 1971, 1982). But if, as in the experimental paradigm above, a MCS is required for the public good to be provided, an unconditionally best strategy can be no longer identified (Frohlich & Oppenheimer, 1978; Hardin, 1982). For an expected value maximizer, the decision to contribute depends on the size of the group, the size of the MCS, the values of the endowment and the benefit, and the probability that the decision maker assigns to the event that each of the other members of his or her group will contribute. Van de Kragt et al. did not attempt to derive decision policies maximizing expected value and apply them to the particular parameters used in their MCS study. Therefore, the answer to the question they originally posed is incomplete, and although Frohlich and Oppenheimer (1978, chap. 3) and Hardin (1982, chap. 4) undertook an expected value maximization analysis of the MCS paradigm, they left the decision maker's beliefs or expectations regarding the decisions of the other group members completely unspecified. Following Straffin's (1978) probabilistic approach to the two classical power indexes of Shapley and Shubik (1954) and Banzhaf (1965), the present article proposes three alternative assumptions about the decision maker's beliefs and then examines briefly their social implications.

Because I am primarily interested in developing expected utility models that are experimentally testable, my focus is on small groups. As the group size grows, the MCS paradigm approaches the n -person Prisoner's Dilemma game. Consequently, the results derived below from the three different models of the decision maker's belief structure become progressively weaker and of less interest as the number of group members grows. In this respect, my results agree with Olson's (1965) central conclusion regarding the effects of group size on the likelihood of group success: large groups will fail, small groups may succeed.

The MCS Experimental Paradigm

The single-trial MCS experimental paradigm described by van de Kragt et al., in which pre-decision communication among group members is prohibited, may be characterized by the following assumptions:

1. The game is played by a group $N = \{1, \dots, n\}$ of n players who are unfamiliar with one another. Communication before or during the game is prohibited.
2. Each player i ($i \in N$) receives a fixed monetary endowment of e units ($e > 0$), which is his or hers to keep.
3. Having received the endowment, each player i must decide privately whether or not to contribute it to the group's benefit.
4. If m or more players contribute, each player i receives a reward (public good) of r units. If $m - 1$ or fewer players contribute, each of the contributors loses his or her endowment.
5. With the exception of the final payoff, no feedback is provided about the game. In particular, player i never learns how many players actually contributed.

I assume that $r > e$. Assumption (5) places mild restrictions on n and m . The first and obvious restriction is $n > 3$. Another restriction is $1 < m < n - 1$. Note that if $m = 1$ and the payoff of player i is 0, player i learns that nobody else contributed, contrary to assumption (5). Similarly, if $m = n - 1$ a payoff of $r + e$ tells player i that all other $n - 1$ players contributed, contrary to assumption (5).

The payoff matrix for player i is shown in Table 1. Let P_{m-1} denote player i 's probability before making his or her private decision that exactly $m - 1$ other players will contribute, and let P_{m+} denote player i 's probability that m or more other players will contribute. Then the expected value of contributing for player i is

$$EV(C) = r(P_{m-1} + P_{m+}),$$

and the expected value of not contributing is

$$\begin{aligned} EV(\bar{C}) &= e(1 - P_{m+}) + (e + r)P_{m+} \\ &= e + rP_{m+}. \end{aligned}$$

Defining $D = EV(C) - EV(\bar{C})$, I obtain immediately

$$\begin{aligned} D &= r(P_{m-1} + P_{m+}) - e - rP_{m+} \\ &= rP_{m-1} - e. \end{aligned} \quad (1)$$

Thus, if he or she subscribes to the maximization of expected value, player i should contribute if $rP_{m-1} > e$ and not contribute otherwise. Because P_{m-1} is the probability of player i making a difference for the attainment of the public good, or being *critical*, equation (1) implies that in order to contribute the expected value of making a difference must be greater than the individual's cost of contributing to the public good. Alternatively, the probability of being critical must exceed the

Table 1. Payoff Matrix for the MCS Paradigm with No Communication and Feedback

Player i 's decision	Number of Players Contributing		
	$m - 2$ or fewer	exactly $m - 1$	m or more
Contribute (C)	0	r	r
Not Contribute (\bar{C})	e	e	$e + r$

Note: $r > e > 0$.

ratio e/r of cost to benefit (Frohlich & Oppenheimer, 1978).

Probability Models for the MCS Paradigm

For a rational and self-motivated player who desires to maximize expected value, the decision of whether or not to contribute depends on the expectations he or she has about the behavior of others. To model these expectations, I exploit a distinction made by Straffin (1977, 1978), who proposed an approach to measure voting power as an answer to a probabilistic question about voting outcomes. Straffin's answer is based on probabilistic assumptions about the nature of the issues to be decided by the weighted voting body as well as the degree of cohesiveness among the members of the body. Both Straffin's approach and the approach proposed in the present article are basically intended to answer the same question, namely, what is the probability that player i 's decision will make a difference in the outcome? The contexts, however, are quite different. Whereas Straffin is concerned with the probability that a bill which player i supports passes, but would fail if player i changed his or her vote, or that a bill which player i opposes fails, but would pass if player i changed his or her vote, the present approach is concerned with the probability that a group will succeed if player i decides to contribute, but will fail otherwise.

Let p_i be interpreted then as player i 's probability that player j ($j \in N, j \neq i$) will contribute. Two possible assumptions are the following:

Homogeneity Assumption. A number p ($0 < p < 1$) is selected, and $p_j = p$ for all j ; or

Heterogeneity Assumption. The p_j 's are selected independently from the beta distribution $B(\alpha, \beta)$ on $[0, 1]$.

According to the homogeneity assumption, player i does not distinguish among the $n - 1$

remaining anonymous players. Rather, he or she assigns each of them the same probability of contributing. On the other hand, the heterogeneity assumption recognizes individual differences among the $n - 1$ remaining members of the group. The assumption that the p_j 's are independently selected from the beta distribution is not at all restrictive (Raiffa & Schlaifer, 1961); it allows much flexibility in describing player i 's expectations regarding the decision of his or her coplayers.

Because P_{m-1} is player i 's probability that exactly $m - 1$ of the remaining $n - 1$ players will contribute, the homogeneity assumption yields directly

$$P_{m-1} = \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m}. \quad (2)$$

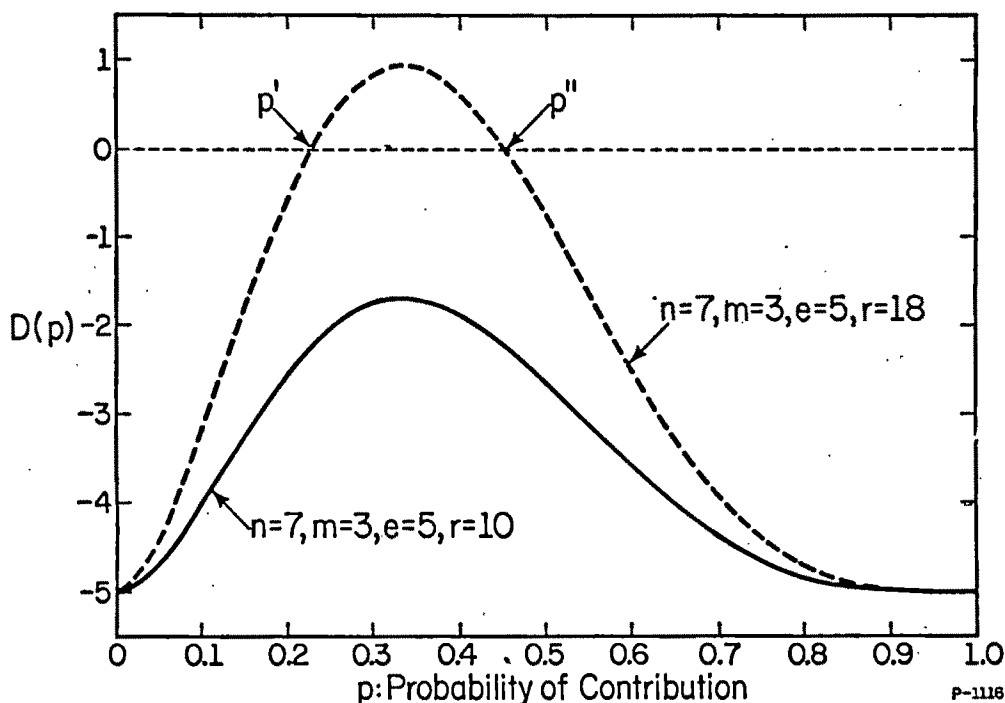
Substituting equation (2) for P_{m-1} in equation (1) and writing $D(p)$ for D to express the dependency of the difference between the expected values $EV(C)$ and $EV(\bar{C})$ on p , yields

$$D(p) = r \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m} - e. \quad (3)$$

The function $D(p)$ in equation (3) is unimodal. It reaches its minimum of $-e$ when either $p = 0$ or $p = 1$ (see Figure 1) and its maximum at $p^* = (m-1)/(n-1)$. As shown in Figure 1, if a horizontal line is plotted at 0, $D(p)$ will fall below the line, touch the line in a single point, or cross the zero line at two points. It follows that if $D(p^*) \leq 0$, player i should never contribute for any value of p . If $D(p^*) > 0$, player i should contribute only if $p' \leq p \leq p''$, where p' and p'' ($0 < p' \leq (m-1)/(n-1) \leq p'' < 1$) are the solutions of the equation

$$r \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m} - e = 0.$$

Because the equation above has at most two solutions, the homogeneity assumption prescribes a closed range $[p', p'']$ when $D(p^*) > 0$. A player should not wish to contribute if too

Figure 1. Two Examples of the Expected Value Difference Function $D(p)$ under the Homogeneity Assumption

few others are expected to contribute (thus wasting her contribution) or if too many others are expected to contribute (she might as well take a free ride).

Example. In the experiments reported by van de Kragt et al., $e = \$5$, $r = \$10$, $n = 7$, and either $m = 3$ or $m = 5$. The solid line in Figure 1 depicts the function $D(p)$, which was computed from equation (3) for $e = 5$, $r = 10$; $n = 7$, and $m = 3$. Because $D(p^*) = D(1/3) = -1.708$, player i should never contribute, regardless of her expectations about the behavior of the other players. The function $D(p)$ for $m = 5$ has the same shape as in Figure 1, with p and $1 - p$ reversed, and is therefore not plotted. Because $D(p^*) = D(2/3) = -1.708$ for $m = 5$, player i should again never contribute in this experimental condition.

To show the dependency of the result reported by van de Kragt et al. on the actual parameters they used, suppose that the value of the public good in their experiment is changed from $r = 10$ to $r = 18$, whereas the other parameters remain the same. The broken line in Figure 1 portrays the function $D(p)$ for $e = 5$, $r = 18$, $n = 7$, and $m = 3$. $D(p)$ reaches its maximum at $P^* = 1/3$ as before. However, it now intersects the zero line at two points: $p' =$

0.23 and $p'' = 0.45$. When r is increased from 10 to 18, a maximization of expected value policy dictates to contribute if $0.23 \leq p \leq 0.45$ and not to contribute otherwise.

Under the heterogeneity assumption, a sample of size $n - 1$ is drawn by binomial sampling from a Bernoulli process whose singular parameter is not a fixed constant p but rather a random variable \bar{p} distributed according to a beta distribution with two parameters $\alpha > 0$ and $\beta > 0$. Then the unconditional distribution of the random variable \bar{m} is a compound distribution known as the beta-binomial (Raiffa & Schlaifer, 1961) or Polya-Eggenberger (Johnson & Kotz, 1969) distribution with the probability mass function

$$f_{\beta\beta}(m|\alpha, \beta, n-1) = \int_0^1 f_b(m|p, n-1) f_{\beta\beta}(p|\alpha, \beta) dp$$

$$= \frac{(m+\alpha-1)!(\beta+n-m-2)!(n-1)!(\alpha+\beta-1)!}{m!(\alpha-1)!(n-m-1)!(\beta-1)!(\alpha+\beta+n-2)!} \quad (4)$$

where $n = 1, 2, \dots$; $m = 0, 1, \dots, n-1$; α and β are positive integers.

Computing P_{m-1} from equation (4), substituting it for P_{m-1} in equation (1), and then writing $D(\alpha, \beta)$ for D in equation (1) to express

the dependency of the difference $EV(C) - EV(\bar{C})$ under the heterogeneity assumption on the distribution of \bar{p} , yields

$$D(\alpha, \beta) = r \left[\frac{(m+\alpha-2)!(\beta+n-m-1)!(n-1)!(\alpha+\beta-1)!}{(m-1)!(\alpha-1)!(n-m)!(\alpha+\beta+n-2)!} \right] - e. \quad (5)$$

When the probability distribution of \bar{p} is uniform ($\alpha = \beta = 1$), $f_{\beta b}$ in equation (4) reduces to $1/n$ and equation (5) reduces to $D(1, 1) = r/n - e$.

Example. Suppose that $e = 5$, $r = 10$, and $n = 7$ as in the van de Kragt et al. experiment, and that player i 's expectations regarding the decisions of her co-players are appropriately represented by a beta distribution with parameters $\alpha = 2$ and $\beta = 4$. The beta distribution $B(2, 4)$ is unimodal and negatively skewed with mean $\alpha/(\alpha + \beta) = 0.333$ and standard deviation $(\alpha\beta/[(\alpha + \beta)^2(\alpha + \beta + 1)])^{1/2} = 0.178$. The mean and standard deviation of the number of

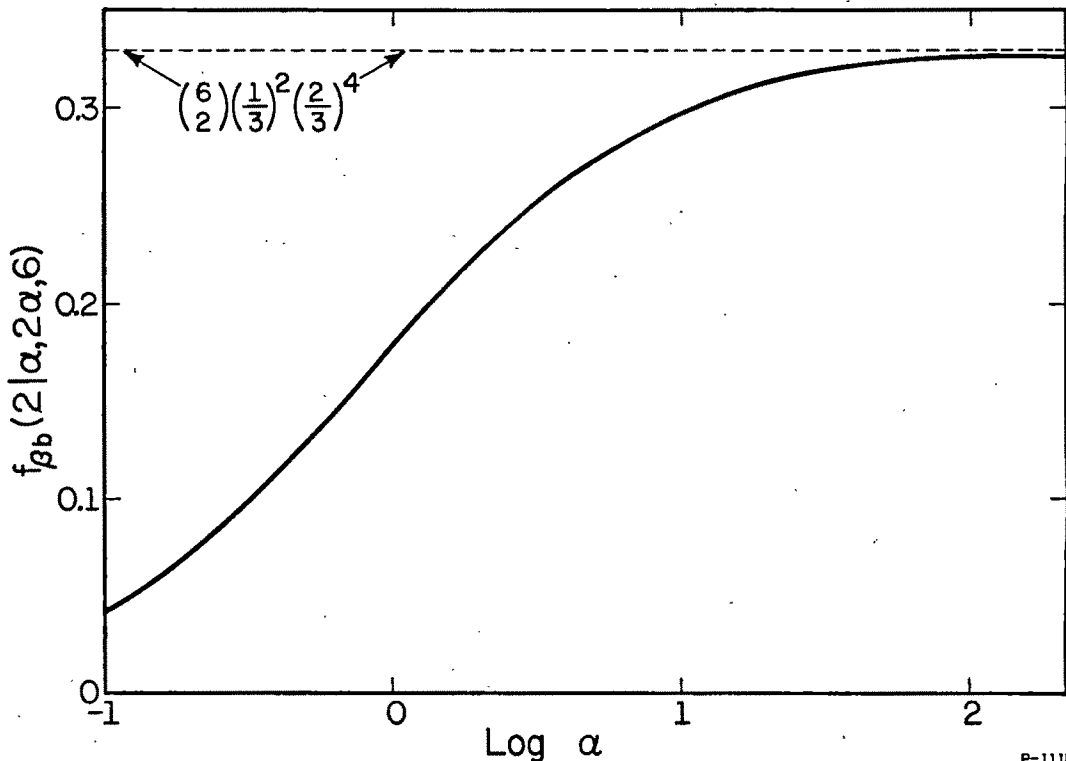
contributors, \bar{m} , are 2 and 1.512, respectively (Raiffa & Schlaifer, 1961, chap. 7). If three or more contributors are required for the provision of public goods ($m = 3$), $P_2 = 0.2273$ and $D(2, 4) = -2.727$, whereas if $m = 5$, $P_4 = 0.0519$ and $D(2, 4) = -4.481$. Because $D(2, 4) < 0$, player i should not contribute in either of these two cases.

Under the heterogeneity assumption, the probability of player i being critical is limited from below by 0 and from above by

$$\binom{n-1}{m-1} \left(\frac{\alpha}{\alpha+\beta} \right)^{m-1} \left(\frac{\beta}{\alpha+\beta} \right)^{n-m}. \quad (6)$$

Consequently, $-e < D(\alpha, \beta) < D(p = \alpha/(\alpha + \beta))$, where $D(p)$ and $D(\alpha, \beta)$ are given by equations (2) and (5), respectively. When both α and β approach 0, the beta distribution approaches a limiting two-point distribution with mass divided between $p = 0$ and $p = 1$. In this case, P_{m-1} ($1 < m < n - 1$) approaches 0 and $D(\alpha, \beta)$ approaches $-e$. Conversely, when α and β increase without bound, the beta distribution approaches a point distribution with all the mass on $p = \alpha/(\alpha + \beta)$. In this case, P_{m-1}

Figure 2. The Beta-Binomial Probability Mass Function Plotted as a Function of $\log \alpha$



approaches the expression in equation (6) and $D(\alpha, \beta)$ approaches $D(\alpha/(\alpha + \beta))$. Figure 2 depicts $f_{\beta b}(2|\alpha, 2\alpha, 6)$ plotted against $\log \alpha$. As α increases, $f_{\beta b}(2|\alpha, 2\alpha, 6)$ is seen to approach P_{m-1} in equation (2) with $n = 7$, $m = 3$, and $p = \alpha/(\alpha + 2\alpha) = 1/3$.

There are situations in which the unorganized group faced with the problem of the provision of public goods is divided into two or more subgroups, which differ or are perceived to differ from one another in their willingness or ability to contribute. Education, wealth, political attitude, age, or sex may either individually or collectively define such groups. For example, people with a college education may be more willing to contribute money to public radio than people without a college education; people who reside under or strongly believe in a centralized government may be less willing to donate blood or contribute to charity than people residing under or strongly believing in a decentralized government (Taylor, 1976); and subjects in an MCS experiment may share the belief that the females in the group are more likely to contribute their endowments than the males. In such cases, neither the homogeneity assumption nor the heterogeneity assumption would describe particularly well the expectations each member i has regarding the decisions of the remaining members of his or her group. Rather, the set N may be partitioned into subsets whose members are homogeneous among themselves, but behave independently of members of other sets (Straffin, 1978).

Formally, a *partial homogeneity structure* of N is a partition $P = (S_1, \dots, S_g)$ of N into g distinct subsets. A reasonable assumption is the following:

P-partial Homogeneity Assumption. Numbers q_h ($h = 1, \dots, g$) are selected independently from $[0, 1]$ and $p_j = q_h$ whenever $j \in S_h$. Given this assumption, the difference in expected value between contributing and not contributing may be computed as in the previous two cases.

Example. To illustrate the *P-partial-homogeneity* assumption, suppose that $n = 7$ and $m = 5$ as before, and that the group N is divided into two ($g = 2$) subsets, with $|S_1| = 3$ and $|S_2| = 4$. I write $D_h(P)$ for D in equation (1) to express the dependency of the difference $EV(C) - EV(\bar{C})$ under the partial homogeneity assumption on both the vector P and the group (subset) h .

Assume first that $i \in S_1$. Then of the remaining six members, two are in group S_1 and four are in group S_2 . To have a total of four contributors, there must be either zero

contributors from Group S_1 and four from S_2 , one contributor from S_1 and three from S_2 , or two contributors from S_1 and two from S_2 . Therefore,

$$\begin{aligned} D_1(P) &= rP_{m-1} - e \\ &= r[(\binom{2}{0}q_1^0(1-q_1)^2 \cdot \binom{4}{4}q_2^4(1-q_2)^0 \\ &\quad + \binom{2}{1}q_1^1(1-q_1)^1 \cdot \binom{4}{3}q_2^3(1-q_2)^1 \\ &\quad + \binom{2}{2}q_1^2(1-q_1)^0 \cdot \binom{4}{2}q_2^2(1-q_2)^2] - e \\ &= r[(1-q_1)^2q_2^4 + 8q_1(1-q_1)q_2^3 \\ &\quad + 6q_1^2q_2^2(1-q_2)^2] - e. \end{aligned} \quad (7)$$

Assuming next that $i \in S_2$, I obtain in a similar manner

$$\begin{aligned} D_2(P) &= r[3q_1q_2^3(1-q_1)^2 + 9(q_1^2-q_1^3)(q_2^2-q_2^3) \\ &\quad + 3q_1^3(1-q_2)^2] - e. \end{aligned} \quad (8)$$

With $e = 5$, $r = 10$, and $n = 7$ as before, equations (7) and (8) yield $D_1(P) = -2.037$ and $D_2(P) = -2.613$.

If $q_1 = q_2 = p$, then equations (7) and (8) reduce to (3) with $n = 7$ and $m = 5$.

Contributions in the van de Kragt et al. Experiment

Recall that in both conditions of the van de Kragt et al. experiment ($m = 3$ and $m = 5$) the public good was provided to 65% of the 7-member groups. Moreover, in most of the successful groups inefficiency occurred because of overprovision. Yet, whether the subjects' expectations in the experiment of van de Kragt et al. are most appropriately captured by the homogeneity assumption, the examples above show that the subjects should have never contributed. Following Dawes's (1980) discussion of cooperative behavior in social dilemma situations, the explanation that I favor for contribution in the MCS task is based on expected utility rather than expected value maximization. Individuals in decision-making tasks with externalities, including the subjects in the van de Kragt et al. experiment, have utilities that determine their behavior. These utilities are associated with aspects of behavior, such as altruism, social norms, and moral conventions, other than the external payoffs. In the MCS task (Table 1), the terminal payoff assumes one of four values: 0, e , r , or $r + e$. Because $r + e > r > e > 0$ and the utility

function is unique up to a linear transformation, I can set $u(0) = 0$ and $u(e+r) = 1$ with no loss of generality. The assumption that $u(r)/u(e) > r/e$ seems reasonable; it is in agreement with what is known about the existence of altruism, the operations of the social norm of cooperativeness, and the dictates of conscience. Given this assumption about the convexity of the utility function in the MCS task, the decision of whether to contribute may be explained by an appropriate selection of $u(r)$ and $u(e)$. This explanation is tautological unless $u(r)$ and $u(e)$ can be assessed independently.

To illustrate this explanation, suppose that $u(x) = ax^2$, where a is selected so that $u(0) = 0$ and $u(e+r) = 1$. For the parameters $e = 5$ and $r = 10$ in the van de Kragt et al. experiment, I obtain $a = 1/225$, $u(0) = 0$, $u(e) = 1/9$, $u(r) = 4/9$, and $u(r+e) = 1$. Substituting $u(r)$ and $u(e)$ above for r and e in equation (3), I have for $n = 7$ and $m = 3$,

$$D(p) = 4/9 \binom{6}{2} p^2 (1-p)^4 - 1/9,$$

with $D(p^*) = D(1/3) = 0.035 > 0$.

Social Implications

What sort of mechanisms may be devised by society to resolve the problem of the provision of lumpy public goods for small groups as abstracted by the MCS paradigm? An examination of equations (3), (5), (7), and (8) suggests that contribution toward the provision of public goods may be engendered by several complementary approaches, which fall in the realms of economics, education, and psychology. I sketch below three approaches, the first two of which have already been described and discussed for the related but more ubiquitous class of social dilemmas (Dawes, 1980; Messick & Brewer, 1983).

Like social dilemmas, the problem of public goods provision is defined in terms of the social payoff structure. Equations (3), (5), (7), and (8) suggest that the simplest proposal for engendering contribution is to change that structure by economic means that increase the r/e ratio. A change in the payoff structure would be produced by increasing the social benefit, by decreasing the size of the contribution, or by both. However, there are always practical considerations which limit the size that the ratio r/e may reach, and there is always the problem of who will institute the change and how (Dawes, 1980).

If, as argued above, the ratio of utilities $u(r)/u(e)$ rather than the ratio of payoffs r/e

matters, contribution could be enhanced by appeals to conscience, by rendering social norms more prevalent, by stressing the importance of altruism for society, and by depreciating self-motivation and selfishness. All of these mechanisms for eliciting cooperative behavior have been the subject of much theoretical discussion in education and practical use in religion.

Desirable as it may be, increasing the $u(r)/u(e)$ ratio may not be sufficient for ensuring contribution. For example, if $p = 1$, the individual should never contribute no matter how large is the $u(r)/u(e)$ ratio. Equations (3), (5), (7), and (8) show that, in addition to increasing the $u(r)/u(e)$ ratio, each individual's expectations about the decisions of the other members of his or her group should also be influenced, controlled, or manipulated. The experimental literature concerning attitude change may suggest mechanisms for changing belief structures. The analysis of the homogeneity, heterogeneity, and partial homogeneity models conducted above shows that individuals are more likely to contribute when they perceive their group as homogeneous: $D(\alpha/(\alpha+\beta))$ is always greater than $D(\alpha,\beta)$. Similarly, if the equality $q_1 = \dots = q_n$ in the partial homogeneity model is not satisfied, there exists a probability number, p^* , such that $D(p^*) > D_r(p)$. Hence, the first step in the third proposal is to convince each player that, with regard to the probability of contributing toward the provision of public goods, all the other members of his or her group are identical. The second step concerns the manipulation of p . Convincing each individual of the homogeneity of his or her group may not ensure contribution. As shown in Figure 1, even if $D(p^*) > 0$, contribution is optimal for some values of p (e.g., p^*) and not optimal for others (e.g., $p = 0$, $p = 1$). An implication of equation (3), which is experimentally testable, is that contribution toward the provision of public goods is most likely to emerge if each player is led to believe that the probability that any of the other players will contribute is at or very close to $p^* = (m-1)/(n-1)$.

Although I know of no direct experimental test of the above implication, the experimental results reported by van de Kragt et al. seem to support it. Van de Kragt et al. reported that subjects in the no-discussion condition "were, to some extent, tailoring their expectations about the incidence of contributing to the level of contributing that was required for public good provision" (1983, p. 118). My analysis of the homogeneity model similarly suggests that the expectations about the number of con-

tributors by a player who actually contributed will equal $(n-1)p^* = m-1$.

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The War Trap Revisited: A Revised Expected Utility Model

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The expected utility framework developed in The War Trap is revised to correct several deficiencies. Risk-taking orientations are now treated as an integral part of the model by introducing concavity or convexity into the utility functions. The zero-sum properties of the theory are largely eliminated, and the tendency toward interpersonal comparisons of utility is removed. Several earlier results are replicated with the new model, and with annual capabilities data. New propositions are deduced that identify important limitations on conflict initiation, and relationships resulting from differences in perceptions are tested. Support is found for the contention that the revised version of the theory, of which the original model is a special case, is a powerful tool for integrating many extant hypotheses about conflict and for explaining a substantial portion of the tendency for some threats to escalate to violence or warfare and for others to be resolved peacefully.

An expected utility approach to the study of international politics offers both the opportunity to deduce propositions about international conflict, and, through the application of admittedly crude indicators, to evaluate the usefulness of those propositions as explanations of actual behavior (Altfeld & Bueno de Mesquita, 1979; Berkowitz, 1983; Bueno de Mesquita, 1981a, 1983, 1984a; Morrow, 1982; Newman, 1982; Petersen, 1983; Wittman, 1979). Many studies that have applied such a framework to international conflict have used the model proposed in *The War Trap*. Although that model has proven helpful in explaining both intuitively obvious cases of international disputes and seemingly counterintuitive ones, still it possesses several serious shortcomings. I intend, therefore, to correct some of those shortcomings by proposing a refined version of the expected utility formulation set out in *The War Trap*. In doing so, I do not wish to suggest that the revised model corrects all of the weak-

esses in the earlier approach. Quite the contrary. Much still remains to be done, and work is going forward on improving the models further.¹ However, it seems useful at this juncture to introduce the modifications developed to date and some empirical analyses associated with them, so that others may react to the modifications and perhaps add to them.

As was pointed out in *The War Trap*, the weakest theoretical component of the expected utility approach as I constructed it was the establishment of four *ad hoc* decision rules used to specify necessary conditions for the initiation of conflict by risk takers or risk avoiders. Although these rules are generally consistent with the flavor of the concepts they are intended to capture, they represent a serious departure from standard treatments of risk.

My main objective here is to reconstruct the model so that it reflects risk through the introduction of concavity or convexity into the utility functions. In doing so, it is imperative that the model give each actor the opportunity to have a differently shaped utility function, with the extremity of the function's curvature embodying the extremity of the decision maker's willingness (or reluctance) to take chances.

A second objective, closely associated with the first, is to build greater protection against interpersonal comparisons of utility into the model (Zagare, 1982). Propositions concerned with the

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¹Appendix 1 contains the decision tree for a revision currently under investigation. All the results deduced from the current version of the theory can be deduced from this newer version. However, the version depicted in Appendix 1 is as yet untested.

escalation of conflict are best evaluated when one can estimate separately the perceptions of the key leaders in i and j . The revised model allows us to do exactly that. We can now estimate how each actor's situation looks as seen through another actor's eyes. Through this development it is possible to calculate the effects of differing perceptions on conflict decision making. Furthermore, it is possible to investigate an actor's decisions not only in isolation, but also in the context of his thinking about what he can do, and what his opponent can do in response (Maoz, 1983). In this way, the revised theory allows us to examine interactive decisions as well as independently made choices.

The alterations alluded to above have the additional characteristic that they greatly reduce the empirical tendency of the earlier model to produce zero-sum results in which i expects to gain exactly what j anticipates losing (Maoz, 1983). Although the theory as originally specified is not inherently zero-sum, still about 70% of the conflicts I studied yielded expected utility values that sum to zero. With the revised model, fewer than 10% of the cases sum to zero.²

Conceptualizing Risk-Taking in the Utility Functions

The central modification of the theory that is explored in this study is the introduction of an endogenously derived, continuous measure of risk-taking propensities. This measure permits the development of a potentially unique curvature to each nation's utility function for each choice situation in which it finds itself.

To evaluate risk-taking propensities I assume that leaders declare policy positions that represent some compromise between what they really want and what they believe is pragmatic or feasible. In particular, I assume that what one perceives is feasible is never more extreme than what one ideally wants. Then, using the original formulation in *The War Trap*, it is possible to estimate the degree to which i 's current policies leave i vulnerable to defeat, where such vulnerability is taken

to be an indicator of the feasibility of the policies being pursued. In particular, I define each nation's security level as $\sum_{j \neq i} E(U_{ji})$.³ The

greater this sum, the more utility i believes its adversaries expect to derive from challenging i . As this sum gets smaller and turns negative, i increasingly is in a position to extract concessions from j , and j is increasingly seen to be incapable of challenging i . In other words, as this sum decreases, i 's relative security increases, so that i is assumed to have adopted safe policies somewhat at the expense of i 's more extreme ideal.

One can identify the hypothetical policy portfolio that would maximize i 's security level (i.e., $\sum E(U_{ji})_{\min}$), and the hypothetical portfolio that leaves i most vulnerable to defeat (i.e., $\sum E(U_{ji})_{\max}$). How proximate i 's actual policies are to these extremes of vulnerability, in turn, may be taken as a reflection of i 's willingness to take risks. In particular, I assume that i 's risk acceptance increases as i 's security score approaches its level of greatest vulnerability, and that i 's risk aversion increases as its security approaches the level possessed by its safest policy portfolio. This risk-taking propensity is defined as:

$$R_i = (2\sum E(U_{ji}) - \sum E(U_{ji})_{\max} - \sum E(U_{ji})_{\min}) / [\sum E(U_{ji})_{\max} - \sum E(U_{ji})_{\min}].$$

This term is then transformed to:

$$r_i = [1 - (R_i/3)] / [1 + (R_i/3)] \quad (1)$$

so that r_i ranges between 2 and .5.⁴ As r_i gets larger, i 's aversion to risks increases.

²The computation of expected utilities used to define the risk component is exactly the computational procedure found in *The War Trap* in which utilities are, for the moment, treated as if they are strictly a function of the similarity in foreign policy commitments of various actors. The utility functions will then have curvature introduced as a function of the proximity of each nation's actual policies to their safest and most dangerous alternatives. I am indebted to David Newman for suggesting this conceptualization of security. He has demonstrated that nations select alliance partners in a manner consistent with the notion that they are attempting to maximize their security as defined here (Newman, 1982).

⁴In the calculation of r_i it is necessary to transform R_i so as to prevent division by zero. That is why the equation calls for dividing R_i by 3. The particular divisor that is chosen will affect the rate at which the curvature of the utility function changes, but will not affect the direction of curvature. This measurement procedure for

³Furthermore, all the cases that sum to zero in the revised construction involve situations where $u_{ij} = U_{ij} = 1$, so that the respective expected utilities equal zero, meaning that the nations in question were extremely close allies. For these nations the unmeasured anticipated change in policy almost surely would have indicated deteriorating relations and, therefore, increased expected utility from waging war. Thus, their expected utilities, if fully estimated, would not have equalled zero, but would have been positive. For the logic underlying this statement see *The War Trap*, pp. 75-78.

The utility for success and the utility for failure may now be defined. Both of these utilities are assumed to be a function of the similarity of policy preferences across actors and the level of willingness to take risks within each actor. With U_{ji}^l being equal to the value i attaches to his own policy portfolio,³ and with U_{ij}^l being equal to the value i attaches to j 's policies as a function of their similarity to the policies of i , I may define the utility for success and failure respectively as:

$$U_{si}^l = 2 - 4[2 - (U_{ji}^l - U_{ij}^l)/4]^{r_i} \quad (2)$$

and

$$U_{fi}^l = 2 - 4[2 - (U_{ij}^l - U_{ji}^l)/4]^{r_i} \quad (3)$$

Similarly, we may define the utility actor i attaches to the policy changes by his adversary that i anticipates will occur in the absence of a challenge by i as:

$$U_{qi}^l = 2 - 4[2 - [(U_{ji}^l - U_{ij}^l)_m - (U_{ji}^l - U_{ij}^l)_{i0l})/4]^{r_i}$$

or equivalently, given that no change in policy is assumed,

$$U_{qi}^l = 2 - 4[(2-0)/4]^{r_i} \quad (4)$$

Of course, the U_{ji}^l , U_{ij}^l and U_{qi}^l terms (with appropriate superscripts) are defined analogously. These terms vary as a function of whose estimate of expected utility is being calculated (i.e., who is the superscripted actor) by varying the risk exponent, so that for expected utility equations with an i superscript, calculations are done as specified above. For equations with a j superscript, j 's risk-taking propensity is used to estimate what j perceives to be the value of success, failure, or no

challenge for i in accordance with the equations delineated below.

I assume that for any superscripted actor $U_{si}^l > U_{qi}^l > U_{fi}^l$ (and equivalently for j), so that winning is better than or equal to no effort to change an adversary's policies.⁷ And not changing an adversary's policies is at least as good as trying and failing. That the definitions specified above are consistent with the structural assumptions found in standard treatments of risk taking is clear from the hypothetical utility functions based on these definitions depicted in Figure 1.

For the multilateral component of the expected utility equations which are developed below as extensions from *The War Trap*, risk is introduced into the utility functions through the following transformation:⁸

$$(U_{ki}^l - U_{kj}^l)' = (U_{ki}^l - U_{kj}^l)e^{RK(U_{ki} - U_{kj})} \quad (5)$$

This functional form assumes that risk averters undervalue support from friends and overvalue opposition from foes, whereas risk accepters overvalue support from friends and undervalue opposition from foes. Figure 2 depicts the effects of the multilateral risk-taking function for risk accepters, risk averters, and risk-neutral decision makers.

The Revised Model

The revised formulation is:

$$\begin{aligned} E^l(U_{ij}) = & [P_i(U_{si}^l) + (1 - P_i)(U_{fi}^l)] \\ & + \sum_{k \neq i, j} (P_{jk} + P_{jk} - 1)(U_{ki}^l - U_{kj}^l)' \\ & - [Q_{qi}^l U_{qi}^l + (1 - Q_{qi}^l Q_{bi}^l)(U_{bi}^l) \\ & + (1 - Q_{bi}^l)(U_{wi}^l)] \end{aligned} \quad (6)$$

estimating risk-taking propensities was developed in close collaboration with Michael Horn and James Morrow, whose invaluable assistance I acknowledge.

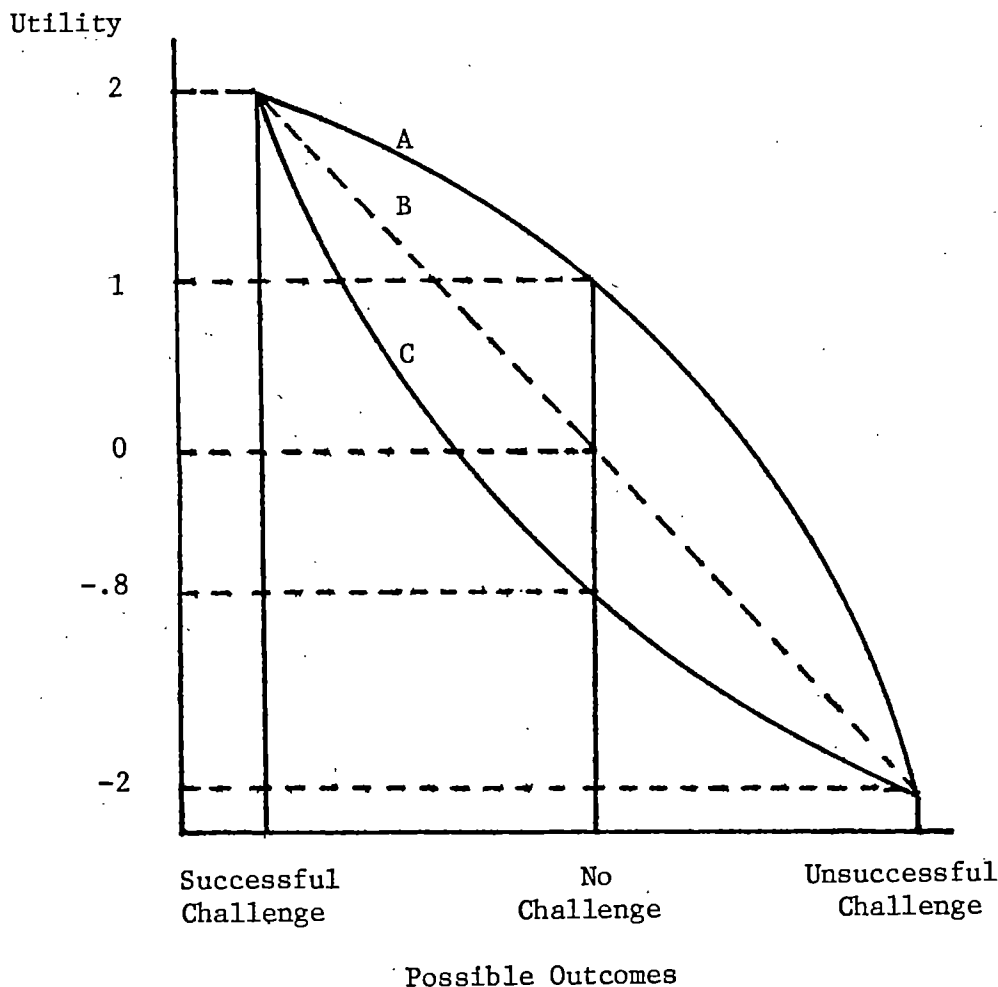
³Both U_{ji}^l and U_{ij}^l are assumed to equal 1.0, with U_{ji}^l and U_{ij}^l ranging between possible values of 1.0 and -1.0.

⁴As in *The War Trap*, all U_{ij}^l terms refer to the degree to which i and j share common foreign policy commitments. U_{ji}^l terms equal the value i attaches to its own policies, a value I define as being equal to 1.0. Once the risk-taking component is combined with these variables, the appropriate utility function is defined. The reason for the transformations by 2's and 4's is to preserve the original scale of numbers while avoiding the generation of imaginary numbers. Because r_i can be less than 1.0, the absence of such transformations would mean that for negative values of, for instance, U_{ji}^l , no real root would exist. This problem is eliminated with the introduction of these transformations.

⁷We may also say that $U_{si}^l, U_{bi}^l > U_{fi}^l$ and $U_{si}^l > U_{fi}^l$, although the relationship between U_{si}^l and U_{bi}^l is situation dependent, as is the relationship between U_{wi}^l and U_{fi}^l . This situational dependence adds considerable richness to the model, allowing decision makers to conclude, for instance, that a challenge is warranted even though defeat is expected. Such a situation arises if the expected value of j 's policy changes in the absence of a challenge is, from i 's perspective, inferior to the expected value for i of challenging j and losing. This, of course, can happen if i is able to impose sufficient costs on j to limit the amount by which j can alter its policies in the face of i 's opposition.

⁸In evaluating equation (5), the reader should keep in mind that all U_{ki}^l and U_{kj}^l are as defined in *The War Trap*, with the risk-taking component representing the innovation introduced here. That component is denoted by the use of a ' in the equation.

Figure 1. The Effects of Risk-Taking on the Curvature of the Utility Functions



Let $U_{aj} = U_{bj} = U_{cj} = -1$. Let $ra = 2$, $rb = 1$, $rc = .5$.

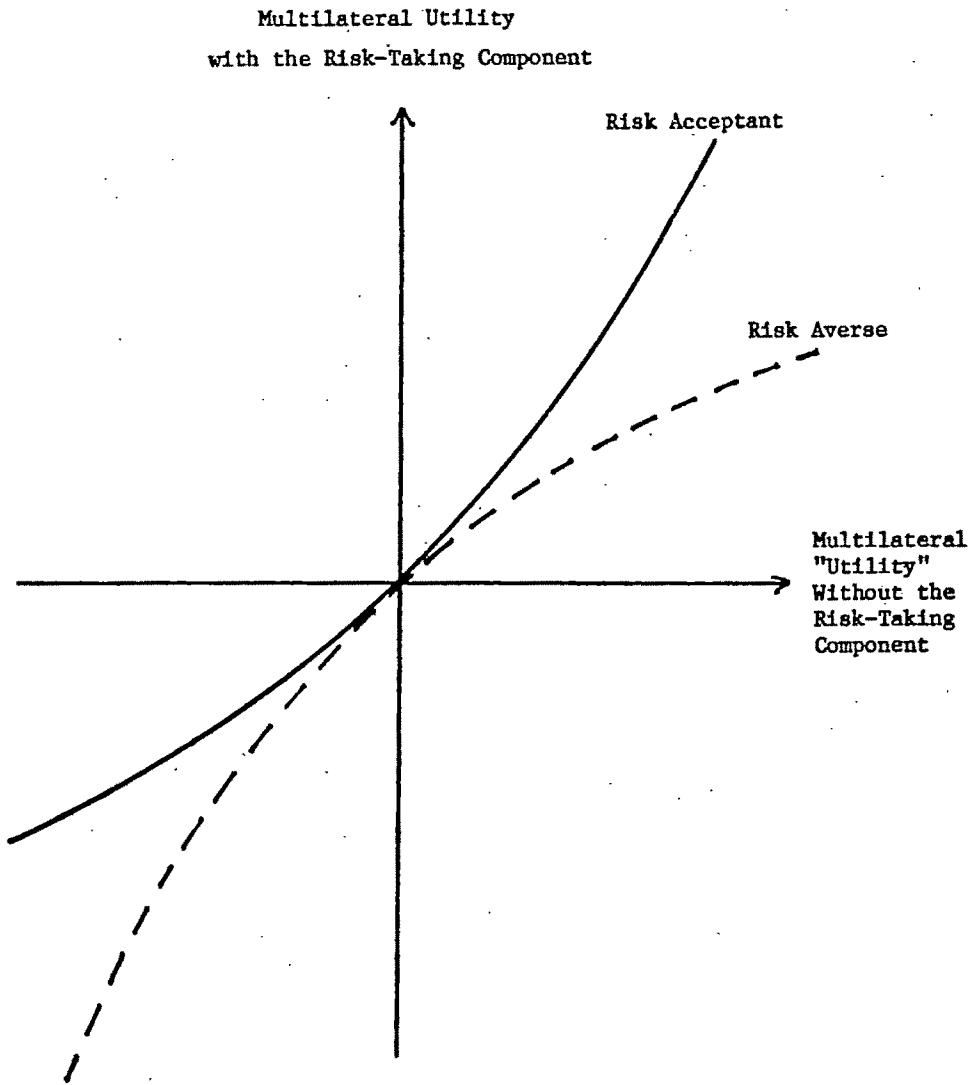
Let each actor's expected utility = the expected utility from challenging j minus the expected utility from not challenging j . Assume the probability of success from a challenge = .5, and assume the no challenge alternative yields the expectation of no change in policy by j for sure. Then:

$E^A(U_{Aj}) = 0 - 1 = -1$. Therefore No Challenge is preferred to the lottery.

$E^B(U_{Bj}) = 0 - 0 = 0$. Therefore (Challenge) Indifferent (No Challenge).

$E^C(U_{Cj}) = 0 - (-.8) = .8$. (Challenge) Preferred to (No Challenge).

Figure 2. Multilateral Utility: The Effect of Risk Taking on the Function



$$\begin{aligned}
 E^j(U_{ij}) = & [P_j(U_{sj}^j) + (1 - P_j)(U_{fj}^j)] \\
 & + \sum_{k \neq i, j} (P_{jk} + P_{jk} - 1)(U_{kj}^j - U_{ki}^j) \\
 & - [Q_{qi}^j U_{qi}^j + (1 - Q_{qi}^j)(Q_{bj}^j(U_{bj}^j) \\
 & + (1 - Q_{bj}^j)(U_{wj}^j))]
 \end{aligned}
 \quad (7)$$

$$\begin{aligned}
 E^i(U_{ij}) = & [P_j(U_{sj}^i) + (1 - P_j)(U_{fj}^i)] \\
 & + \sum_{k \neq i, j} (P_{jk} + P_{jk} - 1)(U_{kj}^i - U_{ki}^i) \\
 & - U_{qi}^i U_{qi}^i + (1 - Q_{qi}^i)(Q_{bj}^i(U_{bj}^i) \\
 & + (1 - Q_{bj}^i)(U_{wj}^i))]
 \end{aligned}
 \quad (8)$$

$$\begin{aligned}
 E^j(U_{ij}) = & [P_j(U_{sj}^j) + (1 - P_j)(U_{fj}^j)] \\
 & + \sum_{k \neq i, j} (P_{jk} + P_{jk} - 1)(U_{kj}^j - U_{ki}^j) \\
 & - [Q_{qi}^j U_{qi}^j + (1 - Q_{qi}^j)(Q_{bj}^j(U_{bj}^j) \\
 & + (1 - Q_{bj}^j)(U_{wj}^j))]
 \end{aligned}
 \quad (9)$$

where

$E^i(U_{ij})$ = i 's perception of the difference in i 's expected utility from challenging j 's policies and from leaving j unchallenged. That is, this

term represents i 's expectation of its net benefit (or loss) from challenging j .

$E^i(U_{ij})$ = i 's perception of the difference in j 's expected utility from challenging i 's policies and from leaving i unchallenged.

$E^j(U_{ji})$ and $E^j(U_{ij})$ have analogous interpretations, but from j 's perspective.

P_i = i 's probability of succeeding in a bilateral contest with j . P_j is j 's probability of defeating i in a bilateral contest.

$(P_{ik} + P_{jk} - 1)$ = the marginal effect of third party k on the probability of i or j succeeding. This, and the associated utility terms, represent the algebraically reduced form of two lotteries, one in which k is assumed to join i , and one in which k is assumed to join j . Since on balance these are mutually exclusive alternatives (as are, for instance, i 's decision to select the strategies of challenging or not challenging j), the net impact of these two lotteries represents k 's marginal effect.⁹

U^i_{ki} = the value i believes is gained from support from k .

U^i_{kj} = the value i believes j gains from k 's support.

$U^i_{ki} - U^i_{kj}$ = the net value i believes will be contributed by k to the contest between i and j .

Analogous terms with j as the superscripted actor refer to j 's perception of k 's value to i and j respectively.

The U_s terms refer to the utility of success for the subscripted actor as perceived by the superscripted actor in the event the subscripted actor challenges the relevant adversary in a bilateral dispute. Thus, U^j_{si} is j 's perception of i 's utility for succeeding in forcing j to change its policies to be in accord with i 's wishes. The U_f terms are analogous to the U_s terms, except that U_f refers to the utility the superscripted actor believes the subscripted actor attaches to being defeated after its initiation of a bilateral challenge. These utilities are a function of the similarity in policies manifested by i and j , and of the risk-taking propensity of the superscripted actor.

The Q_q terms are the probability of the subscripted actor maintaining its current policies in the absence of a challenge by the other actor, with the estimate of that probability being made by the superscripted actor. For simplicity's sake, I will

assume throughout the rest of this study that the Q_q terms equal 1.0, so that the no-challenge lottery reduces to the assumption that in the absence of a challenge one's adversary is anticipated to maintain its existing policies.

The U_b and U_w terms refer to the utility the superscripted actor perceives that the subscripted actor attaches to some anticipated improvement or worsening of existing policy in the absence of a demand for policy change, whereas the U_q terms refer to the utility the superscripted actor perceives the subscripted actor attaches to no change in policy by its potential adversary. Despite the restrictive assumption that the relevant decision maker anticipates no change in its opponent's policies, the new risk-taking procedure that is introduced into the calculation of utilities preserves the existence of a distinct gambling threshold for each nation in each situation in which it finds itself.¹⁰ The expected utility associated with the no-challenge option represents the relevant decision maker's gambling threshold.

Thus, the expected utility from the challenge option must exceed the expected utility from the no-challenge option in order for a rational decision maker to choose to initiate an international dispute.

Equations (6) through (9) are equivalent to the original models in *The War Trap*, except that a) the utility functions are altered to allow concavity or convexity; b) the no-challenge side of the equation is modified so that a situation-specific gambling threshold is now theoretically defined for each nation; and c) analyses of the effects of perceptual differences, as reflected in the risk-taking component of the utility function, are now possible.

Derivations

For risk-neutral decision makers, equations (6) through (9) permit the derivation of all of the propositions found in *The War Trap*. In the absence of that constraint, however, these new equations can yield additional deductions. I turn now to the development of such propositions.

In *The War Trap* I showed that in order for i to be the initiator of a dispute in conflicts expected to remain strictly bilateral, it was necessary that i be as strong or stronger than j , so that $P_i \geq (1 - P_j)$. For strictly risk-neutral or risk-averse

⁹That $(P_{ik} + P_{jk} - 1)$ is the marginal contribution of k to the probability of success by i or j is easily shown. $P_i + (1 - P_i) = 1$. Let P_j (the probability that j succeeds in the bilateral contest) = $(1 - P_i)$. $P_i < P_{jk}$ since P_{jk} = the probability i succeeds given support from k . Similarly $P_j < P_{ik}$. Thus, since $P_i + P_j = 1$, $[P_{ik} + P_{jk} - (P_i + P_j)]$ must be the contribution of k .

¹⁰For a discussion of when this assumption is problematic, the reader should see *The War Trap*. The reader might also note that a measurement procedure for the no-challenge lottery has been developed and is now being tested. Results of those analyses will be reported in subsequent research.

decision makers that statement remains true whether one applies the original formulation or the revision proposed here. However, for risk-acceptant decision makers, it is not the case that initiation is dependent on $P_i > (1 - P_j)$. In general, assuming no third-party intervention, one may say that initiation requires that: $[P_i(U_{si}^i) + (1 - P_i)(U_{ji}^i)] > [U_{qi}^i]$ which, solving in terms of P_i , means

$$P_i \geq [(U_{qi}^i - U_{ji}^i)/(U_{si}^i - U_{ji}^i)] \quad (10)$$

Equation (10) is a necessary, although not sufficient, condition for the initiation by i of a conflict expected to remain strictly bilateral. Of course, equivalent relationships hold for equations (7) through (9). From this derivation one can see that as the value that i attaches to the status quo diminishes, approaching in the limit the value that i attaches to failure, i 's expected utility from conflict exceeds the expected utility from not challenging j even as $P_i \rightarrow 0$. Or, put differently, as the potential benefits from not challenging j (i.e., the numerator in equation (10)) increase, approaching in the limit, the potential stakes from challenging j , in order for i to initiate a dispute, i 's expectation of victory must increase. This is an important result for two reasons.

First, from equation (10) we see that the general law governing the initiation of a conflict expected to remain strictly bilateral does not require, as previously thought, that $P_i > .5$. If i is sufficiently willing to take risks, P_i may be considerably smaller than .5. Second, i 's willingness to take the risk of initiating a conflict when the probability of success is less than .5 means, of course, that under these circumstances i is likely to start a conflict that it will probably lose. This reminds us that the assumption of rationality does not require special insight by decision makers. Nor is that assumption's value properly called into doubt by the observation that some important war initiators suffered severe defeats. Quite the contrary. We can see how the rational actor perspective enhances our ability to understand "bad" decisions and perhaps even to anticipate the likelihood that such decisions will be reached. If, for instance, I substitute a risk-neutral perspective for the risk-acceptant perspective that i actually possesses, I can see precisely how much "excess" utility i derives as a result of looking at the world through the distorting lenses of a risk taker. Then I can estimate whether the "objective" situation would lead to the same expectations as those i possesses as a consequence of i 's willingness to gamble. In some circumstances, I may even be able to reduce the likelihood of conflict by identifying in ad-

vance the magnitude of distortion introduced by risk-taking propensities.¹¹

The solution to equation (10) and other derivations that can be constructed from the expected utility equations demonstrate the limits to bilateral-war initiation. These results show for the first time in a formal, explicit, structured way, just how large a gain or loss, just how large a chance of success or failure, a nation (or its leaders) can tolerate before being dissuaded from waging war. We can see clearly, for instance, that the realist paradigm's focus on power to the nearly total exclusion of all else (Morgenthau, 1973; Vasquez, 1983) is excessive.

Although power imposes real limits on international relations, so too do idealist considerations of policy or ideology, as embedded in the utility terms. We see also that such an individual personality attribute as the willingness to take chances can be incorporated in a systematic analysis of decisions for war or peace. Later I will show that differences in that attribute may be used to derive propositions about the likelihood of conflict escalation. For now, the focus on bilateral conflict should help to remind us that the expected-utility approach, with its focus on the utilities attached to alternative policy positions as well as with its focus on power (through the probability terms) and perceptions (through the risk-taking terms), incorporates much of what is central to many of the competing perspectives on international relations and does so in a theoretically structured, integrative, internally consistent manner.

Turning to the multilateral component of the calculus described in equations (6)-(9), I can extract additional insights that help to explain the conflict process while also demonstrating further the potential of the expected utility framework for integrating many extant theories of war. For instance, such power transition theorists as Organski and Kugler (1980) have argued that alliances are irrelevant when wars are waged between the two most powerful nations. Their reasoning is quite simple. They contend that the two strongest nations are so powerful by comparison with anyone else that allies are reduced to insignificance. They say, "Most of the time alliances are simply not a realistic method of preventing threatening changes in the distribution of world power, given the skewness of relations between the great and the lesser nations, and also among the half-dozen

¹¹In this regard, the model has already been used with considerable success as a tool for forecasting policy decisions in both a research context (Beck & Bueno de Mesquita, 1984; Bueno de Mesquita, 1984) and by government and private-sector organizations.

great powers themselves" (Organski & Kugler, 1980, p. 25). Stated in terms of the multilateral portion of equation (6) (or equivalently, equations (7), (8), or (9)), I can say: Let $P_j = (1 - P_i)$, so that $P_i + P_j = 1$, $P_{ik} > P_i$ and $P_{jk} > P_j$, so that $P_{ik} + P_{jk} > P_i + P_j$.

According to Organski and Kugler, one must assume that $(P_{ik} + P_{jk}) \rightarrow (P_i + P_j)$, where $(P_i + P_j) = 1$, "given the skewness of relations between the great and the lesser nations." Then $(P_{ik} + P_{jk} - 1) \rightarrow 0$. Therefore, $(P_{ik} + P_{jk} - 1)(U_{ki} - U_{kj}) \rightarrow 0$.

Thus, a third party k has no impact on the expected utility of i or j when i and j are dominant powers engaged in conflict. The evidence offered by Organski and Kugler in support of their hypothesis that allies make no difference in power-transition wars is equally well suited as evidence for the expected-utility theory under the power conditions they specify.

Balance-of-power theorists suggest, however, in seeming contradiction to the power-transition argument, that it is exactly when two rivals are about equal in power that allies are critical in tipping the balance in favor of one side or the other, providing the margin of victory (Claude, 1962; Kaplan, 1957; Morgenthau, 1973). Yet the expected-utility theory demonstrates that this apparent contradiction is no contradiction at all. If, unlike Organski and Kugler, I assume that k does have some power, then it must be that $(P_{ik} + P_{jk})$ meaningfully exceeds $(P_i + P_j)$. In that case, barring k 's being indifferent between the policies of i and j , it follows that:

$$(P_{ik} + P_{jk} - 1)(U_{ki} - U_{kj}) \neq 0$$

so that, as suggested by balance-of-power theorists, alliances do make a difference. Thus, the expected-utility theory allows one to reconcile the seemingly contradictory alliance hypotheses of at least two central theories of conflict.

The logic of the multilateral component of the expected-utility formulation also permits us to see why major powers are more likely to become involved in conflict as third parties than are weaker nations:

Let k be a major power, and let i and j be weak nations. Then, in the limit, $(P_{ik} + P_{jk}) \rightarrow 2$, so that $(P_{ik} + P_{jk} - 1) \rightarrow 1$. So long as the difference between U_{ki} and U_{kj} is not zero, one can see that the multilateral component of the calculus is larger when k is assumed to be a major power than when (as in the power transition example), k is assumed to be relatively weak. Therefore, a "powerful" third party is presumed to be able to have an impact on virtually all conflicts, whereas a weak one can have an impact only under a special set of circumstances (in which, for in-

stance, i and j are about as strong as k). Major powers, then, are more likely to play a role in conflicts than are lesser nations (Altfeld & Bueno de Mesquita, 1979).

The revised model demonstrates that war (or lesser conflicts) can be initiated even when losses are expected, so long as those losses are not as large as the liability associated with continuing to live with the existing and anticipated status quo. The gamble of warfare becomes tolerable when the value of the status quo is sufficiently unattractive to offset the losses from war. In that sense, the total expected utility (as defined in equations (6)-(9)) must be positive, indicating either that an actual gain is anticipated or a smaller loss is expected from initiating conflict than from abiding the current situation.

Additional Hypotheses

The formulation stipulated above carries several important behavioral implications. First, since I am still modeling only necessary, and not necessary and sufficient, conditions, I can reiterate that decision maker i can, but need not, choose conflict as a strategy for acquiring policy gains (or reducing anticipated losses) only if equation (6) is not less than zero. This necessary condition is consistent with both the view that the decision maker in question is an expected utility maximizer or an expected utility satisficer. Only the specification of the calculus associated with *all* alternative strategies can allow us to distinguish between these two decision rules. Still, one can say that (H_1) ; equation (6) must be greater than or equal to zero in order for i to initiate a conflict with j .

Thus, by comparing equations (6) and (8), I can calculate i 's perception of the stream of events likely to follow i 's decision to initiate a dispute. Similarly, I can estimate j 's expectations regarding the unfolding of a dispute by comparing equations (7) and (9). To do so, I need merely to posit that i and j each assumes that the other is a rational, expected-utility maximizer or satisficer.

In order to specify the events that are likely to lead to the escalation of a dispute, I must first identify the conditions that define a dispute. A dispute or conflict occurs when i makes a demand of j which is accompanied by the threat to inflict punishment on j if the demand is not fulfilled. The demand may have many characteristics, of course: i may ask j to stop doing something that j was doing, for instance, or i may ask j to begin doing something that j previously did not do. The critical element is that the demand must be accompanied by a threat.

How can j respond to such a demand? Perhaps j can capitulate to the demand, or j can attempt to

negotiate with *i* in the hope of modifying *i*'s demand, or *j* can resist *i*'s demand, with force if necessary. Sometimes, when *j* attempts to negotiate and is unsuccessful, *j* will conclude that resistance (and fighting) is warranted. Indeed, one should expect that conflicts that escalate to include violence almost always pass through the negotiation stage, no matter how briefly. But when *j* capitulates to *i*, we should not expect further escalation except under situations where *i*'s perceptions are changed by the ease with which *j* gives in. Such situations, which may be characterized as describing the circumstance known as appeasement, should be relatively rare. By and large, then, one may perceive that *j* is expected to give in to *i*'s demands without resorting to negotiation or resistance if one believes that *j* perceives capitulation to those demands to be cheaper than resisting them.

One can represent all disputes as falling into one of four circumstances as perceived by one participant, so that 16 combinations of circumstances embody the complete perceptual mix possible among initiators and their foes. Figure 3 displays the four circumstances and provides the algebraic relationship each represents.

Escalation under Shared Perceptions

I will begin the discussion of Figure 3 by focusing first on the circumstances in which conflicts fall into the same portion of the Cartesian coordinates whether the dispute is viewed from *i*'s perspective or *j*'s. Suppose both sides perceive that the dispute falls in the first quadrant. Addressing the case from *i*'s point of view (just for ease of presentation), I can say that *i* believes *i* can extract a net benefit from challenging *j*. At the same time *i* believes that *j* expects to extract a net benefit from *i*. From *i*'s point of view this means that *i* does not expect *j* simply to give in to *i*'s demands, nor does *i* anticipate that a negotiated compromise is likely to be worked out. After all, how can adversaries compromise when both expect to be the recipient of net benefits? Because *j* shares the same perspective when *j* believes both sides expect to win, I can infer that war is highly likely under this "Fight" circumstance. I state this as the following hypothesis:

H₂: If *i* and *j* each believes their dispute falls in quadrant 1 of Figure 3, then the probability that their serious dispute will escalate to war will approach 1.0.

If the ensuing conflict is perceived by both parties to fall in section 2, in which one side is expected to win and the other to lose, but the loser is anticipated to lose less than the winner is demand-

ing, then neither party should expect simple capitulation to the demands being made. Instead, the side that is expected to lose should try to negotiate a compromise settlement. The negotiations will be over the difference in expectations reflected by the line segment *AB* in Figure 3. Some of the time these negotiations will succeed. At other times, however, one should expect that either *i* or *j* is unwilling to make a large enough concession, which leads to war. In general, that will happen when the anticipated costs of fighting and losing are smaller than the cost of the loss being demanded at the outset. I can state the general association as the next two hypotheses:

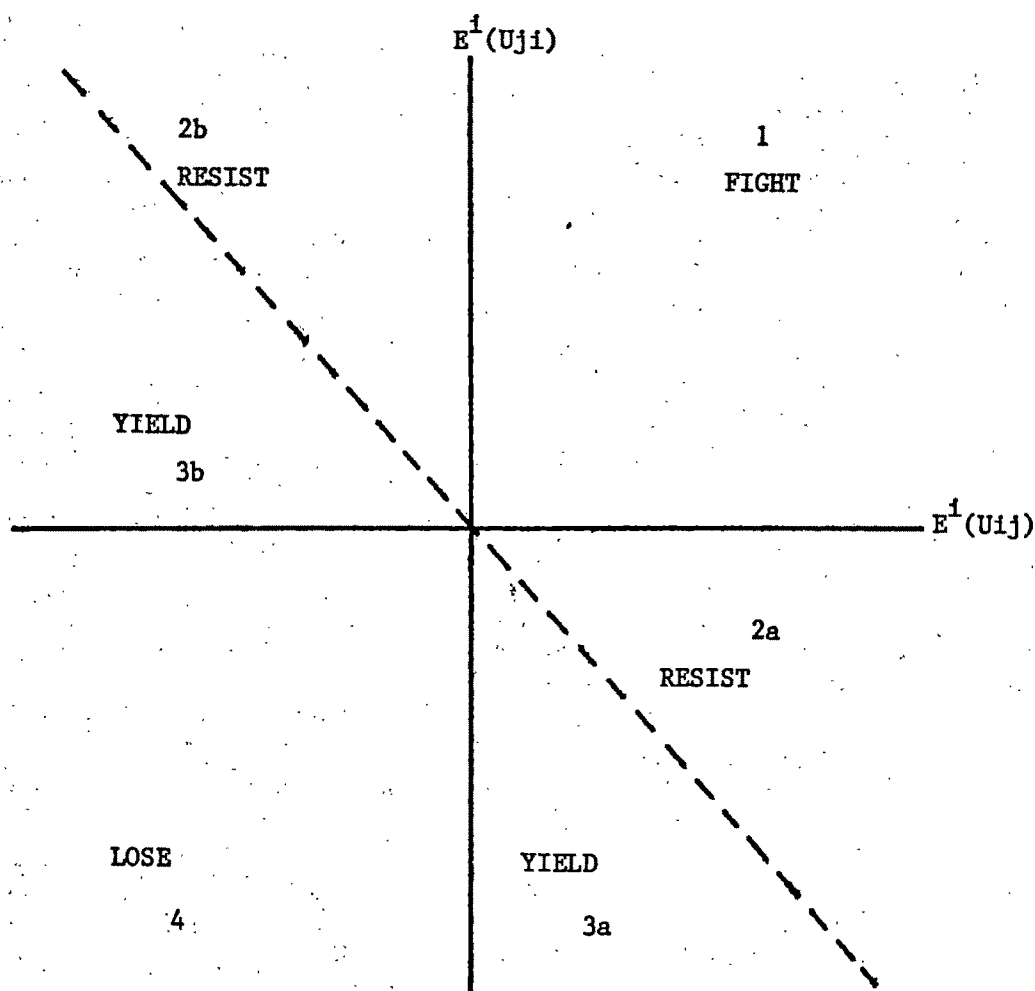
H₃: If both parties to a serious dispute agree on who the winner is anticipated to be, but disagree over how much the loser must give up to the winner, such that the loser believes less has to be sacrificed than the putative winner is demanding (i.e., the dispute falls in sector 2 of Figure 3), then some of the time a negotiated settlement will be reached, so that the conflict does not become as violent as a war, and other times the conflict will escalate to warfare. The probability of war under this Resist circumstance will be lower than under the Fight circumstance depicted in quadrant 1 of Figure 3.

When both initial disputants perceive that the expected utilities surrounding their conflict fall into sector 3, in which the potential victor is perceived to be capable of extracting a larger gain than that actor is perceived to be demanding, the expected loser is better off yielding to those demands than trying to negotiate or to fight for a better settlement. This is so because the loser anticipates that its adversary actually can gain more than is currently being sought. Any resistance to the opponent's modest demands could lead to an escalation in expectations. Certainly if the side making demands believes those demands are modest relative to what it is capable of extracting, it will have no incentive to accept even fewer gains in response to efforts at negotiations by the putative loser. Such conflicts should have a quite low probability of escalating to war, with the only likely exceptions being so-called appeasement circumstances, in which the loser yields so readily that the winner is encouraged to believe that additional benefits can be had cheaply enough to be worth pursuing.

H₄: If *i* and *j* agree on who is the expected winner of their dispute and if each side perceives the conflict falls in sector 3, called the "Yield" condition, then the probability of their dispute escalating to warfare (or violence) should be lower than the comparable probabilities under the "Resist" condition described in the previous hypothesis.

To continue with the circumstances in which *i*

Figure 3. Expected Utility:
The Effects of Perception on the Escalation of Conflicts



Fight: $E^1(U_{ij}) > 0$ and $E^1(U_{ji}) > 0$.

Resist_a: $E^1(U_{ij}) > 0$ and $E^1(U_{ji}) < 0$ and $|E^1(U_{ji})| < E^1(U_{ij})$.

Yield_a: $E^1(U_{ij}) > 0$ and $E^1(U_{ji}) < 0$ and $|E^1(U_{ji})| > E^1(U_{ij})$.

Lose: $E^1(U_{ij}) < 0$ and $E^1(U_{ji}) < 0$.

and j share a common perception about the nature of their dispute, I turn to situations in which the conflict falls in quadrant 4 of Figure 3. In this "Lose" circumstance, each side perceives that it cannot defeat the other side. Demands made under these circumstances are likely to be bluffs, posturing, or trial balloons. With luck one's adversary may give in to the bluffer's demands. Alternatively, one's adversary may respond with a bluff by assuming a tough, perhaps even threatening posture. Because neither side expects a net gain from concrete action, however, the conflict should not escalate beyond verbal threats. War should not take place.

H_5 : When both parties to a dispute perceive that they are facing a losing situation, the dispute should be resolved without warfare. The probability of war in such circumstances should approach zero, whereas the probability of a nonviolent resolution of the dispute should approach 1.

Escalation When Perceptions Differ

I now turn to disputes in which i 's perception of the relationship between i 's expectations and j 's expectations is different from j 's perception of that same relationship. Of course i 's and j 's perceptions may differ in two fundamental ways: i may anticipate that j 's incentives to negotiate or resist i 's demands are less than or greater than j 's perception of those same incentives. For instance, i may perceive that the i - j dispute falls into quadrant 1 (Fight) whereas j perceives that the dispute falls within the "Yield to i " sector (3a). The difference in such perceptions may have profound implications for the likelihood that a conflict will escalate to include violence or warfare.

When i makes a demand of j , i has some expectation about j 's response. If i believes j perceives its expected utility falls in quadrant 1, i expects a violent struggle. If i believes j 's expected utility falls into sector 3a which indicates that i is expected to defeat j , then i expects j to capitulate to its demands without negotiation or confrontation. But j , of course acts on j 's perception of the situation. Thus, if j perceives that the situation falls in sector 3 and that j is going to lose, then even though i might perceive j as falling in sector 2 (Resist), j will give in to i . Conversely, if, for instance, i thinks j falls into the Yield condition while j perceives that its dispute with i falls into the Resist circumstance, i will be surprised to discover that j , rather than giving in, will try to negotiate. Such a circumstance, in turn, is much more likely to lead to violence than if the circumstances were reversed, as in the first example. After all, i thinks it is making a modest demand of j —a demand for a settlement that is smaller than i thought j believed i could impose. If j tries to

negotiate under those circumstances (because j does not share i 's perception of the situation), i is likely to react with hostility toward j 's unreasonable reluctance to give i its modest request. One can state the circumstances covered by these and other situations that involve differences in perceptions as the following hypothesis:

H_6 : If i perceives that its conflict with j should not become violent because j is expected to give in to i , then if j does not give in, there is a higher probability of violence than if i perceives that j will resist or fight i when j perceives that it should give in to i 's demands; that is, when j 's perception of a dispute with i falls into a lower-numbered sector of Figure 3 than i 's perception of the same dispute, the probability of violence is higher than when j 's perception falls into a higher-numbered sector of Figure 3 than does i 's perception.

H_7 : As the dispute moves from the upper right quadrant of Figure 3 toward the lower left quadrant, the number of fatalities associated with the attendant conflicts should diminish steadily, approaching zero in quadrant 3.

Hypotheses two through seven are summarized by the relations depicted in Figure 4. None of these "escalation" hypotheses seems particularly surprising or counterintuitive. Indeed, once the logic of expected utility decision making has been laid out, these hypotheses seem almost obvious. Yet when we turn to their empirical investigation we should not forget that they are not ad hoc hunches, but a direct consequence of a systematic, comprehensive theory that provides hypotheses about conflict initiation, escalation, and termination, as well as about deterrence, detente, and alliance formation. Furthermore, we should bear in mind that they are obvious because of the expected utility framework. Without that framework we could not state these relationships. Indeed, to the best of my knowledge, they appear nowhere else in the literature on crisis management or conflict resolution. Yet many of these hypotheses are fundamental statements about the significance of perceptual differences which play so important a role in much of the conflict literature.

Indeed, several of these represent genuinely testable hypotheses about the relationship between conflict and perceptions.

Research Design

To test the revised model, I focus on 133 European disputes that took place between 1816 and 1965. These disputes include the 51 threats, 48 interventions, and 34 wars, identified in Appendix 2. Equations (6) through (9) were solved for all the

Figure 4. Graphic Depiction of Hypotheses 2-7

j's View of the Situation

	FIGHT	RESIST	YIELD	LOSE
<u>i's</u> <u>View of</u> <u>the</u> <u>Situation</u>	FIGHT	FIGHT	Lower Probability of War or Violence than in Portions of the Figure to the Left and Below	
	RESIST	RESIST		
	YIELD	Higher Probability of War or Violence than in Portions of the Figure to the Right and Above		
	LOSE			

Detailed description of Figure 4: The figure is a 4x4 matrix. The columns are labeled FIGHT, RESIST, YIELD, and LOSE at the top. The rows are labeled FIGHT, RESIST, YIELD, and LOSE on the left. The cells contain the following text:

- Row FIGHT, Column FIGHT: $P(\text{WAR}) \rightarrow 1$
- Row FIGHT, Column RESIST: $P(\text{WAR}) \rightarrow$ High
- Row FIGHT, Column YIELD: Lower Probability of War or Violence than in Portions of the Figure to the Left and Below
- Row FIGHT, Column LOSE: Lower Probability of War or Violence than in Portions of the Figure to the Left and Below
- Row RESIST, Column FIGHT: $P(\text{WAR}) \rightarrow$ High
- Row RESIST, Column RESIST: $P(\text{WAR}) \rightarrow$ Moderate
- Row RESIST, Column YIELD: Lower Probability of War or Violence than in Portions of the Figure to the Left and Below
- Row RESIST, Column LOSE: Lower Probability of War or Violence than in Portions of the Figure to the Left and Below
- Row YIELD, Column FIGHT: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row YIELD, Column RESIST: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row YIELD, Column YIELD: $P(\text{WAR}) \rightarrow$ Low
- Row YIELD, Column LOSE: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row LOSE, Column FIGHT: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row LOSE, Column RESIST: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row LOSE, Column YIELD: Higher Probability of War or Violence than in Portions of the Figure to the Right and Above
- Row LOSE, Column LOSE: $P(\text{WAR}) \rightarrow 0$

European dyads from 1816-1965, the period for which I have complete data.¹² For these analyses, I used the Correlates of War Project's *annual* composite capabilities data from 1816 to 1965. For the handful of conflicts within my data set for which annual data were incomplete I used the closest data point to the conflict. In practice, this meant that the scores for one dyad in 1914 are

based on 1913 data, two 1918 dyads are based on 1919 data, and three 1940 dyads are based on 1939 data. All measurement procedures are the same as those described in *The War Trap*, with the exception of the assessment of risk-taking orientations.

The risk measure, which is described in greater detail in Morrow (1982), is predicated on the notion that national leaders select their security policies with an eye to what they desire and also with an eye to what they think they can safely get away with. Thus, the risk terms are calculated by manipulating the alliance portfolios used as the policy indicator through simulation to locate the best and worst portfolios for any given nation, where the best and worst are defined in terms of the sum of expected utilities of all others vis-à-vis the nation in question under the assumption that

¹²I focus only on European dyads and European disputes here because of the costs of solving the revised model. Since Europe experienced more disputes for which I have data than any other geopolitical region, I begin with that part of the world. In subsequent analyses, as funding permits, I will extend the investigation to all of the geopolitical environments examined in *The War Trap* and to alternative constructions as well.

utilities are strictly a function of similarities in alliance commitments.¹³ That is, the combination of hypothetical alliance commitments for nation i (holding all other existing alliance commitments constant) that leads to the smallest possible sum of expected utilities for each j versus i is found, indicating i 's most secure position. Similarly, the worst hypothetical combination of alliance commitments is identified. These sums define the terms used to measure R_i in equation (1). I then calculate i 's propensity to take risks as a function of where in the range of possible security levels i 's actual alliance commitments place i . If i is exactly at the midpoint of the range, $R_i = 0$. As i moves toward a more risk-acceptant posture (i.e., away from more defensible policies toward less defensible policies), R_i increases, and as i moves closer to its most secure alliance portfolio, R_i decreases. The final risk score, calculated according to equation (1), is constrained so that R_i ranges between $+2$ and $-.5$. Some constraint is required, as noted earlier, to avoid division by zero. These risk scores are then introduced into the utility functions as described in equations (2) through (5).

Results

I begin my examination of the analysis by evaluating, albeit briefly, the results of the risk-taking calculations. It will be recalled that an important limitation of the initial measurement procedure for risk taking as described in *The War Trap* was that risk scores could not be estimated for major powers, compelling me to treat all such nations as if they were risk neutral. This was unfortunate, as the distribution of risk-taking propensities is a subject of considerable import for much of the research on war.

I have noted elsewhere that the theoretical relationship between systemic polarity and war (Bueno de Mesquita, 1978), or the theoretical association between power distributions and the likelihood of war (Bueno de Mesquita, 1981b), for instance, is dependent on what is assumed to be the distribution of risk taking among key national leaders. The contending polarity hypotheses arise because of different implicit assumptions about risk-taking propensities. Deutsch and Singer (1969), who contend that bipolarity tends to pro-

duce war whereas multipolarity tends to yield peace, seem inclined to believe decision makers are generally risk averse, whereas Waltz (1964), who supports the opposite hypothesis, appears to assume that such leaders are typically risk acceptant. I have argued that if risk taking is normally distributed, neither the hypothesis that bipolarity leads to war nor the counterhypothesis that multipolarity leads to war should be generally correct (1978).¹⁴ I have made similar arguments with respect to the contending hypotheses linking power distributions to war (1981b).

What, then, is the distribution of risk-taking propensities using the revised formulation? Risk scores were calculated for each European nation for each year of membership in the international system, for a total of 3,332 annual nation risk scores. The mean R_i value for these cases is .018, with a standard deviation of .503 and a range of values from -1.00 to $+1.00$. The median is .182. In other words, the average score is just about exactly at the risk neutral point. The measures of kurtosis and skewness for the distribution indicate only a moderate deviation from a normal distribution, with that deviation indicating slight skewness toward risk acceptance.¹⁵ The distribution for the major powers was essentially the same as for the lesser nations, with the range of obtained values being identical. Turning to the participants in the 133 pairwise serious disputes investigated here, I find that the overall mean risk score is .026, with the mean for initiators $-.033$, and for targets .085. *The means for all the conflicts tend toward risk neutrality, and the distribution is essentially normal.* Such a distribution casts serious doubts on many prominent hypotheses linking either polarity or power to war. But is there reason to believe that the risk scores estimated

¹³That is, temporarily applying the expected utility equations (without risk or uncertainty taken into account) as developed in *The War Trap*, I identify the worst case and best case alliance strategy for each nation each year, using the original, linear utility functions to define the range of possible expected gains or losses for each nation. These, then, are utilized to measure risk propensities and thereby to introduce curvature into the utility functions.

¹⁴In fact, one need not assume a normal distribution of risk-taking propensities to cast doubt on the polarity or power hypotheses. It only need be the case that risk orientations are widely dispersed, rather than heavily skewed toward risk acceptance or risk aversion.

¹⁵Interestingly, the mean risk-scores for the two centuries are significantly different from each other. The nineteenth-century mean of .064, indicating a slight tendency toward risk-acceptant behavior, stands in contrast to the twentieth century mean of $-.039$. The t -statistic associated with these means is 5.982. The t -statistic for the intercentury difference in risk scores for major powers is also significant ($t = 3.856$). In this case, the analysis indicates that although major powers tended to be slightly risk averse in the nineteenth century, their leaders became more risk averse in the twentieth century. These changes carry important implications which will be investigated in a subsequent study. I wish to thank Michael Horn for his invaluable assistance in calculating these distributional properties.

here are related to actual behavior? To answer this question, let us examine Figure 5. If the indicator of risk taking is meaningful, we should expect that those who were risk acceptant had a substantially higher probability of initiating unsuccessful violent conflicts—wars or interventions—than did those who were risk averse. As can be seen in Figure 5, 13 of 40 risk-acceptant initiators (or 33%) lost their violent conflicts, whereas only 2 of 40 risk-averse initiators (or 5%) suffered a similar fate. The mean risk score among defeated initiators of violent disputes is .286, whereas the mean among victorious initiators is -.139. The difference is significant at less than the .05 level. Apparently risk-acceptant initiators prove to have a much higher probability of defeat than do risk averse initiators, which suggests that they do, indeed, take greater risks.

The analysis, based on the model as presented in *The War Trap*, of the 133 cases included here shows that 107 of 133 initiators had positive expected utility, with only 31 opponents similarly possessing positive expected utility.¹⁶

¹⁶Because I have now shown that in conflicts expected to remain bilateral it is possible for P_i to be less than .5

Yule's Q , a measure of necessary, but not necessary and sufficient, conditions, equals .86 for this analysis. The original model, solved using annual rather than quinquennial capabilities data, shows that 106 initiators had positive expected utility, while 39 opponents were similarly endowed. Yule's Q for this analysis is .81, or essentially the same as with the quinquennial data. Making similar comparisons using i 's expected utility scores based on i 's perception of its own potential (equation (6)) and j 's expected utility scores based on j 's perceptions of its potential (equation (9)) demonstrates that 103 initiators had positive expected utility, 35 opponents were so endowed, and Yule's $Q = .81$. There is essentially no difference in goodness of fit when I compare the original model solved with quinquennial data to its solution based on the annual capabilities

and for i still to initiate a conflict under the rules of rationality, I no longer treat expected utilities of zero for the weaker side as being necessarily less than zero based on expectations regarding the utility associated with anticipated future policy positions. Instead, all zeros are now treated as positive numbers, in accordance with the expectations derived from the modified theory.

Figure 5. Relationship between Risk Taking, War Initiation, and Victory or Defeat

		i Won	i Lost
i is:	Risk-Acceptant	27	13
	Risk-Averse	38	2

data, nor does the switch from the original theoretical form to the new form seem to have mattered. In the original formulation, 107 out of 133 initiators had positive expected utility; 103 out of 133 are similarly endowed in the latest formulation. The difference is not statistically significant. The expected number of initiators with positive expected utility from among the 133 initiators, given the distribution of this attribute in the nearly 80,000 annual European dyads is substantially less than that which is observed. In fact, the difference is so large that the probability that 103 out of 133 initiators had this attribute by chance is less than .001, with $z = 6.281$.

These results seem to be quite encouraging. They demonstrate that the results are rather robust. They also show that the new version of the theory, which is both less ad hoc in its treatment of risk taking and is more parsimonious (having eliminated the need for four discrete decision rules), works as well as the more complicated original version when a simple hypothesis (H_1) linking the sign of expected utility to conflict initiation is examined.

Although these results provide some interesting information, they really are not able to show the key differences between the revised model and the original formulation. To see these, I must turn to the hypotheses that focus on conflict escalation. Here I can explore perceptual differences between i and j more thoroughly. In the original model it was not possible to distinguish between i 's expectations and j 's without making interpersonal comparisons of utility. With the new formulation, no such interpersonal comparisons are required. Because equations (6) and (8) show i 's expectations and equations (7) and (9) similarly indicate j 's, I can evaluate hypotheses (2)-(7) on conflict escalation and perceptions in a manner not previously possible. Let me turn, then, to an examination of the extent to which this added dimension contributes to our understanding of conflict escalation. The results are summarized in Figure 6.

Hypothesis 2 stipulated that the probability of war approaches being equal to one given that both sides perceive the situation as "Fight." Of the 12 disputes that satisfy this condition from both i 's and j 's perspective, 11 (92%) became wars. It is difficult to assess whether or not .92 is statistically different from 1.00, although it seems pretty clear that there is not a substantive difference. Using the normal approximation to the Bernoulli distribution and assuming that the expected proportion of wars equalled .98 demonstrates that the observed value of .92 is not significantly different from the expected value. It appears, then, that the probability of war under this circumstance does approach 1.00.

Hypothesis 3 indicates that the probability of war under the Resist condition should be significantly lower than under the Fight circumstance. Of the 29 disputes for which both i and j shared the perception that the conflict fell within the Resist category, 10 (34%) became wars. This is significantly less than 92%, with the attendant z -score being 4.168.

Hypothesis 4 claims that the likelihood of war under the Yield condition is still lower, and indeed it is. When both sides perceive their conflict to require a Yield (as they did 33 times), only six conflicts (18%) escalated to warfare. The attendant z -score comparing Yields to Resists is 2.147, which indicates a significant difference.

Hypothesis 5 postulates that under the Lose circumstance conflicts do not become wars, so that the probability of war should approach zero. Of the six cases that satisfy the mutual Lose conditions of this hypothesis, five were verbal threats and one involved an intervention in which there was a loss of between 101 and 250 lives. These results support the expectations found in the fifth hypothesis.

Hypothesis 6 focuses attention on situations in which the antagonists in a serious dispute perceive the situation differently. In particular, this hypothesis draws attention to the fact that the decision to escalate lies primarily with the target of a hostile demand. This is not because the target perceives more accurately than the initiator, but because the target acts on his perceptions just as the initiator acts on his perceptions. Thus, if the target believes that capitulation is appropriate, it really does not matter that the initiator was prepared for strong resistance from its opponent. It is completely within the control of the target to yield. Likewise, if the initiator perceives that its demands should prompt a quick surrender, that will not deter the target from fighting if the target perceives fighting to be appropriate. What do we find?

The association is in the predicted direction, but there is not a significantly higher probability of war when j perceives the situation to warrant resistance than when i does. When only i perceived that war was possible (i.e., i perceived the situation to be in the Fight or Resist categories), 0 out of 10 disputes escalated to warfare. When only j thought war was possible (i.e., only j perceived the situation to be Fight or Resist), 5 of 40 events became wars. However, there is a much higher probability that the conflict will escalate beyond a mere verbal threat to include violence. Less than one-fourth of the conflicts in which i 's perception of the prospect of violence was greater than j 's perception actually became violent, whereas 25 of 40 disputes (62.5%) escalated to include violence when j 's perception of the prospect

Figure 6. The Distribution of Wars and Interventions

<i>i</i> 's View of the Situation	<i>j</i> 's View of the Situation			
	Fight	Resist	Yield	Lose
Fight	11/12=.92 ^a 11/12=.92	2/3=.67 2/3=.67	0/10=0 1/10=.10	
Resist	No Cases of Conflict in this Cell	10/29=.34 22/29=.76		
Yield	5/40=.13 25/40=.63		6/33=.18 20/33=.61	0/6=0 1/6=.17
Lose				

^aThe upper set of numbers in each cell represents the number of wars divided by the total number of conflicts falling within the cell category. The lower set of numbers represents the number of violent conflicts (wars and interventions) divided by the total number of conflicts (wars, interventions, and threats) falling within the cell category.

of violence was greater than *i*'s. This difference is significant, with the attendant *z*-score being 5.912.

Let us now turn to the seventh hypothesis, in which the level of fatalities across the conflict categories is discussed. It will be recalled that I hypothesized that Fight situations generally escalate to a more severe level of violence than do disputes in the Resist category. I can add considerable discrimination beyond the threefold categorization of War, Intervention, and Threat to describe disputes. Gochman and Maoz (1982) have coded disputes in terms of the number of fatalities, using a six-fold scheme. Cases in their category 1 had no fatalities, whereas those in cate-

gory 2 had 1-25 fatalities. Category 3 involves disputes in which 26-100 fatalities occurred, whereas category 4 includes 101 to 250 deaths. Category 5 disputes experienced between 251 and 500 fatalities, whereas disputes in the sixth category had more than 501 deaths (and included all Singer and Small wars). Fatality category 6 was both the median and modal category for conflicts satisfying the Fight condition. Although this is a prominent category for disputes involving the Resist condition, the modal category is 1, indicating that many Resist disputes were resolved by peaceful means, with no fatalities. The distribution of fatalities across these two conditions is significantly different and in the predicted direction,

with the strength of that difference so large that it would have occurred by chance fewer than one time in one hundred. Interestingly, each adjacent dispute category—from Fight to Resist, from Resist to Yield, and from Yield to Lose—involves an equivalently significant decline in the level of associated fatalities. Indeed, the strength of the decline is so marked that if I compare the two extreme situations—Fight and Lose—with respect to fatalities, I find Kendall's tau equals $-.64$, with the associated probability that this association has occurred by chance being less than 1 in 10,000.

All seven hypotheses have been strongly supported by the evidence. We have found that the revised model is a powerful tool for predicting conflict escalation, with respect to both movement from nonviolent to violent conflict and to the level of fatalities associated with the use of violence. As we move down the quadrants in Figure 3 we have found that the probability of violence drops, and when violence occurs, its magnitude also drops. The results demonstrate that differences in perceptions have a profound impact on conflict escalation. When the target believes it has a credible prospect of extracting some advantage from fighting, violence is several times more likely than when only the initiator believes the target could derive such an advantage. What is more, the patterns of escalation show that nations behave in a manner fully compatible with the expected utility perspective, taking risks when leaders believe those risks are warranted, and avoiding them otherwise. Indeed, I should close this discussion by noting that when I chooses to begin a war or lesser conflict, his estimate of i 's and j 's expected utility is a rather good forecaster of the eventual outcome of the dispute. The winner possessed positive expected utility 81% of the time (with that for wars 88%). Given the distribution of positive and negative expected utility scores among the roughly 80,000 annual European dyads investigated here, or among the initiators and opponents that participated in the conflicts under study, I would expect so large a proportion of winners to have positive expected utility by chance far fewer than one time in 10,000. Yule's Q for the association between I 's expected utility estimates and the subsequent conflict outcome is $.81$, with a proportionate reduction in error of over 50%. For violent conflicts (wars and interventions), Q is $.89$, and the proportionate reduction in error is nearly 60%.¹⁷ Thus it is

obvious that i 's estimates (as calculated here) of the solutions to equations (6) and (8) are strong predictors of the actual resolution of the ensuing conflicts (with j 's estimates from equations (7) and (9) being equally good), with nearly all of the deviations from i 's apparent expectations being, as noted earlier, the consequence of i 's willingness to take risks.

Conclusions

Although a great deal of progress remains to be made in the development and testing of a theoretically sound explanation of conflict decision making, the results still seem encouraging. The expected utility framework has been modified to correct several deficiencies contained in its earlier formulation, which has allowed the deduction of additional propositions, and the evaluation of significant aspects of the relationship between perceptual differences across decision makers and subsequent conflict patterns. The new results do not negate any of the findings reported in *The War Trap*, but they do build from them and enhance them. The new results point strongly in a direction that indicates that one can predict with considerable confidence the likelihood of a conflict or threat becoming a war. Because many of the theory's terms are manipulable, these results suggest new directions in conflict resolution and conflict management. The results continue to support the contention that this approach may have yielded significant, lawlike generalizations about the initiation, escalation, and termination of international conflict.

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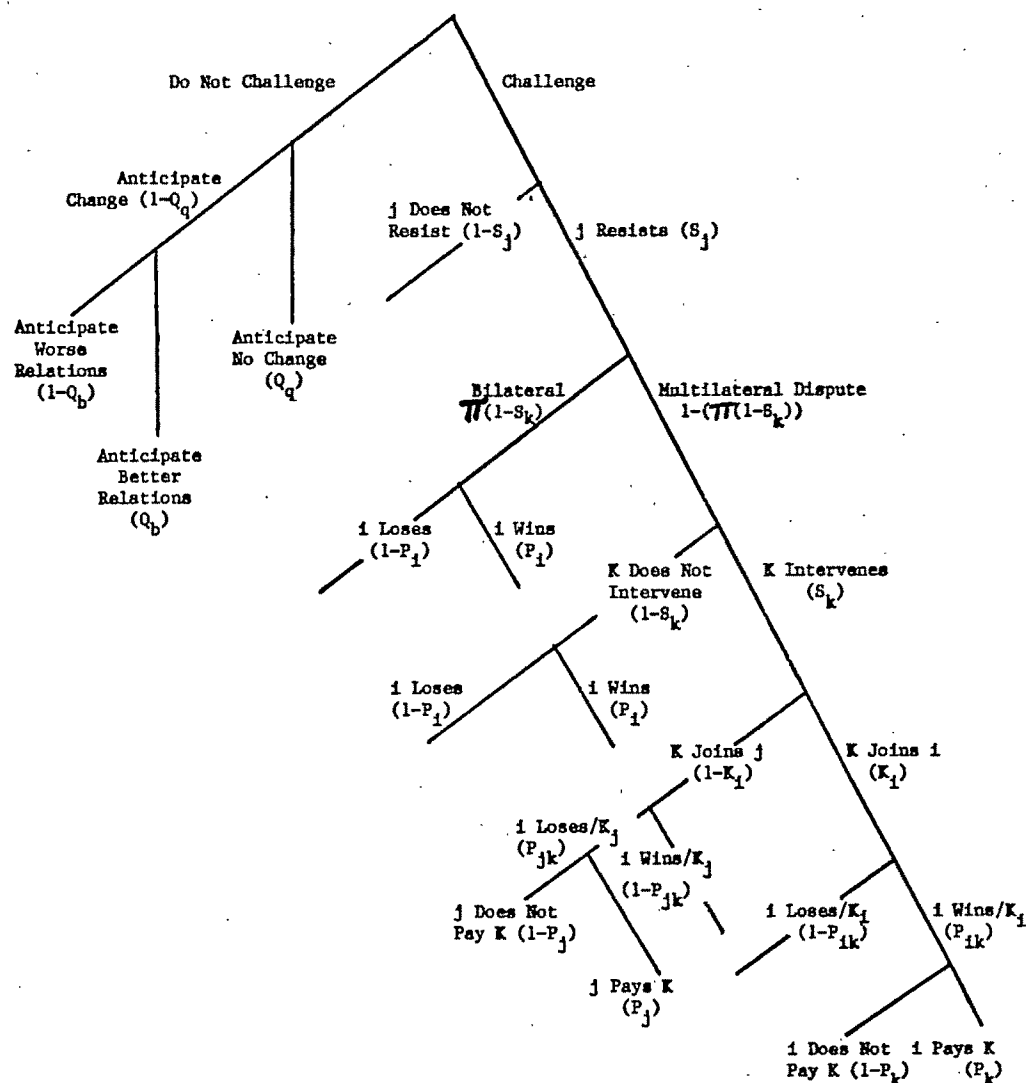
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¹⁷These values, of course, *underestimate* the goodness of fit between the data and the theory. It will be recalled from this discussion, as well as from *The War Trap*, that there are several circumstances under which the theory leads us to expect the initiator to lose. One such

circumstance discussed in this study involves risk acceptant, weak initiators of bilateral conflicts. Another, discussed in *The War Trap*, involves a situation of post-initiation deterrence (see also on this subject Huth & Russett, 1984). Still other such circumstances can be deduced from the expected utility framework.

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Appendix 1. The Decision Tree for a Further Revision



Appendix 2. Expected Utilities Estimated with the Revised Model

Year	Initiator	Opponent	$E^i(U_{ij})$	$E^i(U_{ji})$	$E^j(U_{ij})$	$E^j(U_{ji})$	R_i	R_j
Wars								
1823	France	Spain	.536	-.944	1.072	-1.223	-1.000	.463
1827	UK	Turkey	.240	-.413	.773	-.818	-.958	.398
1827	France	Turkey	.377	-.674	.713	-.919	-.956	.398
1827	Russia	Turkey	.203	-.455	.668	-.680	-.957	.398
1828	Russia	Turkey	.218	-.510	.738	-.797	-.959	.354
1848	Italy	Austria	-.624	1.469	-.981	1.683	.493	.112
1848	Germany	Denmark	.342	-1.454	.993	-.943	-.700	.510
1849	France	Papal	.857	-.664	.298	-.175	.269	.779
1853	Turkey	Russia	-.543	.858	-.627	.912	.371	.253
1859	Austria	Italy	1.350	0.000	1.512	-.320	.427	-.320

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^I(U_{ij})$	$E^I(U_{ji})$	$E^I(U_{ij})$	$E^I(U_{ji})$	R_i	R_j
1860	Italy	Papal	.424	-.206	.380	-.169	.430	.506
1860	Italy	Sicily	.274	-.057	.265	-.049	.430	.461
1864	Germany	Denmark	1.385	-1.352	1.272	-.588	-.186	.550
1866	Germany	Austria	0	0	0	0	.115	.260
1866	Germany	Baden	0	0	0	0	.115	-.919
1866	Germany	Bavaria	0	0	0	0	.115	-.786
1866	Germany	Saxony	0	0	0	0	.115	-.886
1866	Germany	HesElec	0	0	0	0	.115	-.951
1866	Germany	HesGran	0	0	0	0	.115	-.947
1866	Germany	Wrtmbrg	0	0	0	0	.115	-.908
1866	Germany	Hanover	0	0	0	0	.115	-.864
1870	France	Germany	.298	.016	.281	.029	.394	.494
1877	Russia	Turkey	.692	-.968	.793	-.502	-.194	.374
1897	Greece	Turkey	-.297	.523	-.454	.648	.457	.250
1911	Italy	Turkey	.614	-2.066	1.262	-1.571	-.640	.192
1912	Yugoslavia	Turkey	-.490	.836	-.753	1.041	.473	.200
1913	Bulgaria	Yugoslavia	0	0	0	0	.520	.546
1913	Bulgaria	Greece	0	0	0	0	.520	.530
1914	Austria	Yugoslavia	1.499	-2.153	1.587	-1.095	-.391	.546
1919	Rumania	Hungary	.357	-.143	.345	-.129	.370	.404
1919	Greece	Turkey	-.004	.216	-.003	.215	.354	.361
1939	Germany	Poland	.269	-.517	-.017	-.468	-.205	-.524
1939	Russia	Finland	.838	-.857	.830	-.827	-.041	.016
1956	Russia	Hungary	.009	.012	.066	-.048	.509	.087

Interventions

1828	France	Turkey	.328	-.244	.330	-.246	-.959	.354
1832	France	Holland	.689	-.472	.602	-.383	.367	.469
1832	UK	Holland	.791	-.584	.650	-.431	.313	.469
1847	Austria	Papal	1.721	-1.424	1.295	-.564	-.117	.645
1850	UK	Greece	.904	-.737	.590	-.400	.250	.562
1854	UK	Greece	.893	-1.424	.429	-.274	.279	.693
1854	France	Greece	.726	-.530	.415	-.256	.429	.693
1860	France	Italy	.565	-.352	.519	-.302	.359	.430
1886	UK	Greece	.956	-.900	.659	-.462	.099	.495
1886	Austria	Greece	.701	-1.874	1.240	-1.033	-.555	.495
1886	Germany	Greece	.890	-1.768	1.137	-.841	-.436	.495
1886	Russia	Greece	1.097	-1.743	1.129	-1.112	-.324	.495
1886	Italy	Greece	.747	-1.871	1.317	-1.149	-.592	.495
1897	Russia	Greece	1.182	-1.647	1.113	-.850	-.242	.457
1897	Austria	Greece	.755	-1.925	1.375	-1.237	-.607	.457
1897	Germany	Greece	1.216	-1.507	1.093	-.739	-.216	.457
1897	Italy	Greece	.931	-1.885	1.340	-1.102	-.480	.457
1897	France	Greece	1.185	-1.447	1.037	-.761	-.170	.457
1897	UK	Greece	.970	-.964	.702	-.504	.047	.457
1905	Austria	Turkey	.497	-1.770	1.112	-1.194	-.524	.226
1905	Russia	Turkey	.872	-1.548	1.184	-1.014	-.357	.226
1905	UK	Turkey	.826	-.891	.802	-.744	.089	.226
1905	France	Turkey	.688	-1.760	1.133	-1.185	-.403	.226
1905	Italy	Turkey	.503	-2.235	1.133	-1.392	-.633	.226
1918	Russia	Estonia	1.007	-.944	.464	-.294	.061	.667
1918	Russia	UK	-.260	-.009	-.300	.018	.061	.345
1920	UK	Turkey	.840	-.732	.755	-.581	.192	.320
1920	France	Turkey	.849	-.974	.756	-.608	-.024	.320
1920	Italy	Turkey	.783	-.817	.686	-.516	.011	.320
1921	France	Germany	-.206	.054	-.133	.111	-.101	-.007
1921	UK	Germany	.093	-.023	.058	-.064	.062	-.007
1921	Belgium	Germany	-.564	.433	-.492	.457	-.081	-.007
1923	France	Germany	-.209	-.026	-.113	.057	-.148	-.027

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^I(U_{ij})$	$E^I(U_{ji})$	$E^I(U_{ij})$	$E^I(U_{ji})$	R_i	R_j
1923	Belgium	Germany	-.591	.333	-.478	.394	-.148	-.027
1923	Italy	Greece	.759	-.703	.691	-.498	.054	.278
1938	Germany	Austria	.928	-1.732	1.269	-1.070	-.435	.322
1938	Germany	Czechoslovakia	.286	-1.081	.243	-1.071	-.435	-.460
1939	Germany	Czechoslovakia	.538	-1.043	.133	-1.149	-.205	-.458
1939	Germany	Lithuania	.782	-.985	.799	-.732	-.205	.133
1939	Italy	Albania	.808	-.834	.770	-.746	-.113	.128
1939	Russia	Poland	.397	-.439	.064	-.459	-.041	-.524
1939	Russia	Estonia	.636	-.656	.633	-.645	-.041	-.018
1939	Russia	Latvia	.633	-.668	.617	-.726	-.041	-.188
1939	Russia	Lithuania	.687	-.690	.648	-.614	-.041	.133
1940	Russia	Rumania	.774	-.853	.643	-.974	-.041	-.313
1948	Russia	USA	-1.881	1.460	-2.479	1.132	.417	-.378
1948	Russia	UK	-.104	-.554	-.139	-.442	.417	.028
1948	Russia	France	.399	-.720	.172	-1.002	.417	-.262
Threats								
1821	Russia	Turkey	.409	-.813	.974	-1.132	-1.000	.329
1826	Russia	Turkey	.426	-.206	.413	-.191	.384	.412
1840	France	UK	-.714	.307	-.454	.157	-.160	-.957
1840	France	Germany	-.433	-.535	-1.019	-.775	-.160	-.538
1850	Austria	Germany	-.053	.050	-.079	.075	.048	-.755
1853	Austria	Turkey	1.043	-.486	1.013	-.402	.235	.371
1854	Austria	Russia	-.267	.287	-.269	.288	.329	.321
1856	Germany	Switzerland	1.153	-1.318	.979	-.374	-.293	.679
1860	France	Italy	.565	-.352	.519	-.302	.359	.430
1876	Russia	Turkey	.709	-.991	.805	-.513	-.197	.377
1878	UK	Russia	.175	-.409	-.016	-.635	-.001	-.230
1880	UK	Turkey	.000	.000	.000	.000	.020	-.264
1880	France	Turkey	.147	-.113	-.054	-.595	.230	-.264
1880	Russia	Turkey	.013	.136	.003	-.542	.107	-.264
1880	Germany	Turkey	.303	-.630	.084	-.893	-.004	-.264
1880	Austria	Turkey	-.355	-.911	-.412	-.958	-.216	-.264
1880	Italy	Turkey	-.294	.431	-.591	.040	.268	-.264
1881	Turkey	France	-.439	.531	-.465	.544	.332	.302
1885	UK	Russia	.158	-.271	-.173	-.652	.096	-.320
1890	UK	Portugal	1.257	-1.653	1.138	.840	-.248	.470
1898	UK	Turkey	.658	-.892	.662	-.641	.095	.201
1898	France	Turkey	.677	-1.221	.831	-.847	-.195	.201
1898	Russia	Turkey	.915	-1.577	1.056	-.864	-.204	.201
1898	Italy	Turkey	.291	-1.527	.829	-.983	-.460	.201
1898	UK	France	.103	-.021	-.070	-.256	.095	-.195
1906	UK	Turkey	.745	-.790	.970	-.948	.128	.215
1908	Yugoslavia	Austria	-1.574	1.679	-2.242	1.235	.301	-.452
1911	Germany	France	-.346	-.551	-.795	-.691	-.149	-.638
1911	UK	Germany	-.468	-.252	-.392	-.176	-.207	-.149
1912	Russia	Bulgaria	1.001	-2.165	1.406	-1.111	-.526	.541
1912	Austria	Yugoslavia	1.514	-2.172	1.640	-1.253	-.376	.473
1913	Russia	Turkey	.685	-1.777	1.306	-1.247	-.514	.200
1913	Austria	Yugoslavia	1.499	-2.153	1.587	-1.095	-.391	.546
1921	France	Germany	-.206	.054	-.133	.111	-.101	-.007
1921	UK	Germany	.093	-.023	.058	-.064	.062	-.007
1921	Belgium	Germany	-.564	.433	-.492	.457	-.081	-.007
1921	Italy	Germany	-.278	.248	-.264	.256	-.026	-.007
1922	UK	Greece	.770	-.775	.680	-.586	.109	.299
1922	France	Greece	.865	-.141	.839	-.701	-.120	.299
1922	Italy	Greece	.720	-.694	.650	-.450	.024	.299
1934	Italy	Germany	-.686	.373	-.482	.226	-.361	-.754
1934	Italy	Albania	1.214	-1.430	1.287	-1.362	-.361	.033
1940	France	Russia	-.393	.177	-.355	.291	-.425	-.041
1940	UK	Russia	-.475	.419	-.691	.332	.181	-.041

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^I(U_{ij})$	$E^I(U_{ji})$	$E^I(U_{ij})$	$E^I(U_{ji})$	R_i	R_j
1956	Russia	UK	-.224	1.216	-1.628	-.297	.509	-.726
1956	Russia	France	.379	.345	-.949	-.727	.509	-.809
1956	Russia	Poland	.003	.022	.044	-.021	.509	.179
1957	Russia	Turkey	1.634	-1.401	.201	-3.118	.516	-.725
1957	USA	Russia	-28.864	-3.244	-26.601	-3.382	1.000	.516
1960	Russia	Turkey	1.666	-1.473	.177	-3.169	.480	-.716
1960	Russia	Norway	1.404	-1.648	-.035	-1.879	.480	-.830

The Transmission of Legal Precedent: A Study of State Supreme Courts

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In the course of making and justifying decisions, judges on state supreme courts often rely on precedents from other jurisdictions. These judicial references across boundaries constitute at least one means of communication and, in turn, demonstrate a complex web of deference and derogation between and among various courts. I attempt to uncover patterns of citation between the several state supreme courts and to evaluate alternative explanations for these patterns, including distance between courts; similarity of political culture; the prestige, professionalism, legal capital, and caseload of the cited court; the social diversity of the environment; differentials between courts on a number of dimensions; and presence in the same legal reporting region. More globally, I ask: Does the intensity of communications between a pair of courts result from the characteristics of the cited court or from differences and similarities between courts or jurisdictions? The results indicate the importance of legal reporting districts, distance between the courts, cultural linkages between the jurisdictions and, especially, characteristics of the cited court.

When they are obliged to present rationales for choices among alternative public policies, state supreme courts can and often do cite a plethora of legal, moral, and political authorities. Quite apart from statutes, regulations, constitutions, and their own decisions, these courts of last resort sometimes survey jurisdictions around the nation for relevant precedents.¹ Sir George Cornwall Lewis, who contended that a "large proportion of

the general opinions of mankind are derived merely from authority, and are entertained without any distinct understanding of the evidence on which they rest, or the argumentative grounds by which they are supported" (1875, p. 5), no doubt exaggerated, but he pinpointed a universal human tendency to lean on decisions taken in other times and places. Over a decade ago, Walker (1969, 1971) demonstrated rather clearly that legislatures take cues from several national and regional "leaders," and more recently Canon and Baum (1981) explored a similar phenomenon among appellate courts in the law of torts (see also Grupp & Richards, 1975; Kingdon, 1981; Matthews & Stimson, 1975). Decision makers on appellate courts, probably no less than other political actors and institutions, look for and use the experience of the legal reporters of the high courts of the nation (see Caldeira, 1983). That search stems from not only the very real need for information in the face of high levels of uncertainty (Cyert & March, 1963; March & Simon, 1958), but also the pervasiveness of precedent, regardless of source, as a norm of judicial choice making in the United States.

Judicial references across state boundaries, I contend, constitute at least one dimension of communication and demonstrate a complex web of deference and derogation between and among the various courts.² Yet despite the self-evident

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¹For students of the Supreme Court, Mr. Justice Frankfurter's polls of other jurisdictions, especially in cases involving federalism, remain legendary. Thus, for instance, in *Wolf v. Colorado* (338 U.S. 25 [1949]), Justice Frankfurter, for the Court, remarked that when "we find that in fact most of the English-speaking world does not regard as vital to such protection [the right of privacy] the exclusion of evidence thus obtained, we must hesitate to treat this remedy as an essential ingredient of the right. The contrariety of views of the States is particularly impressive in view of the careful consideration which they have given the problem in the light of the *Weeks* decision." He then proceeded to summarize the actions taken in the several states. See also *Betts v. Brady* (316 U.S. 455 [1942]) for Mr. Justice Roberts' poll of the states.

²I, of course, do not wish to imply that citation of precedent from other jurisdictions constitutes the only or perhaps the most important form of inter-court communication. Quite apart from the "exchange" of precedents, state supreme courts communicate through

importance and intrinsic interest of such phenomena, we have little or no precise conception of the forces that lead courts and judges to prefer the precedents of one jurisdiction over another (cf. Caldeira, 1983; Harris, 1980). In this article, which is based on data on inter-jurisdictional citational practices, I explore alternative explanations of the strength of linkages of communication between the several state supreme courts. More specifically, using all interstate references for all states and the District of Columbia for calendar year 1975, I estimate the relative efficacy of two global accounts—characteristics of cited courts (the *attribute model*) and traits of the relationships between judiciaries (the *relational model*)—for patterns of communication between state supreme courts.

Following the path of Walker (1969), Canon and Baum (1981), and others, this investigation should provide further illumination into the diffusion of policies and practices among political institutions. The bases of judicial communication uncovered here can, in addition, bring us to a clearer or at least fuller appreciation of political leadership among state supreme courts in particular and perhaps even of the more general phenomenon of relations between leaders and followers. In this latter sense, I see this article as a contribution to the burgeoning scholarship on the analysis of networks and social contexts (see, e.g., Knoke & Kuklinski, 1982). Lest anyone be mistaken, flows of political information, such as conversations among the citizenry and cue-receiving and cue-sending inside the legislature, can and often do have quite dramatic consequences for public policy (on mass behavior, see Eulau, 1980; on legislatures, see Kingdon, 1981; Matthews & Stimson, 1975).

forums such as the Conference of Chief Justices, New York University's Institute of Judicial Administration, and the various meetings of bar associations. Furthermore, I acknowledge at the outset that the citation of precedent from other courts can result from motives other than the earnest search for solutions of possible help used in other states. Thus, for example, justices and law clerks might simply string together references on the hope that sheer numbers will impress the consumers of their opinions. Nevertheless, I have encountered no evidence in the data presented here that justices of state supreme courts show a tendency to include the precedents of a particular court or courts more than others in so-called "string-citations"—a bias which, if it existed, would constitute a serious worry.

To lessen the repetition of "inter-state communication," I have taken the literary liberty of using a number of terms. The price, possible confusion about my purposes, pales in comparison to the potential tedium.

Studying Judicial Interactions

Several studies bear in various degrees on the research on interactions between courts I report in this article. Shapiro (1970) has argued that the evolution of the law of torts illustrates the phenomenon that 52 appellate courts which make decisions quite independently of one another arrive at a relatively unified set of doctrines. The principle of comity governs American law, but state supreme courts "constantly cite the decisions of others as persuasive or illustrative or worthy of consideration in making their own" (Shapiro, 1970, p. 51). These patterns in the development of the law of torts provide ammunition for Shapiro's contention that at least in some fields courts constitute very effective networks for the communication of information about the success and failure of policies (Shapiro, 1965, 1970, 1972). More recently, Canon and Baum (1981; see also Baum & Canon, 1982), in a study of innovations in the law of torts, have shown rather persuasively that a particular set of state supreme courts does not lead others in a consistent fashion across doctrines. Innovative judicial policymaking, at least in torts, embraces an appreciable amount of serendipity—not least of all because courts do not take the initiative. Building upon a line of reasoning that spans the intellectual traditions of law and economics, Landes and Posner (1976, pp. 250-251) "treat the body of legal precedents created by judicial decisions in prior periods as a capital stock that yields a flow of information services which depreciates over time as new conditions arise that were not foreseen by the framers of the existing precedents." Most important for my purposes, Landes and Posner articulate the concept of "legal capital"; as we shall see below, that theoretical framework can in a more limited way figure fruitfully in a multivariate model of judicial communication.

Even more central to the present concerns, Harris (1979, p. 1; see also 1980) has argued that "occasions on which [state courts of last resort] put each others' decisions . . . to use are instances of communication between the courts and about the common law." Naturally, Harris asks: "What are the characteristics of the highest appellate courts, and of their jurisdictions, the states, and what are the relations between the states, that have historically led to this communication." On the basis of data from a sample of 16 states collected at decadal intervals from 1870-1970 (see Friedman, Kagan, Cartwright, & Wheeler, 1981; Kagan, Cartwright, Friedman, & Wheeler, 1977, 1978; Mason, 1978), Harris (1979, p. 29) reports that "in all periods, state supreme courts tend to cite the courts of the states from which their own state received its population." In sum, patterns of

migration, capturing the flow of cultural values, account in large part for inter-court communication. Quite apart from traces of migration, state supreme courts' stock of "legal capital" (i.e., the quantity of its reports) figured significantly, although less prominently; physical distance between judiciaries made no difference; presence in the same legal reporting region manifested a moderate coefficient; and, finally, courts located in jurisdictions possessed of laggardly legislatures tended to follow judges in more innovative environments. But even in the context of an impressive multivariate account, the highest appellate courts in New York, California, and Massachusetts garner measurably more references than do others; indeed, in one specification, dummies for these jurisdictions produce coefficients of magnitude second only to migratory patterns.¹

More recently but much in the same vein, I have initiated investigations into the transmission of precedent between state supreme courts. These explorations have as a by-product yielded an article on the comparative prestige of the highest appellate courts of the several states (Caldeira, 1983; compare Mott, 1936; see also Merryman, 1954, 1977). In that article I argued that references between courts constitute handy and nonreactive indicators of hierarchies of prestige among the highest judiciaries, and I developed a measure of judicial reputation and examined competing explanations of differentials in prestige. "Social diversity, apart from case load, [ranked] first among the firmament of influences on judicial reputation, and professionalism [trailed] as a distant second" (p. 104). Thus state supreme courts nested in more professional structures and located in socially diverse jurisdictions received, on average, a greater measure of deference. These influences, in the aggregate, account for differences in the sheer amount of references a particular

state supreme court receives, but that does not inform about the relationships between institutions.

That nexus between high benches is the focus of this article.

Issues of Measurement

Through the medium of legal reports, judges on appellate courts leave extraordinarily well-documented clues about processes of decision making. In recent years scholars of public law, now focusing on judicial behavior, have once again begun to follow that set of trails (e.g., Johnson, 1980; Kagan et al., 1977, 1978; see also Cartwright, 1975; Llewellyn, 1960, p. 514). The unit of analysis here—the state supreme court's reference to one of its sister courts—for the purposes of this article seems straightforward enough and did not pose substantial problems of coding and classification. (For a discussion of the vagaries of legal reports as evidence, see Cartwright, 1975, p. 381). Most of the time, a reference across jurisdictions jumped out from the page. (See Nagel, 1962, for evidence that appellate courts almost always cite other jurisdictions in a favorable or neutral fashion.)

The issue, then, is how to operationalize the intensity of the communication between judicial institutions. To encapsulate judicial interaction, I began with a simple count of the number of references each state supreme court made to every other state court of last resort in 1975. Because in many ways it functions as a state, I included the Court of Appeals for the District of Columbia. From that count, I created a large sociometric matrix (51 by 51), which runs the gamut of 2601 relationships. The first row shows the number of times that Alabama's Supreme Court cited those of Alaska, Arizona, Arkansas, etc.; the second row gives Alaska's references to Alabama, Arizona, Arkansas, etc.; and in the last row, Wyoming's mentioned decisions from Alabama, Alaska, Arizona, etc. Some courts make extensive use of precedents from other jurisdictions; others, for a variety of reasons, choose to rely on their own stock of legal rules. Left unstandardized, the matrix is in some circumstances at the mercy of the heaviest users of precedent; such states might well have a disproportionate and misleading influence on the final results.

To standardize for heavy usage, I counted the number of citations each of the courts made in 1975. That in turn facilitated computation of the percentage of this total that any particular supreme court received. If state supreme courts sought legal authority on a random basis, each high bench should receive about 2% of the references from the others. In the summary, each state supreme court should, randomly, have a

¹Although Harris's study contains much to recommend it, he bases his generalizations on an exceedingly small sample (16 states)—chosen for good reasons, to be sure, but nonetheless not representative of the full panoply of American states. More serious for a study of relations between governmental units, Harris has observations for only a limited segment of the entire network of such phenomena—a matrix of 16 by 48 instead of the full complement of 50 by 50. Unfortunately, because he must work with a partial matrix, Harris can speak of fewer than a thousand relations rather than a full range of possibilities. Because his colleagues on Yale's project on state supreme courts made a conscious and necessary decision to collect and code only a sample for each of the 16 states, Harris confronted a matrix much less dense in connections than he might have. Consequently, I suspect, he has missed some of the weaker relationships.

total of 100%—2% from each of the remaining 50 high courts. But courts do not use precedent from out of state in a random pattern. These percentages then replace the row numbers in the 51×51 sociometric matrix. In the larger analysis, each interaction in the matrix becomes a dependent variable $N = 2601$). Because I have not counted a court's references to itself, the first case remains missing; the Alabama Supreme Court's citation of Alaska constitutes the second case, of Arizona the third, and of Wyoming the fifty-first. That pattern continues until Wyoming's Supreme Court interacts with the Wisconsin Supreme Court. (The final case, of course, is missing.)

Procedures for matching judicial interactions and explanatory phenomena follow more or less the same pattern. If an explanation consists of an argument that a characteristic of the state supreme court increases or decreases the references it receives (the *attribute model*), then I attached the value to all cases in which it served as the target. Thus, if the Alabama Supreme Court scored 12 on legal professionalism, I placed this number on every 52d case, beginning with the first. That constitutes the simplest set of influences. For another class of explanations, though, I contend that differences or similarities in the characteristics of each pair of state supreme courts depress or facilitate interactions (the *relational model*). For instance, state supreme courts in less diverse jurisdictions might refer to high benches in the more heterogeneous jurisdictions. Or perhaps communication occurs between judges in similar environments. To generate such relational variables, I have performed simple subtraction on the relevant characteristic between the citing and the cited state supreme courts. These influences can, of course, take on both negative and positive values. If negative, the relational variable indicates that the citing court ranks lower; if positive, the cited court has less of the quality.

Explanations of Judicial Communication

There are numerous and often subtle reasons for interactions between appellate courts, and so it should not surprise that a complete explanatory specification involves a raft of influences. Social scientists in previous work have identified a number of bases of communication, such as proximity, similarity, and prestige. At the most basic level these explanations fall into two categories. I shall not dwell on this matter, because the distinction I make is so well-grounded in the literature of social psychology. First, does the intensity of communication between a pair of courts result from characteristics of the cited state supreme court? This I call the *attribute model*. Second, do

differences and similarities between courts or jurisdictions increase or decrease the rate of interaction between appellate judges? I label this notion the *relational model*. From one perspective, it makes good sense to argue that the traits of a state supreme court or the host jurisdiction increase or decrease the attractiveness of its stock of legal precedents. After all, scholars of sociometry have often demonstrated the substantial influence of wealth, physical appearance, and status on the choice of friends and leaders. And yet one can make a good case that myriad social, political, and economic connections structure the communication of precedent between state supreme courts. The importance of the workplace, neighborhood, and political party as determinants of electoral choice comes immediately to mind as an example.

Sifting through the multitude of explanations and variables poses some awkward problems of presentation; most of the explanations have variants that come under both headings. Thus, for instance, I test the effects of the prestige of the cited court and of the differential in prestige on patterns of communication.

Legal Reporting Region

Since the 1880s, the West Publishing Company of St. Paul, Minnesota, has collected and reported the opinions of the several state supreme courts in seven separate editions, divided more or less into geographic regions. Until quite recently, each of the state supreme courts published its own opinions in an individual report, but, especially in the 1960s, more and more of the states shifted to reliance on a regional edition. These reporting regions should make a difference in patterns of citation for at least three reasons: physical access, the scarce resources of time and energy, and psychological distance. The latter two require little elaboration. Lacking the time and energy to scan all of the state and regional reports, lawyers and judges without a good reason for doing otherwise should peruse the regional edition that incorporates the decisions of their jurisdiction. Beyond that consideration, lawyers and judges are likely to perceive the reports of other regions as less salient, not relevant for use in making decisions.

In a bygone era even appellate judges had access to a relatively small proportion of judicial decisions. Not all or even most members of appellate courts then lived and worked in the state capitol (Adamany, 1969). Located in the countryside, some of these judges may have placed reliance on personal, municipal, and county law libraries, most of which had quite limited collections. Nowadays most of the state supreme courts have substantial law libraries, including books for

the chambers of individual judges. Many practicing lawyers have little access to the best and most extensive repositories of judicial opinions, and since appellate briefs constitute one of the means through which precedents come to the attention of state supreme courts (Marvell, 1978), this limitation may well filter back into the reports. To be sure, most lawyers who actually handle appeals before state supreme courts practice in large firms in metropolitan areas and have easy access to a wide variety of reports. Changes in the nature of appellate litigation and improvements in communication (e.g., Lexis) have no doubt blunted the effects of physical access, although I suspect that it may still play a role in the more isolated regions of the United States.

These considerations lead to the expectation that a state supreme court should on average choose the precedent of courts inside the same reporting region more often than it does others. I have in mind here more than the conventional sense of regionalism (e.g., north versus south); in a surprising number of instances, West has not followed traditional usage in allocating a state to one or another region. Thus, for instance, the *Southwestern Reporter* includes opinions from Arizona, Kentucky, Missouri, Tennessee, and Texas—not at all a familiar combination. Should the legal reporting region manifest a significant impact on judicial communication, one can conclude with considerable confidence that, even apart from the standard meaning of region, presence or absence in the same report makes a difference. To evaluate this proposition, I have used dummy variables, scored 0 for absent and 1 for present, for the seven West reporters—Atlantic (*AT*), Northwestern (*NW*), Northeastern (*NE*), Pacific (*P*), Southeastern (*SE*), Southern (*S*), and Southwestern (*SW*). The relevant equation, which I have estimated with ordinary least squares, produces:

$$\begin{aligned}
 JC = & 1.88 + .98 AT + 1.99 NE + .90 SE \\
 & (38.06) \quad (3.95) \quad (2.98) \quad (1.74) \\
 & + 1.78 S + .56 P + 1.95 NW \\
 & (2.67) \quad (3.14) \quad (5.43) \\
 R^2 = & .026 \quad N = 2550
 \end{aligned}$$

where *JC*, the dependent variable, represents judicial communication; *t*-ratios appear under the appropriate unstandardized regression coefficients; R^2 , the coefficient of multiple determination, monitors the amount of variance explained; and *N* equals the number of cases. To avoid statistical problems, I have excluded the southwestern region, and so the intercept yields the coefficient for that variable.

Not at all surprisingly, legal reporting region alone does not account for a substantial amount of variation (a bit more than 2.6%) in the transmission of precedent between appellate courts. Still, each of the regions musters a significant coefficient, all of which at least meet the criterion of .01 level. State supreme courts, regardless of region, manifest a consistent pattern of citing inside rather than outside their own report. The magnitude of the effect varies considerably across regions—from modest, among the Pacific states ($b = .56$), through substantial, for the members of the northwestern ($b = 1.99$) and northwestern regions ($b = 1.95$). Contrary to what one might reasonably expect, supreme courts in the south do not manifest as great a degree of parochialism as do others, at least in this preliminary cut.

Geographical Proximity

For geographers, physical distance between organizations, individuals, and jurisdictions has long functioned as a chief determinant of patterns of human behavior, but political scientists have for the most part ignored this often crucial facet of politics. The simple accident of adjacency sometimes explains a substantial amount about social life. Political communication, for instance, has a greater chance of happening between neighbors or members of the same workplace than it does between two individuals living in different localities. The probability of interaction increases not only because of physical proximity but also because individuals who dwell next to each other normally share a wide range of traits, such as social class, economic wherewithal, and attachment to the neighborhood.

If judicial behavior conforms to these widely observed social laws, it is equally plausible that, *ceteris paribus*, a state supreme court refers more often to the precedents of its neighbors. Numerous influences undergird and reinforce the impact of geographical proximity. Two of these spring immediately to mind: first, each state has very distinctive qualities about it, but neighboring jurisdictions most often share at least some aspects of social, political, and cultural values. For example, although in many ways they are different, Minnesota and Iowa have relatively similar political cultures; New York and New Jersey suffer from many of the same ethnic and religious cleavages; and North Dakota and South Dakota hold in common a pattern of economic activity.

Second, as the physical and I presume psychological distances between two state supreme courts increase, it becomes less likely that justices will regard one another's precedents as apt. This proposition can assume at least two forms: one can argue for a linear and inverse, or, alternatively,

for a curvilinear and negative relationship between distance and communication. The first is straightforward: one state supreme court's propensity to cite another declines proportionately the greater the distance between them. In the second form there is a recognition that, beyond a particular threshold, distance has less and less impact on communication; in sum, that remoteness decreases communication up to a point and then has little or no effect. For appellate judges in an eastern state, for instance, the decisions of the supreme courts of Colorado and Hawaii perhaps do not seem all that different.

To capture the effect of distance (D) on judicial communication, I use number of miles between each pair of state capitols; the square of D is labelled DSQ . The results of a simple regression indicate that distance does indeed have an inverse impact on communication between courts [$JC = 2.51 (31.98) - .003 (7.87) D$; $r^2 = .024$; note that the t -ratio appears in parentheses to the right of the appropriate unstandardized coefficient], but the relationship manifests distinctively nonlinear qualities [$JC = 3.02 (25.86) - .001 (8.24) D + .17-07 (5.89) DSQ$; $R^2 = .037$].

Legal Capital

Legal capital consists of "both the abstract, more or less fact-independent rules, and the existing group of matchable fact situations" (Harris, 1979, p. 2; see Landes & Posner, 1976; Posner, 1973, 1977; Rubin, 1977). It represents the collective and collected wisdom, amassed over decades, of an appellate bench. Broad experience, reflected in a stock of legal capital, no doubt makes the decisions of a state supreme court attractive sources of information (on legislatures, see Walker, 1969). Sheer quantity of experience does not, of course, translate directly into influence; breadth, versatility, and competence make a difference. Yet Shapiro (1972) has underscored the importance of "redundancy" in networks of communication. "Legal discourse," he states, "in the style of *stare decisis*, then, is not a unique phenomenon, but an instance of communication with extremely high levels of redundancy" (1972, p. 129; see also Landes, 1969). The larger the stock of legal precedent state supreme court possesses, the more often it can figure in the opinions of sister courts, regardless of how voluminous the store of cases in the reports of these benches.⁴ Or, alternatively, perhaps a court

possessed of a huge store of precedents does not serve as a favorite for the entire range of supreme courts, but, rather, for benches lacking a substantial amount of doctrine.

To measure legal capital, I have used the number of running feet of legal reports each state supreme court has produced from its inception until 1970 (Merryman, 1977, pp. 401-404; cf. Harris, 1979, p. 5)—although in a more perfect world I should have preferred to have the number of decisions made or of pages written. At the simplest level, it turns out that a sizable legal capital does make a court more central in the network of communication [$JC = 1.25 (18.06) + .017 (14.96) LC$; $r^2 = .082$]; and that supreme courts relatively low on precedent refer more often to courts with more legal stock [$JC = 2.04 (43.99) - .008 (10.35) LCDIFF$; $r^2 = .042$].

Size of Caseload

Especially in the last decade and a half, scholars, lawyers, and judges have increased public and professional consciousness of the existence of a crisis in the volume of and demand for legal processes (see Caldeira, 1982; Casper & Posner, 1976). Quite apart from the issue of justice for individuals, the crisis of volume often leaves pronounced marks on the nature of the business of state supreme courts (Kagan et al., 1978, p. 971). For instance, "on average, courts with larger caseloads wrote more opinions which were shorter, which used few citations, and which referred less often to treatises, legal encyclopedias, and law reviews" (Kagan et al., 1978, p. 972). Signs suggest that overload increases the "risk of routine, poorly crafted opinions," not at all a surprising consequence. Opinions dashed off with abandon and based on trivial disputes do not make prime candidates for notice among state supreme courts. Flooded dockets in courts of last resort go hand in hand with the lack of an intermediate appellate bench and discretionary powers of review; if a state supreme court has a set of judges interposed between it and the lower reaches of the judiciary and has the authority to reject cases, it can usually hold its workload to manageable limits. The presence of an intermediate appellate bench, then, increases judicial leisure and discretion. In consequence, the nature of the cases changes, judges become more fractious and opinionated, and courts probably evince greater willingness to take bold in-

⁴Merryman remarks: "Assuming some sort of even, if random, distribution of different fact situations and legal questions among the reported decisions, the probability that one will find a case in point in the decisions of

a given state should be a function of the number of its published decisions. . . . Thus, the more published decisions, the more citations" (1977, p. 403).

initatives in policymaking (see Atkins & Glick, 1976; Canon & Jaros, 1970; Jaros & Canon, 1972).

Regardless of the causes, on the basis of these considerations, heavy caseloads should decrease the attractiveness of a state supreme court's decisions. For example, the Rhode Island Supreme Court, which issues hordes of opinions almost all utterly lacking originality (Beiser, 1973), receives the notice of very few of its sister courts (Caldeira, n.d.). There are no doubt upper limits on the capacity of excessive caseloads to decrease reliance on a court's precedents; after all, even the hardest-pressed benches figure in at least some of the decisions of state supreme courts around the nation. Preliminary statistical analyses provide some slight evidence that the size of its caseload (taken from Marvell & Kuykendall, 1980) decreases the probability that a state supreme court's precedents will achieve widespread attention [$JC = 2.41 (23.19) - .009 (4.74) C; R^2 = .009$] and a shred more that it does so at a diminishing rate of effectiveness [$JC = 2.97 (14.01) - .034 (4.05) C + .0002 (.303) C^2; R^2 = .01$].

Judicial Professionalism

Structural characteristics of courts vary a great deal across American state jurisdictions—from the simple, streamlined, and modern through the complex, variegated, and traditional. Professionalism, some have argued, implies a modernized structure and an absence of partisan politics in the selection and retention of judges. Legal reformers have for many years, and with substantial success, attempted to gain public support for an ideal of state judiciaries as separate, hermetically sealed off from the nefarious influences of parties, governmental officials, and the siren song of public opinion. This conception in fact usually translates into the adoption of the American Bar Association's models for judicial selection and organization, creation of an office of professional administrator of the courts, more or less permanent tenure for judges, and, finally, salaries munificent enough to attract and retain jurists of high stature (Glick & Vines, 1972, p. 11).

For the most part, more professionalized judiciaries can claim greater prestige than less professionalized state supreme courts (Caldeira, 1983, p. 99). This should be expected, since it is the lawyers and judges who have set out the desiderata of professional judiciaries. These professional courts, commanding high respect and having marked out a trail of independence, may well find it easier to recruit judges of remarkable abilities. Insulated in the main from considerations of partisan manipulation, such jurists may "produce decisions that are more widely ap-

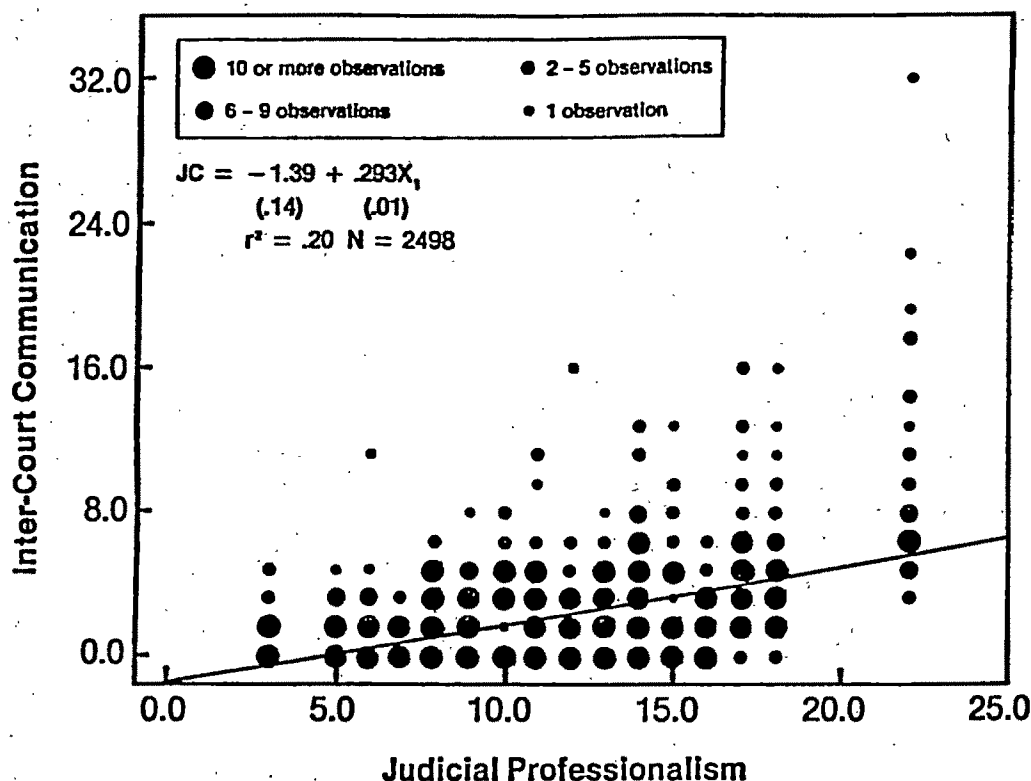
plicable over states and time, because they can afford to let abstract rules rather than political concerns decide cases" (Harris, 1979, p. 10). In sum, if members of a state supreme court need not fret about the reaction of this or that faction or cabal to particular facets of its decisions, then these judges have the freedom to make more principled choices. Such precedents presumably have a better chance of serving large numbers of jurisdictions.

Law professors, who played a large part in creation of the American Bar Association's criteria, no doubt accord the professional courts quite favorable treatment in scholarly circles such as law reviews, casebooks, encyclopedias, and perhaps most important of all, the classrooms of schools of law around the nation. It turns out that the relationship between professionalism and prestige is quite strong (Caldeira, 1983, p. 99). Some fifty years ago, Mott (1936, p. 305) reported a very high correlation ($r = .89$) between a court's prestige, monitored through other bench's references to it, and the number of its opinions reprinted in a sample of casebooks. He observed, "It is clear that those courts which are drawn on most frequently for opinions in the casebooks are more likely to influence the legal thinking of the next generation of lawyers than those which are ignored" (p. 305). Moreover, as I remarked earlier, an intermediate appellate court, which is an important component of professionalism, allows a state supreme court the freedom necessary for repose, reflection, discussion, and writing (see Atkins & Glick, 1976; Canon & Jaros, 1970; Jaros & Canon, 1972; Kagan et al., 1978, p. 972).

These considerations compel the proposition that state supreme courts more often invoke the decisions of high courts high on the scale of professionalism. It is equally plausible, however, that differences in professionalism between courts color patterns of communication. Thus, a bench low on professionalism might refer often to the set of courts highest on that attribute, but courts located in more or less similar ranges of professionalism—high or low—might not choose each others' precedents. To measure professionalism, I have chosen a straightforward and often-used indicator (Glick & Vines, 1972; for applications, see Canon & Baum, 1981; Glick, 1981) that has a potential range of 0 through 25 and an actual of 3.4 through 21.7.

Figure 1, which portrays the simple relationship between professionalism and judicial communication, supplies substantial support for the proposition at issue: state supreme courts ranking high on professionalism achieve more attention. Still, as befits a relationship at such a level of aggregation, there is a great deal of dispersion around the line

Figure 1. The Effect of Judicial Professionalism on Communication Between Courts
(Figures in parentheses are standard errors.)



Sources: The data on professionalism are from Glick and Vines (1973, p. 12). The data on communication were collected by the author.

of regression. Finally, differences between states in professionalism do matter: courts low on the totem pole, on average, more often choose precedents of the higher-ups [$JC = 2.04$ (45.25) - .141 (16.00) $LPDIFF$; $r^2 = .095$], although this relationship is not as strong as that for the initial formulation.

Social Complexity

High levels of social diversity and development create an environment particularly hospitable to the use of law and to doctrinal innovation. "In societies in which the level of economic development is relatively low and in which social relationships are close and interdependent, customary devices of social control and mechanisms of conflict resolution seem to predominate" (Grossman & Sarat, 1975, p. 323; see also Schwartz & Miller, 1963). These ties that bind, once broken, give way

to an explosion of complex disputes, new varieties of rights claimed, and, of course, room to spare for creative common-law judging. Social change instigates problems that soon make the application of tried-and-true legal rules anomalous, so judges especially on state courts of appeal must reshape old doctrines and invent new ones. Large populations translate into more cases at law (for a theory of litigation, see Casper & Posner, 1976; cf. McIntosh, 1983) and, in turn, into more and more "problematic" issues (see also Dahl & Tufte, 1973). Conversely, "sparse population reduces opportunities for judicial innovation" (Canon & Baum, 1981, p. 980). Industrialization and urbanization, which almost always occur at the same juncture, increase the use of law and litigation (Friedman, 1973, p. 17). There is some evidence that litigation, if not the use of law per se, tapers off as industrialization, population, and urbanization reach a threshold; but a preponder-

ance of the evidence supports the view that these forces constitute leading indicators during an initial period.

Regardless of the precise character of the relationship, we know that if and when a new issue comes to the fore, it most often arises in the most populous, diverse, industrialized localities. State supreme courts at work in more diverse milieus, processing more cases of first impression, have the opportunity to contrive precedents that, if not always adopted intact, at the very least merit the rapt attention of jurists who follow in the wake. Or, in an alternative formulation of the relationship, supreme courts in the least modern should invoke the previous decisions of the benches in the more modern states; in essence, the parochials should follow the cosmopolitans. To operationalize societal complexity, I have chosen the size of a state's population—not an ideal measure.³ The

³There are a number of paths I could have taken in testing the notion that social complexity influences interstate communication. First, as I mentioned in the text, I could have divided the general idea into a series of more specific propositions about the effects of urbanization, industrialization, size of population, and growth on patterns of citation. Lamentably, because of the rather substantial correlations among these variables, statistical problems immediately arise. Thus, for example, depending upon how I defined urbanization, the correlation between it and population varied between .5 and .7. If I included indicators of industrialization, population, change in population, and urbanization in the same equation, multicollinearity caused some of the signs to run in a nonsensical direction. It is, for instance, implausible that urbanization decreases the attractiveness of a state supreme court's precedent, but that is exactly what I encountered in equations with industrialization, population, and urbanization. Furthermore, if in some formulations the signs for urbanization or industrialization did not go awry, the coefficients always approached zero. The size of a state's population washed out the effects of other variables, and this result held regardless of how I operationalized urbanization or industrialization. Moreover, the inclusion of variables other than size of population neither improves the statistical fit nor changes the substantive results.

Second, I could have constructed a composite of a number of important indicators of societal complexity, a strategy that many students of comparative politics have adopted. In the context of American subnational politics, some scholars have used techniques of data reduction such as factor analysis to transform larger numbers of variables into a smaller set of dimensions. Sharkansky and Hofferbert (1969), for example, reduced a passel of indicators into factors labelled "affluence" and "industrialization." These composites, though tidy, naturally lack conceptual clarity and an intuitively discernible metric. The items that constitute "industrialization," for instance, are a disparate lot, ranging from the value of industrial property to the size of population. A single title, "industrialization," im-

plies a good deal more coherence than indeed exists. If a significant relationship between judicial communication and a composite of "industrialization" or "affluence" had materialized, I am not at all certain how I could or should have interpreted it.

correlations among indicators of urbanization, size of population, and industrialization run so large that one cannot distinguish among them. That fact is a double-edged sword; it means, on one hand, that I cannot estimate the separate effects of the three forces on inter-state communication, but on the other hand, because size of population dominates, I can treat it as a proxy for the constellation of forces parading under the rubric of modernization. In any event, statistical analysis yields solid evidence of the impact of societal complexity on patterns of communication; population alone explains 30% of the variation in rates of reference [$JC = .829 (15.38) + .296-07 (32.47) P; r^2 = .293$]. Furthermore, the differential in population for a pair of states has a pronounced effect on the interaction between the two supreme courts [$JC = 2.01 (46.94) - .15-07 (20.81) PDIFF; r^2 = .145$], although, as in foregoing explanations, this more sophisticated formulation stands at least initially as less compelling than the simpler one.

Prestige

The relative prestige of a particular supreme court might incline the judges of other courts to consult and to invoke its precedents more often and to lend them more weight than these judges might otherwise do (see Mott, 1936).⁴ Evaluations regarding prestige "are functionally autonomous from the hierarchies of wealth and power—that is, . . . subjective responses to particular social positions that are rooted in generally shared" values (Laumann & Heinz, 1977, p. 161). Thus a supreme court might, because of the influence of prestige, receive more than the share of references predicted simply on the basis of other considerations. For instance, in 1975 state courts of last resort used the California Supreme Court's

plies a good deal more coherence than indeed exists. If a significant relationship between judicial communication and a composite of "industrialization" or "affluence" had materialized, I am not at all certain how I could or should have interpreted it.

In light of these issues, I chose the size of population as an indicator of social complexity. It does not, I admit, perfectly reflect all aspects of social complexity. But the size of population surely constitutes one of the most crucial characteristics of this phenomenon. Having opted for one indicator with a clearing meaning and metric, I know what I have and do not have.

Goldthorpe and Hope define prestige as a "particular form of social power and advantage that is of a symbolic rather than an economic or political character, and which gives rise to structured relationships of deference, acceptance, and derogation" (1972, p. 19, 21).

precedents far more often than the size of its population, the extensiveness of its legal capital, or its rank on judicial professionalism indicated (Caldeira, 1983).

In this article, Harris specifies dummies for the high courts of New York, California, and Massachusetts, because these benches "have historically had a great deal of prestige" and in the analyses become "statistical outliers" (1979, p. 18). Indeed he reports particularly large coefficients for the highest benches of Massachusetts and New York in the period between 1870 and 1900 and for California after World War II (Harris, 1979, p. 27a). So far I have reviewed the argument for and evidence of the simple, linear effect of prestige on intercourt communications; that line of reasoning implies that all state supreme courts choose the precedents of the more prestigious over the less prestigious. It is equally possible that the least prestigious state supreme courts might invoke the common law of the most, and the highest ranking might opt for their own stock of decisions. This in effect means that the differences between courts and not the characteristics of the cited high bench affect rates of interaction. If such a pattern occurs, we should observe a strong association between communication and the differential in prestige between courts.

To test this pair of propositions, I use a variation on a summary indicator of the total references each state supreme court drew from others as a measure of prestige. To guard against spurious associations, I have purged these two indicators of any artifactual correlations between prestige and judicial communication.⁷ This route

is preferable because it allows us to use a high level of measurement and just one variable. These relationships, at least at the simplest level, make a very strong showing: the most prestigious supreme courts obtained more references than did the least [$JC = 2.01 (51.64) + 1.27 (32.45) PREST$; $r^2 = .29$], quite an impressive fit for data of this sort, and benches lowest in the hierarchy manifested a marked tendency to opt for the precedent of the highest [$JC = 2.007 (46.98) - .628 (20.96) PRESTDIF$; $r^2 = .147$].

Cultural Linkage

Social scientists have demonstrated over and over again the effects of the social, political, and economic similarities among states (Savage, 1978; Sharkansky, 1970) and of the regional loyalties and affiliations (Gray, 1973; Walker, 1969) on the making and contours of public policy. These shared values or "political culture," it is thought, predispose states to reach decisions of more or less the same character. Too often, of course, political culture has functioned as an explanation of last resort, an account of any and all inexplicable residual patterns; but, although abused, this line of reasoning is appealing, for it squares with daily observation. No one, for example, doubts the distinctive nature of southern politics and culture. Shared cultural values—political, social, and economic—stem from patterns and flows of migration and settlement in the United States. As populations of the various states have moved across the nation and found new homes, these individuals have carried with them and transplanted into new soil the attitudes, beliefs, and practices of a former life. The leading exponent of this line of thinking (Elazar, 1966, 1970) contends that

⁷Conceptually, as I have argued in the text, I believe that we can distinguish between prestige and inter-court communication—just as we separate wealth, political power, and prestige. In fact, the idea is quite simple: a state supreme court is more likely to cite another court if others also refer to it often. Empirically, I would of course have preferred to use an attitudinal indicator of justices' perceptions of the prestige of state supreme courts, but that seemed out of the question. Although the measures of inter-court communication and of prestige derive from the same original source—a count of citations in legal reporters—the subsequent transformations performed make them empirically distinguishable indicators.

To operationalize prestige, I begin with the scores from a count of the total of references that any particular supreme court has received from all others. This ranges from 461 for California to 0 for the District of Columbia. Then I attach this score for a court to each case in which that court is on the receiving end of a reference. At this point, for each case, I subtract the dependent variable from the score on prestige in order to purge the latter of any artifactual association with judicial communication. For any particular pair of state

supreme courts (e.g., Alabama cites Colorado), this number is the score on prestige for the receiving court minus the score for the share of the first court's references devoted to the second court. In other words, in computing prestige here, I have excluded references between pairs of state supreme courts involved in each case. To create a differential measure of prestige, I have followed the procedure outlined in the third section.

These transformations apparently succeed, for the statistical analyses manifest no telltale signs of problems. The coefficient of determination, for instance, although substantial, is not overwhelming ($r^2 = .29$). Last but not least and to anticipate the final results, as a precautionary matter I have estimated the multivariate regressions presented in Tables 1-3 without prestige and found that its exclusion had only a modest influence on the size and stability of other coefficients. In these alternative regressions, the only appreciable difference was that the fit was a bit worse and the effect of caseload became more pronounced.

patterns of migration and especially the settling of the frontier had a major hand in shaping the outlines of a state's political culture; succeeding waves in the later nineteenth and early twentieth centuries reinforced and sometimes substantially modified the initial distribution of values (see, more generally, Gastil, 1975a). To the extent that a state furnishes the citizens for another, it has an influence on the culture of that jurisdiction.

If patterns of migration have a profound effect more generally on politics, it is plausible to expect that at least a minimal cultural linkage should increase communication among state supreme courts. If citizens of one state move to a second and if this reasoning is correct, the state supreme court of the second should on average choose from among the stock of legal precedent of the first's highest court more often than from others.⁸ Cultural linkage might translate into higher levels of intercourt communication, first through lawyers, law professors, and litigants who bring with them a preference for the precedents of their former homes, and second, through judicial favoritism toward the law of a particular state seen as somehow similar or as an appropriate leading light in the region.

In sum, if a state contains an appreciable number of citizens from another jurisdiction, so the argument goes, its supreme court should choose the precedents of that state's high bench more often than it does others. To measure cultural linkage, I have used data on migration—more specifically, Gastil's estimates of the proportion of a particular state's population drawn from the several states (1975b). In the results of a simple regression, there is a substantial amount of support for the idea that patterns of migration influence the character and extent of communication among state supreme courts [$JC = 1.63 (35.76) + .709 (22.80) M; r^2 = .17$].

Findings

Thus far, I have presented some preliminary evidence for and against several explanations for patterns of interstate communication of precedent, and, for all of them, at least some support has materialized. The issue that remains, then, is: Does each of the potential determinants exercise

an independent and significant influence on intercourt communication? In outlining these explanations, I have at various junctures spoken of two global categories—relationships between states, and characteristics of the cited court. Before evaluating the fully specified equation, I present statistical results first of all for the relational model, consisting of differentials in various attributes, proximity, legal reporting region, and the distribution of migration, and second, for the attribute model, which comprises particular characteristics of cited courts. The results so far indicate more support for the idea that the traits of state supreme courts and their contexts facilitate the communication of precedent more than the relationships between pairs of benches. I shall show in a moment how that notion fares. Table 1 embodies estimates from a regression of patterns of communication on relational influences. Recall that, aside from distance, legal reporting region, and cultural linkage, each independent variable is the result of subtracting the cited supreme court's value from the citing court's.

First of all, taken as a whole, this equation yields a decent first step toward an explanation of intercourt communication; the fit is adequate, if not impressive; 12 of the 13 coefficients reach .15 level of significance and most do much better, and all but one of the signs run in the expected direction. These results indicate that, in general, differentials between state supreme courts do indeed influence patterns of communication. If a state supreme court has relatively little prestige and ranks low in professionalism and social diversity, on average, such a high bench more often invokes the precedents of its sister courts higher up the ladder. Furthermore, cultural linkage manifested a substantial impact on judicial interactions; each 4% increase in the proportion of a state's residents who have migrated from another jurisdiction increments by 2% the share of the state supreme court's precedents drawn from the former home of its new inhabitants. Distance between state supreme courts performs exactly as expected. Remoteness decreases communication until a threshold is reached and then exercises less and less effect, but before that threshold is reached, each thousand miles of territory between a pair of courts depresses the proportion of precedents invoked by about 0.33%.

Legal capital, on the other hand, has an effect quite the opposite of that which I had suggested. Supreme courts rich in legal capital apparently opt for the decisions of high benches less well-stocked—instead of, as I argued, the other way around. The California Supreme Court's dominance of intercourt communications (Caldeira, n.d.) no doubt does much to reverse the predicted relationship, since the Golden State joined the

⁸In Nevada, for example, much of the population has come from next door in California. The Nevada Supreme Court, in choosing among the decisions of state appellate courts, has invoked the precedents of California's Supreme Court in about 32% of the instances. No doubt the extraordinary dependency of Nevada on California marks an upper limit, but this stark example sensitizes us to the potential effects of migration on interstate communication.

Union at a relatively late date. Finally, the coefficients for West's regions support in general the proposition that the manner in which courts disseminate their opinions has an impact on communication, although the results here are not so unequivocal as in the bivariate setting.

Standardized regression coefficients, which appear in the first column of Table 1, afford a comparative assessment of the importance of the alternative explanations. (By way of a caveat, I should state that interpretations of the standardized coefficients from quadratic formulations because of multicollinearity most often prove hazardous. For this reason, I eschew substantive remarks about the relative influence of distance.) Cultural linkage ranks first and differential in prestige second among the influences on intercourt communication, and societal complexity trails in third place. Furthermore, legal capital and judicial professionalism, although not nearly so potent as cultural linkage and prestige, nevertheless manifest a measurable impact. Of the several state supreme courts, judiciaries in the Northwest, Northeast, and Atlantic regions of West's scheme demonstrate a rather pronounced bias in favor of precedent within their own reports. Still, alongside the results for prestige and cultural linkage, the standardized coefficients for legal regions pale in comparison.

There is, however, equally and in some ways more persuasive evidence in support of the contention that the characteristics of the cited court structure the communication of precedent—as Table 2 suggests. Once again, we encounter significant coefficients in abundance, signs for the

most part in the correct direction, and an even better fit. These coefficients imply that prestige, professionalism, and societal complexity increase the chances that a supreme court's precedent will accumulate references. Here again the errant sign for legal capital appears, and, again, as an explanation, I submit the influence of California, a court relatively poor in legal capital. If a supreme court processed a large caseload, *ceteris paribus*, its sister courts invoked its decisions more often than if it had a smaller one, but of course after a certain juncture size made less and less difference. Of the independent variables, societal complexity and prestige exert the greatest influence, and professionalism and legal capital significantly less. In this pass at the data, the large and prestigious states collect the lion's share of the references. Table 3 presents the estimates for the fully embellished equation and allows us to speak in more authoritative terms. A more complete assessment yields some coefficients at odds with the results reported in Tables 1 and 2. These appear because the full range of controls unmasks several apparent but ultimately spurious relationships.

Perhaps most striking, four of the most plausible relational characteristics fail to reach or approach statistical significance in this fuller consideration; differentials between states and courts on the dimensions of prestige, professionalism, societal complexity, and legal capital do not influence the communication of precedent. The fit of this equation is very impressive in light of the inevitable noise present in such data. In the choice of precedent, then, the characteristics of the cited

Table 1. Regression of Intercourt Communication on Relationships between States and Courts

	β	<i>b</i>	<i>t</i> -ratio	Significance
Intercept		2.021	16.31	.001
Prestige	-.232	-.386	-9.10	.001
Legal professionalism	-.110	-.051	-5.39	.001
Size of population	-.179	-7.192-08	-7.06	.001
Legal capital	.138	.006	6.16	.001
Cultural linkage	.324	.571	17.32	.001
Distance	-.132	-.0003	-2.57	.005
Distance squared	.079	3.88-08	1.47	.150
Legal reporting districts				
Atlantic	.046	.557	2.28	.010
Northeast	.047	1.401	2.49	.005
Southeast	-.009	-.222	-.50	n.s.
South	.026	.783	1.39	.150
Pacific	.024	.214	1.42	.150
Northwest	.068	1.174	3.82	.001

$R^2 = .324$
 $N = 2450$
 $D.F. = 2436$

Table 2. Regression of Intercourt Communication on Characteristics of Cited Courts

	β	<i>b</i>	<i>t</i> -Ratio	Significance
Intercept		-.337	-1.11	n.s.
Prestige	.277	.653	10.16	.001
Legal professionalism	.154	.101	7.47	.001
Size of population	.363	2.099-07	14.07	.001
Legal capital	-.119	-.007	-5.80	.001
Caseload	.260	.026	3.26	.001
Caseload squared	-.241	-2.11-04	-3.03	.001
R^2	.38			
<i>N</i>	2450			
<i>D.F.</i>	2443			

court dominate. Thus a supreme court's prestige, professionalism, societal diversity, and, to a lesser extent, the size of caseload can and do incline the highest courts of the several states to invoke its decisions more often. Legal reporting edition, too, intervenes in the transmission of precedent. Supreme courts within the same region, aside from the northeasterners, manifest a significant propensity to choose each others' decisions. That phenomenon comes out most clearly for the Northwest and the Pacific and least for Northeast and the Southeast.

Although differentials for the greater part wash out, two—distance and patterns of migration—remain central in a full-blown account of judicial communication. The logic is simple: the greater the distance between two supreme courts, the lower the likelihood of communication, and the more substantial the cultural penetration of one state by another, the more likely the recipient state court is to cite the precedents of the original name state's court. On the basis of the standardized coefficients, prestige and societal complexity rank first in consequences; cultural linkage and legal capital fall in a second stratum. Legal professionalism exerts just a little less influence. It is unfortunate that for statistical reasons I cannot make much of the coefficients for caseload and distance, for on both intuitive and theoretical grounds this pair of explanations holds promise. That much said, logarithmic rather than quadratic versions produce quite an impressive standardized coefficient for distance and a miniscule one for the size of caseload. For the natural log of distance, $\beta = -.209$; for a similar formulation of caseload, $\beta = .05$. Finally, although none of standardized coefficients for legal reporting regions is in and of itself very large, these dummies taken together translate into a substantial influence on the way appellate judges choose precedents. Decisions made at the West Publishing Company years ago have had marked consequences for judicial communication.

This full model explains about half of the variance in patterns of communication, but much remains. Residuals often shed light on the weaknesses of an explanatory scheme, so for each of the cited supreme courts I have computed a mean citational score (i.e., the average share of references received) and mean residual (i.e., the average difference between predicted and actual for the sum of the interactions). These results (not shown) exhibit no systematic patterns of error—that is, the state supreme courts for which the model produces the largest errors share no obvious characteristics or relationships. The California Supreme Court so dominates the communication of precedent that even after a full multivariate treatment, it leads the pack. More interesting, some unexpected high benches generate large positive residuals: the model substantially underestimated the shares of the supreme courts of Alabama, Kentucky, Mississippi, Missouri, South Dakota, and Wyoming. These supreme courts do better in gathering citations than predicted on the basis of such factors as prestige, societal complexity, migration, and distance. Equally surprising, the supreme courts of Connecticut, Michigan, Ohio, Oklahoma, and Texas did much less well than the average high bench. It is not, of course, that these courts receive few references, but rather that they score so high on most of the independent variables and in consequence build up unfulfilled expectations.

Summary and Conclusion

Characterized as they are by so many nuances, patterns of reference between state supreme courts defy simple description or explanation, but in this article I have attempted to uncover these traces of intercourt communication, and, more important, to evaluate alternative explanations of such forms of communication. These rival accounts include distance between courts, similarity in the political culture, the prestige, legal struc-

Table 3. The Full Equation for Intercourt Communication

	β	b	t -Ratio	Significance
Intercept		-.091	-.27	n.s.
Characteristics of cited court				
Prestige	.255	.601	7.14	.001
Legal professionalism	.192	.126	6.04	.001
Size of population	.363	2.04-07	10.32	.001
Legal capital	-.153	-.009	-5.77	.001
Caseload	.220	.022	2.97	.005
Caseload squared	-.184	-1.64-04	-2.54	.005
Relational characteristics				
Prestige	-.026	-.043	-.79	n.s.
Legal professionalism	-.024	-.011	-.87	n.s.
Size of population	.003	1.175-08	.91	n.s.
Legal capital	.005	.0002	.13	n.s.
Cultural linkage	.213	.375	12.64	.001
Distance	-.307	-.0007	-5.82	.001
Distance squared	.198	1.039-07	4.43	.001
Legal reporting districts				
Atlantic	.055	.678	3.04	.001
Northeast	.011	.342	.70	n.s.
Southeast	.024	.573	1.49	.150
South	.049	1.493	3.01	.005
Pacific	.084	.745	5.25	.001
Northwest	.078	1.348	4.66	.001
$R^2 = .490$				
$N = 2401$				
$D.F. = 2382$				

ture, legal capital, and caseload of a court, the societal complexity of a state, and presence in the same legal reporting district. Based on analyses of all citations between state supreme courts in 1975, the results indicate the importance of legal reporting districts, distance between the courts, cultural linkages between the states, and most fundamentally the characteristics of the cited court (e.g., prestige). Statistical analyses of single elements of the full model proved misleading; if I had halted after examination of the results on the relational explanations, I would have arrived at very different—and wrong—conclusions.

More globally, although there is substantial support for both conceptions, the attribute model on the whole outperformed the relational model of judicial communication. Social connections—ties that bind state supreme courts—do indeed matter a great deal, but it is a court's characteristics that contribute most to the attractiveness of its legal precedents. The success of the attribute model in this context places this form of communication between courts in the same company as many other formal and informal institutions or groups.

The results I have reported here bring into vivid

relief the broad similarities between the transmission of legal precedent among state supreme courts and the flow of information and cue-taking within and between legislatures. Legislators, we know, lean on like-minded and similarly situated fellow specialists. Innovative policies apparently diffuse in quite different fashions among legislatures and courts (Canon & Baum, 1981), but the research I have reported here, insofar as it contributes to the comparative analysis of institutions, establishes the tendency of state supreme courts to choose the precedent of "leaders" on a variety of dimensions. These judges, like their colleagues in legislative halls, evince a marked propensity to rely on the lead of the more professional, prestigious courts located in the more diverse and populous states. Legal precedent flows from the more diverse, prestigious, and professional to the state supreme courts that rank lower as well as high on these dimensions.

To the extent that these leaders consistently adopt policies of a certain ideological stripe, the flow of precedent introduces a potentially important bias in the network of communication. Unfortunately, there is no reliable, quantitative measure of the ideological content of state

supreme court's decisions, but it takes no more than a cursory examination to bring one to the preliminary conclusion that the courts cited most often in most jurisdictions have a decidedly liberal cast. If closer analysis supports this speculation about the political character of the state supreme court's decisions, then there is good reason to believe that policies and precedents of a particular kind will more often receive the attention of appellate judges. In addition, it is most likely that the sorts of precedent that state supreme courts will choose will, for example, favor criminal defendants, tenants over landlords, debtors over creditors, and plaintiffs against defendants in tort. Of course from another perspective it makes perfect sense that the decisions of the more liberal state supreme courts receive greater notice. These courts, after all, demonstrate a greater willingness to move away from the status quo. Precedents in which a court announces a change naturally cause more comment and controversy than does an affirmation of past practice. Still, whether natural or not, such a bias in the content of the flow of precedents does exist.

So far I have emphasized the vertical bias of communication between courts, but I suppose that the mere existence and the pervasiveness of such flows of information between state supreme courts testify to the health and vigor of our federal system. National political institutions may dominate much of the time in most fields of endeavor, but in the development of the common law, state supreme courts dance to their own drummer. To be sure, because of the biases in the network, the tune is likely to have national sound about it. In spite of the bias in favor of the precedents of more prestigious, professional courts in diverse states, there are a number of vestiges of a persistent regionalism. Legal reporting regions, cultural affinities, and distance constrain the flow of precedent. Here as elsewhere (see Walker, 1969), such forces stand as barriers to the complete homogenization and nationalization of policies across jurisdictions. The interesting issue of whether the influence of regional forces in the diffusion of precedent has declined as media of communication have improved—a plausible hypothesis—lies outside the purview of this study.

Quite apart from the light shed on the transmission of legal precedent, the results I report here have implications too numerous to mention for the study of political communication within and between organizations and the relationship between information and decision making. To note just two, though, the search for information within state supreme courts takes place vertically more often than horizontally and is to a surprising degree quite limited—which makes the courts,

especially in this latter regard, very much the same as most organizations and individuals.

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Forthcoming Articles

The following articles have been scheduled for publication in the June, 1985 issue.

Richard Born, "Partisan Intentions and Election Day Realities in the Congressional Redistricting Process"

Bruce E. Cain, "Assessing the Partisan Effects of Redistricting"

Richard E. Dagger, "Rights, Boundaries, and the Bonds of Community"

Ellen Jones and Fred W. Grupp, "Modernization and Traditionality in a Multiethnic Society: The Soviet Case"

Douglas Madsen, "A Biochemical Property Relating to Power Seeking in Humans"

Eileen L. McDonagh and H. Douglas Price, "Woman Suffrage in the Progressive Era: Patterns of Opposition and Support in Referenda Voting, 1910-1980"

Arthur H. Miller and Martin P. Wattenberg, "Throwing the Rascals Out: Policy and Performance Evaluations of Presidential Candidates, 1952-1980"

Charles W. Ostrom, Jr., and Dennis M. Simon, "Promise and Performance: A Dynamic Model of Presidential Popularity"

Robert D. Plotnick and Richard P. Winters,

"A Politico-Economic Theory of Income Redistribution"

Keith T. Poole and R. Steven Daniels, "Ideology, Party, and Voting in the U.S. Congress, 1959-1980"

Karen A. Rasler and William R. Thompson, "War Making and State Making: Governmental Expenditures, Tax Revenues, and Global Wars"

Herbert A. Simon, "Human Nature in Politics: The Dialogue of Psychology with Political Science"

John R. Wright, "PACs, Contributions, and Roll Calls: An Organizational Perspective"

Errata

In "Equality, Efficiency, and Politics in Soviet Bilingual Education Policy, 1934-1980" by Barbara Anderson and Brian D. Silver (Vol. 78, p. 1031), the graphs for Figures 1 and 2 were inadvertently transposed. The lower graph should be Figure 1; the upper graph belongs with the caption for Figure 2. We regret the error.

In Jack R. Van Der Slik's review of *Computers and Politics* by James Danziger, William Dutton, Rob Kling, and Kenneth Kramer (1983, Vol. 77, p. 755), James Danziger should have been identified as a political scientist.

BOOK REVIEWS

American Government and Politics

The Carter Years: The President and Policy Making. Edited by M. Glenn Abernathy, Dilys M. Hill, and Phil Williams. (New York: St. Martin's Press, 1984. Pp. viii + 227. \$22.50.)

Great expectations accompanied the start of the Carter presidency. Jimmy Carter had made many promises during the 1976 presidential campaign. Toward an economy burdened by stagflation, he promised noninflationary economic growth. Toward a people angry over various socioeconomic inequalities, he promised consumer protection, welfare, and tax reforms. Toward those Americans frustrated over governmental inefficiency, he promised to reorganize the bureaucracy and balance the budget. Toward a public growth cynical over Kissingerian Realpolitik, he promised a moralistic defense and foreign policy. Above all, he promised "a government as good as its people." Within four years these expectations had turned to dust. Defeated by the massive Reagan landslide, Carter left office with a reputation of incompetence and defeat. What happened? This question constitutes the focus of the 10 contributors to *The Carter Years: The President and Policy Making*.

The book's introduction suggests that initially Carter was placed at a disadvantage because of a resurgent Congress, a declining presidency, and a growing conservative mood within the nation, factors that would make the achievement of Carter's goals uncertain. These problems could only be overcome by a president who could impose a vision of what he wanted to accomplish, set priorities, and have a style, staff, and strategy that could work with Congress (p. 10). Subsequent essays indicate that, to one degree or another, ineffective communication of goals, disordered priorities, or inappropriate modes of legislative style added up to unrealized policies. For example, in his essay on domestic policy, Dilys Hill points out that Carter's tendency to overload the policy agenda, his failure to specify which proposals were of primary rather than secondary importance, and his aloofness toward the policy passage stage of legislation not only led to congressional rejection of most of Carter's proposals, but also to an image of amateurishness, a

deadly reputation in Washington. Hill also notes that Carter's proclivity to propose sweeping, comprehensive policy in a political system that tends toward incrementalism was inappropriate (p. 30). By the end of his term, Carter found Congress rejecting his proposals in such areas as health policy, tax reform, welfare reform, urban reform, campaign reform, and consumer protection. Although he was somewhat successful in the area of energy policy, much of that success was achieved amidst the politically damaging spectacle of long gas lines in the summer of 1979.

Raymond Moore's article on Carter's foreign policy notes that Carter wished to deemphasize East-West issues and in its place build a global community upon the moral pillars of human rights, disarmament, the resolution of regional conflict in Southern Africa and the Middle East, and more developmental aid for the Third World. However, only through gaining the Soviet Union's agreement to work for a common agenda—an agreement Carter never sought—could most of these goals be realized. Carter's failure to impose his idealistic vision on the Soviet Union and others pointed to the ultimate failure of his foreign policy.

Phil Williams's essay on defense argues that Carter's inability to get such key actors as the Armed Services, U.S. public opinion, and the Soviet Union to share his moralistic defense views led him to policy failure.

The Carter Years is a serviceable book; it presents its readers with a good description of what took place during the Carter years. What the book fails to do is explain adequately why events proceeded as they did. It fails to flesh out why Carter was unable to impose his vision on the domestic and foreign policy arenas, why he failed to order his priorities, or why he failed to develop a proper legislative-leadership style. The answers most of the contributors give to these questions seem rather facile. Is it enough to conclude that Carter's scattershot approach to legislation derived from his feeling that if he made many proposals, at least some would pass (p. 179)? Were Carter's problems with imposing his vision on domestic and foreign policy actors merely due to his inadequate communication skills? It could be

that the underlying cause of these problems was due to Carter's ambiguous feelings toward power. Or it could be that Carter's southern heritage influenced his presidential behavior. Clifton and Pierce McCleskey, authors of the book's best essay, suggest just that. They indicate that Carter developed a distaste for political parties from his experience as a Georgia politician, where the Democratic party was often incompetent and sometimes corrupt. Unfortunately, few of the other essays display this analytical power.

Interestingly, Carter's greatest achievement, the Camp David accords, which are hardly mentioned in the book, offers an example of where Carter's southern populist roots could be positively utilized and his (theoretically) ambiguous attitudes toward power could be resolved. For in his meetings with Sadat and Begin, who like himself were deeply religious, Carter could fully seek to accomplish the Christian goals of peace and goodwill and believe that power could be translated into God's missionary work on earth.

LESLIE SCHUSTER

New York Institute of Technology

The Supreme Court and Constitutional Democracy. By John Agresto. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 182. \$25.00, cloth; \$7.95, paper.)

John Agresto has written a book that tries to provide an outline for a new solution to the perennial problem of judicial review and democracy. "How," he asks, "do we, as a nation, give the Court the power and opportunity to guide us through our living and developing Constitution and still prevent the Court from substituting its principles of the Constitution, and elevating its rule over ours? In that question is the whole tangled problem this book has sought to unroll" (p. 160).

Agresto makes use of historical analysis of the origin and growth of judicial review to present a picture of an early judicial review deeply rooted in the principle of separation of powers. With Lincoln he deplores the emergence of the notion that judicial interpretation of the Constitution is final and binding on the other branches. Calling for a restoration of checks on the Court, he defends the effectiveness of Congress' power to ask the Court to reconsider its decisions (especially when supported by the force of public and scholarly opinion and timely use of the appointment

power). Yet, apparently sensing the inadequacy of such checks, Agresto calls for scholars to consider how they may be used more effectively.

The last chapter elaborates more fully the two goods Agresto wants to achieve at the same time. The Court is called upon to fulfill an important role in our society: not just checking legislation, but investigating questions of political and moral philosophy, fostering the growth of evolving national principles. At the same time, this must not be permitted to yield judicial oligarchy, especially with the developing power of the courts to give affirmative commands. "Checked activism" is the *via media* whereby these twin goals are to be achieved.

But "checked activism" is not the answer, in my opinion. The reasons why are hinted at in Agresto's own ambivalent attitude toward the Framers. He clearly has a high regard for them—no work is cited more often than *The Federalist*. But ultimately, he departs from their understanding of judicial power. His most frequent citation to describe the "activism" side of his "checked activism" is to Alexander Bickel, who expressly rejected *Marbury v. Madison* as too narrow a rationale. Like Bickel, but unlike the Founders I think, Agresto argues that the judiciary has been entrusted with the task of developing our evolving ideals and principles, seeing in them implications which the Framers did not have sufficient foresight to see. Perhaps it is not surprising, then, that Agresto implicitly criticizes the Framers for not attaching more effective checks to the judiciary (e.g., allowing an extraordinary congressional majority to override an exercise of judicial review). The Framers did not need such a check because they provided for a more limited judicial power, which would employ judicial review simply to enforce clear constitutional commands. I think that the Framers would have been rightly suspicious of whether "checked activism" would be sufficiently checked.

But my concerns about the solution proposed by this book should not obscure its considerable merit. *The Supreme Court and Constitutional Democracy* deals with a question which is the permanent question about national judicial power in America, and it makes a substantial contribution to our contemporary discussion by reminding us of the context of judicial review as it was originally conceived: separation of powers and checks and balances. Only relatively recently in American history has the notion of the finality of Court interpretations ("the Constitution is what the judges say it is") become so widespread, and Agresto's book shows why this view is such an unfortunate one. If it is not successful in its attempt to attain the best of both worlds (constitutional restraint and judicial activism), it does focus our

attention on the wisdom of the founding, which may point us in the right direction.

CHRISTOPHER WOLFE

Marquette University

Strong Democracy: Participatory Politics for a New Age. By Benjamin R. Barber (Berkeley: University of California Press, 1984. Pp. xvi + 320. \$16.95.)

Thomas Jefferson, Morris Janowitz (*The Reconstruction of Patriotism: Educating for Civic Consciousness*, University of Chicago Press, 1984), Harry Boyte (*The Backyard Revolution: Understanding the New Citizen Movement*, Temple University Press, 1981), and now Benjamin Barber have all made seminal contributions to the growing literature on citizen participation. Barber begins his closely reasoned philosophic work with Jefferson's often-quoted "no safe depository of the ultimate power of the society but the people themselves." The theme of this perceptive analysis is found in my own favorite quotation from Jefferson: "It is rare that the public sentiment decides immorally or unwisely, and the individual who differs from it ought to distrust and examine well his own opinion." Those who refrain from "deciding," for example, by not voting at elections, signal the bankruptcy of democracy according to Barber.

He argues for "strong democracy" as being far superior to what he terms three inferior types of democracy: anarchist, realist, or minimalist. Barber defines his preferred type of democracy as being politics in the participatory mode, resolving conflict through a participatory process of ongoing, proximate self-legislation, and the creation of a political community capable of transforming dependent private individuals into free citizens and partial and private interests into public goods (p. 151).

Barber aims "to associate democracy with a civic culture nearer to the themes of participation, citizenship, and political activity that are democracy's central virtues. We must do so without falling victim to either the nostalgia for ancient, small-scale republics that has made so many communitarian theories seem irrelevant to modern life, or to the taste for monolithic collectivism that can turn large-scale direct democracy into plebiscitary tyranny" (p. 25).

In Barber's view, the need for politics arises when some action of public consequence becomes necessary, and thus a public choice must be made that is reasonable in the face of conflict despite the absence of an independent ground of judgment. His response to the need for public action is

strong democracy, where conflict is resolved through the creation of a political community capable of transforming dependent, private individuals into free citizens and partial and private interests into public goods (p. 132).

The institutional reforms to achieve strong democracy include first, strong democratic talk (neighborhood assemblies, television town meetings and a civic communications cooperative, civic education and equal access to information, representative town meetings, and office-holding by lot); second, strong democratic decision making (a national initiative and referendum process, electronic balloting such as the Qube module in Columbus, Ohio, vouchers, and the market approach to public choice); and third, strong democratic action (universal citizen service and related training and employment opportunities, local options, democracy in the workplace, recreating the neighborhood as a physical public space).

Barber concludes: "This program does not illustrate strong democracy; it *is* strong democracy. . . . in the end, human freedom will be found not in caverns of private solitude but in the noisy assemblies where women and men meet daily as citizens and discover in each other's talk the consolation of a common humanity" (pp. 307, 311).

Are Barber's proposals for the revitalization of citizenship realistic and feasible? Can they be implemented in the twentieth century via the New England town meetings that I have attended and studied? Do the proposals require that all citizens be philosopher kings? Would the National Issues Forums of the Domestic Policy Association satisfy Barber's requirements? Will the so-called decline of our political parties mark the rise of "strong democratic talk and decisions and actions" at the neighborhood level throughout the length and breadth of our nation? Time will tell; the odds are against Barber.

NORMAN N. GILL

Marquette University

The Impossible Presidency: Illusions and Realities of Executive Power. By Harold M. Barger. (Glenview, Ill.: Scott, Foresman, 1984. Pp. ix + 450. \$9.95, paper.)

If the late 1960s and early 1970s epitomized the period of the "imperial presidency," the post-imperial presidency is characterized as the "impossible presidency" in this readable and interesting book. It offers a provocative challenge to Arthur M. Schlesinger, Jr.'s thesis in *The Imperial Presidency* (Houghton Mifflin, 1973). Indeed, it challenges the primary contribution of

post-World War II historians of the American presidency by questioning the validity of the presidential greatness game of Arthur M. Schlesinger, Sr. (*Presidential Greatness*, Irvington Publishers, 1966). In brief, Barger suggests that American historians have led the gullible public and the elite media down a primrose path via catch words such as "presidential power" and "presidential greatness" that has contributed to many of the problems of the post-Eisenhower presidency.

Barger seriously questions whether presidents have too much power. Going far beyond the White House mouse problem in the Carter administration, Barger cites abundant evidence of presidential weakness in the presidential roles of legislator-leader, administrator-leader, and free world leader. The decline of political parties prevents consistent leadership in the new decentralized Congress, the bureaucracy has grown too large for presidential management, and the changing global situation prevents America from running the world by itself. The myth of presidential power leads to many of the unattainable expectations of presidents and the public.

A major source behind the myth of presidential power is the notion of presidential greatness. Barger traces the quest for the perfect American president or the philosopher king to the post-Eisenhower phenomenon of the disposable president, because recent incumbents are unable to measure up to the greatness of Washington, Lincoln, and FDR. Barger particularly faults the media for accepting the notion that presidential greatness should be the norm. Americans should not expect all presidents to be heroes.

Barger rejects the traditional global reforms that consume so much time of many political scientists as being largely as unrealistic as myths of presidential power and greatness. He is more willing to settle for smaller innovations that might actually be accomplished and have some impact for the good. For example, rather than advocating establishment of a single six-year term or the abolition of the electoral college, Barger suggests that it might be more realistic and helpful (because past reforms often have caused many of our present problems) for scholars to hold seminars on the presidency for neophyte presidents and their staffs. One might extend this to the media and presidential candidates.

Historians are likely to challenge the thesis of this book with the latest and most sophisticated effort in the presidential rating game that suggests that our recent presidents have been underestimated when placed in historical perspective. For example, Robert K. Murray and Tim H. Blessing have found in their extensive polls of American historians that, except for Nixon, the nine presidents from 1929 to 1979 are now rated as average

or above: "No other fifty year period in American history, except for the era of the Founding Fathers and the early Republic, surpasses that performance record" (*Journal of American History*, 1983, 70(3), 552). In other words, the thesis of inadequate leadership in the modern presidency may itself be mythical. In any case, this clearly written, engaging, and economical paperback makes for a competitive possibility in American presidency courses.

WILLIAM D. PEDERSON

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The Interest Group Society. By Jeffrey M. Berry. (Boston: Little, Brown, 1984. Pp. x + 244. \$8.95, paper.)

Berry has undertaken a careful and thoughtful analysis of the role of interest groups in the American political process. His treatment of them is a valuable contribution to the literature of American politics.

The growth in the number and importance of pressure groups on the American political scene is familiar to all observers of our political life. Berry refers to it, accurately, as "the advocacy explosion." He discusses it within the context of "Madison's dilemma"—the age-old problem of "curing the mischiefs of faction." Early in his study Berry reviews the influence that pluralist thought has had within the discipline of political science. He notes that there is a growing, but certainly not new, popular perception that the power of interest groups is in some respects unhealthy.

The book gives particular stress to the reality that the waxing of interest groups has come during a period of party decline. The general weakening of partisanship and the rise in issue voting have been closely linked. Increasingly, more and more citizens look to interest groups to speak for them in the political process. The growth of interest group politics—whatever the attendant problems—has broadened representation within the political system. Yet the decline of the parties and the proliferation of interest groups clearly has worked to the advantage of those already well represented in the political process. Particular attention is given to the growing success and effectiveness of business pressure groups, notably the Business Roundtable. Berry also stresses that the appearance of new interest groups reflects changes in technology and the ever-increasing complexity of our society.

The analysis of the internal organization of in-

terest groups is well done. Here the "iron law of oligarchy" is pervasive. Many of these organizations are dominated by a small cadre of key workers. But the diversity in internal organizational arrangement is of considerable interest. A key problem for many organizations—a problem not faced by the single-issue pressure groups—is over the allocation of resources among issues.

Berry's discussion of lobbying is pragmatic and sophisticated, with some excellent analysis of the recruitment and background of lobbyists. There is a fine description of advocacy tactics. The growing influence of law firms, public relations firms, and the preeminence of superstars such as Tommy Boggs and Robert K. Gray are fascinating parts of the phenomenon of lobbying.

As would be expected, there is a major stress on the growing power and significance of political action committees (PACs). The substantial increase in the amounts of money being poured into congressional and senatorial campaigns is staggering. Even so, an enormous amount of money is spent by many PACs on administrative overhead, with a relatively small part of the money donated actually reaching candidates. The heavy support which most PACs give to incumbents is well documented.

Berry concludes that on balance the growth of interest group politics has furthered the democratic process. But it has not been without costs to our political system. He clearly embraces the Madison dictum that faction must not be fought at the expense of political liberty. The prospect for curbing the influence of factions—the "policy maximizers"—must come from a revitalization and strengthening of the political parties, the "vote maximizers." Berry rejects the notion that weak parties are an inevitable condition of American political life. He specifically calls for reform in the campaign finance laws so as to reduce the dependence of candidates on PAC funds.

A noteworthy feature of the book is the excellent bibliography. Berry's study will be of significance not only to students of interest groups and political parties, but also to all who are concerned about the political processes of American democracy.

RAYMOND H. GUSTESON

Ohio University

Redefining Human Life: Reproductive Technologies and Social Policy. By Robert H. Blank. (Boulder, Colo.: Westview Press, 1984. Pp. xvii + 270. \$22.95.)

Abortion and Woman's Choice: The State, Sexuality and Reproductive Freedom. By Rosalind Pollack Petchesky. (New York: Longman, 1984. Pp. xii + 404. \$22.95.)

These two volumes attempt to reflect upon the devastatingly difficult issues of human reproduction facing policymakers and indeed the entire political and social system, issues accorded special poignancy by the almost uncontrollable advance of technology in the late twentieth century. A glance at the titles of the books might lead one to assume that Petchesky's is the more narrow consideration, whereas Blank's is the heroic attempt to grasp the entire universe of problems. In the reading one is led to the opposite conclusion, for although *Redefining Human Life* by Blank is a prodigious effort to review both technology and policy response to it, it is *Abortion and Woman's Choice*, Petchesky's massive and yet elegant statement, that provides us with the bold, sweeping theoretical perspectives on human reproduction with which we can stage the policy debate in the largest arena. Blank does yeoman duty in presenting traditional liberal understanding of human life overlaid with traditional liberal attempts to manage technocratic invasion of those traditional assumptions, but insight into redefinition requires the most serious inquiry into the limits of liberal ideology when confronted with such questions, and Blank provides us with less help in making such inquiry than one could wish, given his ambitions. Petchesky, on the other hand, comes to grips with the problem immediately, setting forth a brilliant if controversial challenge to liberal ideology that gives the remainder of the book a coherence often reaching the point of devastating persuasiveness and, although one may (and doubtless many will) choose to argue with her initial assumptions, her own argument proceeds from them with a relentless internal logic.

Both Blank and Petchesky devote considerable effort to locating human reproduction within larger historical, social, and political contexts. Petchesky thoroughly challenges liberal assumptions about these contexts, especially with regard to recent attention to reproductive freedom as an extension of the right to privacy. Her perspective is clear from the outset: She will argue that "reproduction generally, and fertility control in particular, must be understood as a historically determined, socially organized activity" (p. ix) the consequences of which, for women and society,

are as obscured by liberal analysis as they are limited by liberal policymaking. Her perspective is both feminist and socialist, but one of her most brilliant accomplishments is to reach beyond facile acceptance of usual feminist or socialist "party lines" to critically examine both as rigorously as she critiques liberalism. Her early chapters, providing historical accounts of reproduction and fertility control from a social rather than biological or technological emphasis, employ this perspective so effectively that any reader should come away with a greatly enriched understanding of the accumulated weight of values and practices burdening contemporary society; the history she structures and articulates contributes a genuinely original voice.

Blank too strives to seat his argument in an interactive depiction of reproductive behavior and the political system. But his theoretical framework is unrelievedly pluralistic/incrementalist. From such a perspective Blank is unable to give adequate attention to one crux of reproductive control: How shall the state, attempting to "balance interests," finally resolve policy dilemmas when interests *can't* be balanced, when all attentive actors concede that the game is zero sum? Thus, much of the book consists of stating present policy dilemmas caused by technology, trying to anticipate future dilemmas (but once again from a pluralistic, liberal stance alone), and providing a careful but all too familiar evaluation of the system's likelihood to disaggregate and bargain away the policy questions. The near absence of reference to feminist formulations of these problems also contributes to the straits Blank's arguments are in, and for logical rather than ideological reasons. Both Petchesky and Blank cogently describe the system's varying justifications through time for reproductive policy in terms of population control, eugenics, pronatalism (for certain classes), and especially in terms of medical establishment models. But because Blank does not take the feminist perspective into account, only Petchesky can examine the complete array of consequences these justifications produce now and for future attempts to resolve the dilemmas. Blank's argument finds itself in the awkward position, for example, of arguing that the maternal right to control her own body is clearly established, but simultaneously relies on evidence that the courts are already moving in the direction of controlling maternal behavior in the interests of fetal rights to a safe environment; Petchesky's analysis removes the apparent confusion here with her convincing argument that the "woman's right to control" is not now and never has been firmly established. Blank asserts that the woman "alone makes the ultimate decision [regarding 'fetal environment'] . . . the fact is that it is the actions of

the mother, not the father, which affects [*sic*] the fetus" (p. 146), but at a later point attributes drastically reduced sperm counts in the American male population to environmental hazards; what he does not emphasize is that mounting evidence shows that sperm and sperm producing capability are qualitatively as well as quantitatively at risk from environmental hazard. This is only one way in which it is not the woman alone who makes these ultimate decisions, but women and men and social institutions who are implicated. Petchesky can account for such a formulation, but Blank cannot; what is even more serious is that the incrementalist policymaking system that Blank painstakingly describes cannot. It is to be regretted that Blank limited the number of alternative perspectives from which to construct his thesis, because the extraordinary compendium of technical and political information he offers us requires much greater theoretical cohesion than it now has if it is to be a guide to scholars and political actors.

Both books should be read, although opponents of abortion will find both books especially difficult. But the questions both books address are difficult no matter what one's personal feelings. *Redefining Human Life* is most useful as a clear description of current technology and system response. *Abortion and Woman's Choice* is a statement of political theory that should become a part of all interested scholars' repertoires as well as an invaluable aid to policymakers faced with these harrowing choices.

SUE TOLLESON RINEHART

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Civil Liberties and American Democracy. By John Brigham. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xiii + 300. \$12.95, paper.)

This book is both a brief for and an exercise in "ideological jurisprudence," an approach that emphasizes the intellectual content of court decisions rather than the political or personal influences that may underlie them. Brigham acknowledges that politics plays a part in judicial decision making, but argues that political struggles that find their way to court are given shape and weight by the language and ideas of the Constitution and prior case law.

Brigham has produced an exhaustive summary of that case law organized under five comprehensive headings: freedom, due process, liberty, property, and equality. Noting that a chapter on property is something of an anomaly in a civil liberties treatise, Brigham contends that property

in American law has consisted of "settled or legitimate expectations" (p. 204) that deserve protection from state usurpation. "In this form," he concludes, "property is a fundamental right that we can again associate with civil liberties" (p. 205). One measure of Brigham's scholarship is that his index lists nearly 500 Supreme Court citations. Among other things, the book will serve as a useful reference work for the constitutional law student seeking a great volume of material reduced to manageable dimensions.

Several of Brigham's chapters are fairly straightforward syntheses of case law. Readers will appreciate Brigham's ability to connect and illuminate cases by focusing on ideas rather than outcomes. His discussion of due process in particular is a model of clarity and tight organization, all the more remarkable because it covers civil as well as criminal due process guarantees. Other chapters, however, have a fitful, start-and-stop quality about them, the result of Brigham's decision to give much more attention to some issues than to others. For example, in chapter 2, "Freedom," pornography is discussed in considerable detail, whereas freedom of religion is covered only in broad outline. Brigham explains that a more comprehensive discussion of the religion cases, with their often tortured lines of reasoning, would "focus on policy decisions and their consequences. It would not provide greater insight into the ideological structure of constitutional freedom" (p. 83). But the validity of Brigham's approach needs to be demonstrated rather than simply asserted; glossing over cases and issues that do not lend themselves to his framework tends to undercut Brigham's brief for ideology as a tool for analyzing constitutional law. Ideas are important in the law not merely because scholars say so, but because judges and lawyers treat them as important. When they do not, one expects the ideologically inclined author to offer some extended analysis of that failure.

Brigham's work bristles with challenging theses. Perhaps the most provocative is his argument that "the base for civil liberties is not the individual . . . but the citizen as person who was the source of a new kind of power" (p. 261). Because rights have meaning only in a social (or political) setting, they must be interpreted with reference to both their social causes and their consequences. Applying this standard, Brigham is critical of the doctrine of "pure tolerance," which he says fails to consider the social harm caused by pornography. He also deplores what he calls "pure equality," which celebrates "colorblindness" and "sex blindness" but ignores "the historical situation that produced the constitutional standard in the first place" (p. 246). While faulting the "color-blind" Supreme Court justices who handed Allan

Bakke his victory, Brigham also takes Equal Rights Amendment advocates to task for adopting language that promotes the goal of "pure equality" over that of protecting women against discrimination.

Brigham's critique of civil liberties law does credit to the intellectual tradition in which he works. One does wish that he had not chosen to speak in more than one voice. His first and last chapters, written in a textbook-like tone, are padded with material that any reader sophisticated enough to appreciate the insights of the middle chapters will find irrelevant. On the whole, however, this is "ideological jurisprudence" at its best: by taking legal ideas seriously, Brigham compels us to take *his* ideas seriously.

RICHARD J. MAIMAN

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Bureaucratic Power in Society. by Richard Chackerian and Gilbert Abcarian. (Chicago: Nelson-Hall, 1984. Pp. 208. \$23.95, cloth; \$11.95, paper.)

In this relatively small volume the authors construct a Maslow-Weber labyrinth and attempt to lead the reader to the exit. Unfortunately, even if one is well versed in a wide range of sociological concepts, the exit is at best elusive.

The volume starts with the assertion that "Today, there is a crisis of trust in governmental and corporate institutions" (p. 1). It ends with the conclusion that "Unfortunately, the decline in the legitimacy of bureaucratic power has been associated with a weakening ability of industrial bureaucracies to meet material needs, with family arrangements that are increasingly unable to provide adequate emotional support to children, and with a political system that is less controllable by its citizens and even less capable of supporting material needs" (p. 175). In between, Chackerian and Abcarian attempt to demonstrate how personal development (Abraham Maslow's self-actualization) has been inhibited by the diffusion of bureaucratic values (Max Weber) to primary institutions (family, school, and polity).

Chapter 1 states the issues to be explored: "The diffusion of bureaucratic values to primary institutions and the individual consequences of this diffusion. . ." (p. 12). The following chapters develop the major tenets of the work, for example, in chapter 2, the nature of bureaucracy (Weber) and human needs (Maslow); in chapters 3 and 4, the diffusion of bureaucratic values. The remainder of the work ostensibly develops ideal types for the family, polity, and school; examines the extent to which these primary institutions

meet the models; and concludes that there exists a crisis of trust and legitimacy resulting from the diffusion of bureaucratic (instrumental) values to primary institutions thereby voiding or weakening intrinsic values.

Although the objective and framework of this volume make sense intuitively, the substance of the work is presented confusingly, some is not relevant to the thesis, and the overall conclusions of the work are unsupported.

Chapter 3 is a review of the socialization process as it relates to diffusion of bureaucratic values to the family, school, and the polity. The chapter, "Socialization as Diffusion," is illustrative of the ambiguity of concepts, or the ambiguity of relationships among various concepts, evident throughout this work. The volume appears to be a compilation of concepts that are unassimilated into an understandable bureaucratic values diffusion and personal development model.

To illustrate, chapter 7 is one of three written to provide "Specification of ideal models [which] will provide the criteria for evaluating the actual forms of these institutions [family, school, and polity] . . ." (p. 70). Yet no ideal models are presented or evaluations made. Rather, chapter 7 sets out to "examine several aspects of [the] crisis in schooling. . ." (p. 111). The authors conclude by stating that "A 'hidden curriculum' teaches children to accept certain social myths as reality, in particular the virtues of product consumption and gratification of material wants" (p. 124). And, "educational research identifies bureaucracy and the diffusion of bureaucratic values as a major factor in the contemporary educational crisis" (p. 125).

These conclusions typify the problems with the work. At best, they are over simplifications; at worst, they ignore so many other relevant factors as to be incorrect. It seems to me that one can hardly ignore the impacts of the "individual rights" movements that dominated the U.S. political and social scene for most of the 1960s and 1970s, most of which ran directly counter to the imposition of standardized, bureaucratic values. One could argue that, at least for this time period, the emphasis on individual rights as made manifest in school integration, family values, women's roles, and voting rights had a greater impact on primary institutions than the diffusion of bureaucratic values. Perhaps one should ask females and blacks whether their personal development potential is greater or less now than before 1960.

The work concludes with the chapter "Loss of Trust and Legitimacy." The point made is that the "legitimization of the ideas of hierarchy and impersonality . . . have detracted from the ability of the family, school, and polity to perform . . .

their core functions of meeting individual needs for affection and personal growth" (p. 154). The net result for these primary institutions is a weakened "efficacy in generating trust and legitimacy" (p. 154).

Are the core functions of family, school, and polity to provide affection and personal growth? How do these relate to trust and legitimacy in governmental and corporate institutions? How does any of this relate to Maslow's highest level of human needs, self-actualization?

There is general acceptance of the notion that individuals' higher level needs (self-actualization in Maslow's terms) are difficult to satisfy in large, impersonal bureaucratic institutions. However, the conclusion that the instrumental relationships that characterize bureaucracies have become so diffused in our primary institutions that personal development is thwarted, and that this has resulted in a crisis of lack of trust and legitimacy in government, is not made convincingly.

GERALD W. JOHNSON

Auburn University

City Money: Political Processes, Fiscal Strain and Retrenchment. By Terry Nichols Clark and Lorna Crowley Ferguson. (New York: Columbia University Press, 1983. Pp. ix + 440. \$37.50, cloth; \$15.00, paper.)

From the mid-1970s, there has been rapid development of a substantial literature in the field of urban fiscal strain. Common to most of this literature has been the attempt to devise measures of fiscal strain and to offer plausible causes for, and feasible solutions to, this phenomenon. *City Money* can be viewed initially as a cut above the rest of the works in this area, because it represents a commendable effort to develop the following: (1) improved measures of fiscal strain; (2) a comprehensive model to explain strain, which was produced by diligently pulling together a disparate literature; (3) reasonable courses of action that cities may take to cope with this problem; and (4) indepth analyses of several cities.

Although not directly stated, it is implicit from a reading of this book that past studies have failed to capture the full scope of fiscal strain. Clark and Ferguson define fiscal strain as "poor adaptation of (1) fiscal policy outputs of the city government to (2) private sector activities. . ." (p. 45) and measure it with a ratio dividing (1) by (2). The various ratio indicators use four different numerators (general expenditures, own revenues, common functions, and debt) and five denominators (median family income, population change from 1960 to 1970, population change from 1970 to

1974, population change from 1974 to 1977, and city wealth index). The various combinations of numerators and denominators produce 20 separate indicators of strain used as different measures of the dependent variable for purposes of testing the model.

According to Clark and Ferguson, the construction of a model that increases our knowledge of the sources or causes of strain necessitates dealing with political decision making (p. viii). After noting that "past work on fiscal policy has often dismissed politics as idiosyncratic and hence amenable to anecdotal treatment" (p. viii), they offer a systems analysis model of urban fiscal strain that posits specific linkages between the economic base, political decision making, and urban fiscal strain (chap. 1). Much of the remainder of the book (chaps. 4-8) is concerned with presenting a sound theoretical justification for inclusion of the independent variables in the model (i.e., citizen preferences, organized group activity, political leader attitudes, white ethnic and black power influence, municipal employee unions, tax preferences of the middle class, and migration) and testing the significance of each with the aid of data from the Permanent Community Sample (PCS) of 62 cities. Whereas this portion of the book would probably be of greatest interest to those concerned with theory-building, chapter 10 could be most appreciated by the practitioner, because it takes the knowledge obtained from the testing of the model and translates it into viable strategies aimed at reducing fiscal strain.

Nevertheless, several troublesome aspects of the book potentially compromise its quality and usefulness. These areas of concern are as follows: (1) frequent use of an unorthodox significance level for testing hypotheses (i.e., $\alpha < .10$); (2) questionable shifts in the level of analysis (e.g., from city to county in migration analysis in chapter 8); and (3) incidences of making generalizations about influences of statistically insignificant variables (e.g., "1974-77 changes in employee numbers," pp. 164-165). Furthermore, the book often suffers from poorly constructed tables and figures and awkward sentence structure that serve to confuse and bewilder the reader. Finally, the nature of the PCS makes it difficult (if not impossible) to report findings on the basis of region, population size, and central city or suburban status. Yet each reader will have to make a judgment as to how serious these problems are and to what extent they undercut the utility of the book.

J. EDWIN BENTON

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Studying the Presidency. Edited by George C. Edwards III and Stephen J. Wayne. (Knoxville: University of Tennessee Press, 1983. Pp. viii + 312. \$9.95, paper.)

For the media and the public, the president is, if not the personification of government, at least its major component. By contrast, political scientists have devoted far more attention to the study of Congress. In his introduction, Stephen J. Wayne argues that this is due both to the institution of the presidency and the difficulty of obtaining the kind of data that lends itself to either quantitative analysis or reliable generalization. But he believes that scholars are beginning to overcome these problems and hopes that *Studying the Presidency* will "stimulate them further" (p. 10).

The book is divided into two parts, the first discussing different approaches taken by students of the presidency and the second providing a guide to research sources and techniques. Although the essays vary in quality, they are better integrated in goals and approaches than is usual in such collections. "Approaches," by Wayne, begins with a clear and sensible survey of the literature, dividing it into legal, institutional, political, and psychological perspectives. Although a good introduction for the beginning student, it will serve as no more than a review for those reasonably familiar with the field. More interesting are the remaining three selections. Building on work by Harry Eckstein, Norman C. Thomas demonstrates that case studies have suggested and tested important theoretical propositions. George C. Edwards III makes the case for quantitative analysis. Because so much has been written on this subject, his failure to break new ground is hardly surprising, but he does provide some useful cautions about the pitfalls involved in using aggregate data and what quantitative analysis cannot do. John Kessel's account of his own research serves to remind us that the polished products we read in books and journals often come from a series of gropings that resemble educated guesses more than confident theory-building.

The second part of the book concentrates on gathering data. It consists of three essays about data sources available in research libraries, two about presidential libraries, and two about interviewing presidential aides. Jennifer De Toro provides a list of available sources of information about the presidency, with a brief description of what each does and does not include, but she does not analyze the strengths and weaknesses of each or the problems they can pose for researchers.

A more critical review of sources on executive-legislative relations by G. Calvin MacKezie does better, but is hampered occasionally by sketchiness, as in his discussion of the *Congressional*

Record. MacKenzie suggests that one of the publication's major purposes is "to provide a verbatim transcript of everything said on the floor of both houses," but it is not until several paragraphs later that we learn that we should "be careful to try to distinguish that which was actually spoken on the floor from that which was merely inserted for publication. The *Record* includes some helpful keys for making that distinction, but none of these is fully reliable" (pp. 162-163). Not only should MacKenzie have told the reader what these keys and their limitations are, he should also have pointed out that members are permitted to "revise and extend" their remarks, which makes the *Record* less than reliable as a verbatim transcript. Louis Fisher's essay on legal sources could also use more length, because these are the sources most likely to be unfamiliar to political scientists. Fisher begins by defending the importance of the legal perspective. Because this part of his essay is too brief to convince any skeptics, it should either have been expanded into a full article for the first part of the book or omitted entirely, allowing his list and brief description of legal sources to have been expanded to include more critical discussion as well as harder to locate sources such as briefs and oral arguments.

Martha Joynt Kumar does a fine job of evaluating the utility and limitations of the material available in presidential libraries for understanding the decision-making process and developing and testing hypotheses. By contrast, Larry Berman mixes a few useful practical suggestions with long extracts from library materials, most of which can be obtained by writing to presidential libraries.

The two essays on interviewing provide the kind of contrast one expects between journalists and scholars. The journalist, Dom Bonafede, provides many useful do's and don'ts without exploring them too systematically or in too much depth. The political scientist, Joseph Pika, has written the best of the practical chapters in the book. In a nontechnical way, he explains how to gain access, prepare for the interview, decide what to ask and how to ask it, establish rapport, deal with unexpected problems, and analyze the resulting data.

The editors are to be commended for addressing an important area that needs attention. Unfortunately, the effort falls far short of the need. The articles by Thomas and Pika should be of wide interest, that of Kumar of interest to those who are considering utilizing presidential libraries, and the others primarily of interest to those little acquainted with research on the presidency.

BRUCE E. ALTSCHULER

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Public Sector Management, Systems and Ethics.
By Louise C. Gawthrop. (Bloomington: Indiana University Press, 1984. Pp. 173. \$22.50.)

There is no dearth of critical analyses of contemporary organizations in the United States. We know that bureaucracy has become dysfunctional, particularly in many government settings. Clearly, the classical bureaucratic organization continues to offer certain advantages such as the application of consistency, continuity, and predictability to the organizational environment through routinized decision-making and problem-solving procedures. The dilemma that must be confronted today, however, is that the organizational environment has grown exceedingly complex and well beyond the capacity of classical organizations to deal effectively with rapid social, economic, technological, and political change. The critical task today is to redesign our obsolete organizations to cope with the complexity of change.

Louise C. Gawthrop's critique of classical public organizations (his "network X"), although adequate, adds little to our knowledge or awareness. Rather, the real contribution of this brief but thoughtful and thought-provoking volume is in bringing together the notions of management, systems, and ethics into an integrated metasystem for analyzing and substantially redesigning our public organizations. Gawthrop thus moves beyond the conventional nail-biting and teeth-gnashing literature on the problems and shortcomings of modern organizations into new theoretical territory, advocating a novel way of thinking about organizations and demonstrating how redesign can make them more capable of continuously adapting to the rigorous demands of the future.

Drawing eclectically from the theoretical work of Ludwig von Bertalanffy and others, Gawthrop prefaces his argument with a discussion of general systems theory and its essential components of holism, teleology, hierarchy, and temporality. Applied to these four systems components are the "essential ontological concepts of sense of purpose, sense of consequence, sense of history, and sense of order" (p. 38). The result is a model of a new public sector organization, referred to by Gawthrop as "network Y," that promises to achieve "a horizontally focused, 'dynamic pathway' through society to a purposeful reality; a capacity to increase the level of anticipatory knowledge of the future; a capacity to monitor the changing causal texture of the social order on a day-to-day basis . . . and a capacity to enable public sector decision makers to undo the negatives of past policy, programmatic, and operating decisions" (p. 38). The principal objective is to redesign organizations so that public administra-

tors can strategically adapt to frequent, complex demands for change.

Recognizing that the bulk of government policy activity will continue to be composed of routinized, highly programmed performance activities (the organization's "maintenance system"), Gawthrop's proposed new organizational design integrates traditional program maintenance functions with future-oriented systems management and planning. The result is an organic, matrix-type organization that operates with strong information processing capabilities and little hierarchical differentiation, except at the level of the chief executive officer who maintains political and policy responsibility for the organization. Importantly, the network Y organization moves beyond the ateleological nature of systems design to "a transcendent sense of purpose" that encourages the capacities to find objective reality, to accept tension as value-enhancing, and to view "criticism and criticalness as positive components of qualitative growth" (p. 127). For public administrators, effective management becomes a product of individual responsibility for actions and a "critical consciousness" set within "a creative ethical framework that is conducive to a systems notion of organizational responsiveness and responsibility . . . [and] congruous to the basic tenets of democratic theory" (p. 137).

Gawthrop is disturbingly short on examples of network Y in practice. Nonetheless, while Gawthrop's "ethics of civility" may ultimately prove to be a pipe dream and network Y unattainable, *Public Sector Management, Systems, and Ethics* constitutes a major contribution to the literature of public administration generally and public organization theory specifically. It unquestionably elevates the level of debate on the shape of organizations of the future and is certain to leave its mark on the literature and, one would hope, on the practice of public administration.

RICHARD C. KEARNEY

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Campaigning for Congress. By Edie N. Goldenberg and Michael W. Traugott. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. 207. \$9.50, paper.)

Congressional election campaigns have been examined in only limited ways in congressional research. Analyses have dealt with losers more often than winners, studied contests in one state, and portrayed a single candidate's campaign. In *Campaigning for Congress*, Goldenberg and

Traugott move beyond these specialized accounts to provide a more general, thoroughgoing report on congressional campaigns. The book is both conceptually and empirically important. Conceptually, it presents campaigns as dynamic, interactive occurrences of the candidates together, rather than discrete enterprises of each candidate individually. Campaigns unfold over time, as the strategies of one candidate are inseparably linked to strategies of the other candidate. Additionally, the authors stress the degree of electoral uncertainty as a key element of the campaign. The interrelated strategies of how to run are ultimately defined by the uncertainty (or certainty) of who will win. Empirically, these themes are developed by a close examination of 86 House races in 1978. Data were gathered in each district from interviews of the campaign managers for both major party candidates, expenditure records measured at several points throughout the full pre-election period (1977 to 1978), media market information, and voters' survey responses.

Relying on the interview data, the authors characterize how campaign managers continually evaluate the electoral uncertainty of their races depending upon relative party strength and the shifting competitiveness of the candidates. From the managers' assessments, a typology is proposed depicting sure winners and vulnerable incumbents, sure losers and hopeful challengers. Notable differences exist in the ways these four types of candidates approach their campaigns. Although the discussion of managers' strategies and tactics is at times overly descriptive, it provides an inside view of how campaigners perceive the campaign.

The most intriguing part of the book focuses on the dynamics of campaign spending and candidate uncertainty. Although most research has measured the total aggregated impact of campaign spending on congressional races, Goldenberg and Traugott monitor quarterly candidate expenditures for the entire interelection period. They observe preemptive spending by incumbents who finance campaign activities well before their challengers begin their campaigns. Incumbents thereby minimize uncertainty early and foreclose later challenger efforts. In addition, evidence of reactive spending is uncovered as candidates spend competitively in races perceived to be close.

The book also breaks new ground in analyzing media influence on the campaign. Regardless of uncertainty, all candidates spend greatest amounts of money on media expenses. The authors calculate the efficiency of television and newspaper markets. The more closely the district and the market overlap, the more likely the campaign will be covered, and the more valuable money spent on media exposure will be.

Finally, the book considers voters' responses to the campaign. A model linking the campaign to voter turnout, information, and choice is suggested. Unfortunately, the direct link posed in the model is explored only by inference. Basic knowledge of voters is presented without considering key elements of the campaign as revealed in the campaign manager interviews. Integrating managers' perceptions of uncertainty and of the campaign with voters' attitudes and actions would have enriched the study.

Nevertheless, the book provides the most comprehensive treatment to date of congressional campaigns. The breadth of the data and the development of the themes of uncertainty and strategic actions and reactions of candidates make a worthwhile addition to understanding congressional elections.

LYN RAGSDALE

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TVA: Fifty Years of Grass-Roots Bureaucracy.

Edited by Erwin C. Hargrove and Paul K. Conkin. (Champaign: University of Illinois Press, 1984. Pp. ix + 345. \$24.95.)

This work provides social scientists with a rare opportunity to consider a large governmental agency over a long time frame. We are in a position to examine Philip Selznick's classic theory of cooptation (*TVA and the Grass Roots*, Harper & Row, 1966), Marver Bernstein's life cycle of the bureaucracy (*Regulating Business by Independent Commissions*, Greenwood Press, 1955), as well as other theoretical studies in the light of history. Although there are references throughout to various theoretical works, the various essays in this book focus more on the idiosyncracies of the TVA. With the exception of the theories noted above, the reader is required to put the TVA in a broader theoretical context. As Hargrove notes in his introduction, "We have written analytical history rather than theory" (p. xvi).

The 50 years of the TVA highlights some important themes. "Grass-roots democracy" was more talk than substance. The apolitical bureaucracy is a myth. Expertise is an important political resource for the bureaucracy. Bureaucracies need not ossify.

Although I fault the work for not developing the theoretical implications of organizational leadership, organizational change, bureaucratic power, and the like, this book is a wonderful resource. It is composed of 12 essays, each of which is meticulously researched, well written,

and full of insights on the changing role and changing light in which the TVA is viewed.

The work holds together very well. It is organized into four sections: "The Formative Years," "Leadership and Government," "Contemporary Problems," and "TVA and American Democracy." The first part presents the historical evolution of the TVA. Paul K. Conkin explains the legislative history of the TVA Act. The novelty of the idea allowed great bureaucratic discretion, thus great opportunity and great uncertainty. Richard Lowitt traces the early development through the glory years of World War II. He notes early conflicts and the tremendous public relations value of grass-roots democracy. It is clear that in reality those benefited were the rich farmers and not the grass roots. Wilmon H. Droze explains the TVA since 1945. This period is marked by intermittent warfare with Congress and a coalition of private power advocates. More recently, its close relationship with the AEC and its headlong thrust into nuclear energy has tarnished the TVA's image.

Part 2 includes an article by Hargrove which profiles the leadership styles of the seven TVA chairmen. He concludes that Bernstein's theory is not applicable, that leadership requires bargaining, management, and agenda-setting skills, and that, "The 'myth' of the apolitical organization has been a political resource sustained by politics" (p. 117). Avery Leiserson argues that political insulation provided by the TVA's organizational status has allowed it to be flexible. Although that insulation from politics gives the TVA a wide berth, a keen awareness of political reality has made the TVA successful and generally accountable. Vernon W. Ruttan believes that the TVA has been successful because it has had the human resource capability to adapt to a changing economic, technological, and political environment.

The third section highlights contemporary problems. The essays highlight some common themes that can be summarized as an arrogance of power. William Bruce Wheeler and Michael J. McDonald note an ambivalence about public involvement and a technocratic outlook. Dean Hill Rivkin states that public interest law forced TVA to consider social costs as part of their technocratic benefit-cost ratios. Richard A. Couto notes that TVA was a victim of its own rhetoric when low-cost power gave way to higher costs.

The last section deals with the democratic implications of the TVA. Cranford D. Goodwin deals with attempts to expand the Valley Authority concept and concludes that political timing and problems of coalition formation led to failure. William C. Havard notes the clash of technocratic values with humanistic values. The

early successes of the TVA represented a clear victory for the former. In the long run, however, the transformation of the South from rural to urban is not without its costs. Dewey Grantham also notes the ambiguous status of TVA. Certainly, it has been quite successful with regional development but has contributed less to social reform.

In sum, this is an interesting data base with many keen insights for students of legislative process, the bureaucracy, and public policy formation.

ROBERT W. KWEIT

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Presidents and Their Parties: Leadership or Neglect? Edited by Robert Harmel. (New York: Praeger Publishers, 1984. Pp. ix + 275. \$26.95.)

This edited volume makes a worthy contribution to the study of the American presidency by focusing on the president's role as party leader. In the past, the relationship between presidents and parties has been treated with perhaps a chapter in some textbooks on the presidency; this is the first sustained treatment of the subject in book form that I know of. This book attempts to survey the complex story of president-party relations, which according to Harmel are based on "an interplay of tradition, events and necessities of the moment, presidents' backgrounds and personal political skills, and the governmental, political and social context within which presidents and their parties coexist" (pp. 15-16). In order to do this survey, Harmel includes essays by Ralph Goldman, Harold Bass, Howard Reiter, Roger Brown, Sidney Milkis, George Edwards, III, Jeff Fishel, and two by himself.

Goldman argues in "The American President as Party Leader: A Synoptic History" that presidents fit into four factional types: the non-partisan, the subpartisan (a factional leader within the party), transpartisan (moving across party boundaries), and the partisan. Moreover, he details five cycles of presidential partisanship and names the cycles after the respective leader of the cycle: Madison, Van Buren, Chandler, the Barnum-Hanna cycle, and the Farley cycle. Goldman suggests that Reagan could be the new cyclical leader for the 1980s.

Harold Bass looks into relations between the White House and the national party organization and finds similar themes of White House dominance, White House selection of party staffers, and White House supervision of national party activities. Howard Reiter examines the resources presidents can use in trying to influence the out-

comes of national party conventions, and Roger Brown finds that "presidents as campaigners for their parties have been motivated by self-interest rather than altruistic urges to support and strengthen the party system" (p. 146). Sidney Milkis concentrates on Franklin D. Roosevelt's unsuccessful efforts to purge his party in 1938.

In two other important chapters, Edwards and Fishel deal with the president and the party in government. Edwards, in "Presidential Party Leadership in Congress," points out the severe limitations that American presidents have in their efforts to act as party leader for the U.S. Congress. Although the president may try to work with party leaders of the House and the Senate, and although he may hope that that shared policy preferences will guide congressional behavior, the president is often frustrated in his efforts to lead because of congressional decision making that is influenced by narrow district concerns, constituent service, and the pressures of reelection.

Fishel, in "Presidents, Parties, and Platforms: From Campaign Promise to Presidential Performance," makes significant points that platforms and candidate issue papers provide useful information about the broad outlines of expected presidential policy. Even though these documents may be unclear, they can provide directions in keeping elected officials accountable to some standards as measured by their fulfillment of promises. Of course Fishel is sophisticated enough to note the problems with this kind of study, such as promises that are ambiguous or that could not possibly be kept, yet he has built upon the works of other scholars in his attempt to use quantitative content analysis of presidential campaign speeches, position papers, and platform representations.

Harmel has done an excellent job of editing a volume to fill the void in the study of presidents and parties. Like any edited volume, the quality of the articles varies, but this volume is consistently on the mark in focusing the study of presidential-party relationships. As Harmel observes, "Whether they like it or not, party and presidency have become too interrelated over the years to allow either to totally dismiss the other. The future of the parties—and, to a lesser extent, the future effectiveness of the presidency—will be affected by the nature of their relationship" (p. 262). This volume goes a long way in helping us to understand those relationships.

JOHN ORMAN

Fairfield University

Strategies for Effective Desegregation: Lessons from Research. By Willis D. Hawley et al. (Lexington, Mass.: D.C. Heath, 1983. Pp. x + 210. \$23.95, cloth; \$13.95, paper.)

A joint effort by nine coauthors representing several disciplines, this book blends advocacy and analysis in a generally systematic and often compelling fashion. Each chapter employs a format of topical discussion followed by evidence from the literature and, when available, examples from desegregation experience. Hawley and associates offer readers (1) reviews of the school desegregation literature; (2) proposals to reduce racial isolation in public schools and to avoid resegregation in schools already desegregated; (3) recommendations for building student, parental, and community support for desegregation; (4) suggestions to change curriculum, student evaluation, instructional methods, and teacher training for the sake of "more effective" desegregation; and (5) an attack on the "new mythology" about school desegregation, said by the authors to combine pessimism and misinformation.

Academics will find the book most useful for discussion of the impact of different types of desegregation plans on white pupil loss, and a brief but very good treatment of reciprocal relationships between segregated schools and segregated housing.

A more general audience, including parents, school administrators, and others directly involved in school desegregation, will profit from chapter 7's discussion of how smaller schools can promote more teacher attention to individual pupils, perhaps improve educational quality, and otherwise help ease racial tensions during the first year or so of implementation. Other chapters offer useful suggestions for parental involvement in school activities, maintaining contact with families whose children have left the public schools, and publicizing positive aspects of desegregation.

To their credit, Hawley et al. declare that their proposals should not be regarded as "foolproof recipes" or suitable for all locales. Readers are invited to draw their own conclusions about the feasibility of each recommendation.

I suspect readers will heed this good advice when they come upon some of chapter 7's ideas to transform public schooling in the cause of interracial harmony and minority student self-esteem. Here desegregation is taken as a license to "innovate," and the "multi-ethnic" approach advocated appears bent on avoiding almost any scholastic situation in which pupils figure as individuals rather than members of categorical groups. Whether educational quality will be enhanced as a result is open to serious question, and widespread

doubts on this score surely contribute to the pessimism lamented by the authors.

The book would have benefited from more systematic discussion of the role of middle-class white pupils in school desegregation. Chapter 3 advocates reassignment of as many such students as possible to serve as achievement role models for minority and lower-income white classmates, but chapter 7's approach appears to minimize the white achievement thesis by accentuating racial differences and favoring "pluralistic" standards. Still another topic needing more development is minority diversity. Occasional references to limited-English proficiency and foreign-born students do little to disaggregate so variegated a grouping. Too often "minority" seems to mean "lower-income black."

Despite these reservations, *Strategies for Effective Desegregation* should be carefully read by academics, educational administrators, and members of the general public interested in one of the nation's most difficult and enduring controversies.

EMMETT H. BUELL, JR.

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The Frustration of Policy: Responses to Crime by American Cities. By Herbert Jacob. (Boston: Little, Brown, 1984. Pp. xiv + 188. \$9.95, paper.)

The Politics of Law and Order: Street Crime and Public Policy. By Stuart A. Scheingold. (New York: Longman, 1984. Pp. xiv + 238. \$25.00, cloth; \$13.50, paper.)

Street crime is a political issue in which symbolic gestures and the appearance of action often become substitutes for substantive policies. The two books reviewed here drive home this point in different but complementary ways.

Jacob's book is based on a massive investigation of the political histories of 10 cities and their criminal justice systems as they grappled with the rise in crime between 1948 and 1978. Douglas Yate's "ungovernable city" thesis (*The Ungovernable City*, MIT Press, 1977) provides Jacob with a basic perspective of the evidence indicating that although the cities expanded their law enforcement activities, "those expansions were not simply a response to crime" (p. 166), and for his conclusion that the governments did "many little things while pretending to pursue a grand strategy" and that often "they believed their pretensions" (p. 165). The political

saliency of crime rose throughout the 30-year period in all 10 cities, but at the same time local policy agendas grew more crowded, elections became more heated and closely fought, and divisive issues such as race relations arose which when combined reduced the chances for effective anticrime programs. These difficulties were compounded further by the fragmentation of local governments, the absence of cohesive policy-making bodies in the law enforcement area (which reflected the proliferation of local political power bases according to a rather controversial measure used by Jacob), the unrecognized fact that national forces shaped the dynamics of local crime patterns, and last but not least the lack of reliable technological knowledge to design programs. In the face of these obstacles and the tenacity of crime, it is little wonder city officials maintained the illusion they were responding to the problem.

Scheingold's perspective rests on his claim that "conflict criminology," which views crime as the "politicization of social conflict" (p. 28), offers a better vantage point to explore crime and its political impacts than the "mainstream" views of such scholars as James W. Wilson or the Marxist alternative of criminologists like Richard Quinney. After challenging the mainstream's assumptions about crime and reform as well as Marxist expectations regarding legal repression, Scheingold states the politics of law and order often is "just so much sound and fury" (p. 204). Its effects are problematic and less intense than these perspectives suggest because its impulses "tend to be refracted by the political and bureaucratic forces that control the course of criminal process" (p. 28). Scheingold's book, then, is an extended argument with mainstream and Marxist writers over the cultural antecedents and dynamics of popular fears about crime and attitudes about criminal punishment, the role of the news media and entertainment industry in perpetuating and shaping these views, and how the cultural politics of law and order reinforce the punitive views of the police on the street and reduce the chances for reform while clashing with the less punitively inclined values of the criminal courts.

Both books are strongest when discussing crime as a political issue and when analyzing the police response. Scheingold draws on existing research to show that public fears of crime and attitudes about punishment are at best weakly related to crime rates or personal victimization. Pointing at the newspapers, television, movies, and popular novels, he suggests the public learns about crime and how to respond to it from these sources rather than from experience. His chapters on the police summarize the literature on their occupational values and behavior on the streets and raise questions about the feasibility of alternative policing

styles under current conditions. Jacob's empirical data buttress much of Scheingold's argument. His analysis indicates that newspapers increasingly emphasized violent crimes while expanding their coverage of crime over the years. More importantly, the prominence of crime on urban policy agendas failed to reflect actual crime rates and patterns. The politics of crime, then, seems to have taken on a life of its own. This conclusion gains further strength from Jacob's fascinating look at the response of police departments. Whether the measure was changes in police expenditures, arrest rates and priorities, reorganization of departments, or appointments of new police chiefs, the driving force rarely was crime rates according to Jacob's analysis.

The weakest parts of the books are their sections on the criminal courts and their conclusions. Jacob can say little about sentencing trends because the necessary data were not available. He confines his analysis to changes in court resources and how appellate and local court decisions shaped local politics. There is an important chapter on changes in local ordinances and state criminal laws, particularly the recent move toward determinate sentencing, but the impact of these changes on local sentencing practices could not be documented easily. Scheingold defends the guilty plea process using a "bureaucratic justice" model and argues that "due process liberals" are co-opted by law and order advocates on the issue of limiting the discretion of court officials because they lack a normative theory of punishment. The force of his argument is weakened, however, by his misreading of the policy-oriented research in this area, which is chiefly concerned with determining how much punishment is needed to achieve different policy goals and the costs of these measures than with disparities and discretion *per se*. Finally, the conclusions of the two books are disappointing. Jacob's recommendation that sentences be made shorter in particular simply has no foundation in his book. Scheingold's argument for "neighborhood justice" similarly lacks much grounding in the discussion that precedes it, and it is unclear as to how the idea would operate given the cultural "constants" he takes pains to identify.

Despite their flaws, these books ought to find receptive markets. Jacob's book is concise and crisply written, the analysis is presented in a straightforward manner, and it addresses challenging political and policy questions. Scheingold's volume is less tightly organized in part because it is more ruminative, but also because the conflict criminology perspective gets blurred as the argument proceeds. Nonetheless, it stands as a useful alternative to the more orthodox mainstream and Marxist literature on the politics of

crime. Both books warrant the thoughtful attention of their readers.

ROY B. FLEMMING

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Firearms and Violence: Issues of Public Policy.

Edited by Don B. Kates, Jr. (San Francisco: Institute for Public Policy Research, 1984. Pp. xxxiii + 571. \$38.00.)

The evidence of this volume is that there is no direct link in the contemporary United States between changes in levels of gun ownership and interpersonal violence, or between gun control and reduction of violent crime. On the first of these points, careful quantitative analysis by Gary Kleck suggests that conventional wisdom has the causal connection wrong: in fact, increases in violent crime tend to lead to increased gun ownership, presumably for defensive purposes. On the second point, analysis by Joseph P. Magaddino and Marshall H. Medoff of the impact of state firearm control laws (cross-sectionally) and of the federal Gun Control Act of 1968 (longitudinally) shows no significant effects on violent crime rates.

Handguns are involved in approximately 25,000 homicide, suicide, and accident deaths yearly, but because the American stock numbers about 54 million handguns, it is implausible that general measures of registration or confiscation can have more than marginal effects. From the perspective of enforceability, approaches that emphasize the banning of handguns are even less likely to succeed than did Prohibition because of the size of the weapons stock, strong incentives for non-compliance (self-defense, armed robbery), and overload on the criminal justice system (Don B. Kates, Jr.). A formal market analysis by Steven Balkin and John F. McDonald of stricter handgun controls, based on plausible assumptions, suggests that offender use of guns would be less affected than any other uses. In fact, there are good reasons for thinking that attempts to control handguns only (Kleck), or even more narrowly "Saturday-night specials" (Paula D. McClain), would push potential gun abusers into greater reliance on more deadly long (and cut-off) weapons, with potentially disastrous effects on fatality rates in assaults and robberies.

These are some of the key conclusions of the careful and rigorous analyses included in this volume. These are not the conclusions of "gun nuts," although Kates, a civil rights lawyer and former law professor, is on record as a strong opponent of conventional gun-control efforts on civil libertarian grounds. He claims not to have "asked . . . the positions of the other contributors

on particular firearms policy options" (p. 524). Some of them, including Wright, Kleck, and David J. Bordua, have previously favored gun-control legislation. The evidence adduced here, including reanalysis of past findings which seemed to support gun control, has led them to modify their policy preferences. The results do not, however, lead to uncritical endorsement of the "no controls" position. The high rates of interpersonal violence in the United States have socio-economic origins; the availability of weapons is largely incidental. The key to any effective attempt to reduce violence by controlling guns, Kates concludes, is to "disarm . . . persons who have a high potential for gun misuse" (p. 531), which in practice means people with some past involvement in violent offenses (Kleck and Bordua).

Historical and constitutional dimensions of the gun control debate are also addressed. The Second Amendment's protection of "the right of the people to keep and bear Arms" is shown by Stephen P. Halbrook to have origins in classical political philosophy and by Joyce Lee Malcolm to be firmly rooted in pre-nineteenth century British political tradition.

If the weight of evidence indicates that the conventional approaches to gun control are contrary to the American legal tradition and are likely to be counterproductive, the question remains why they continue to be advocated. Public opinion, which is often claimed to be strongly on the side of gun control, is reanalyzed by Bordua, who concludes from his comparisons of contending poll results that "The problem is more one of elite curtailment of a popular liberty about which the public is largely apathetic than of elite interference with a reform backed by passionate public desire" (p. 69). In a more speculative essay, Raymond G. Kessler summarizes the "political functions of gun control," but unaccountably misses its most obvious function in contemporary America: It is easy advocacy for public figures who want to appear responsive to an intractable social problem. The only strident chapter in the volume, by William R. Tonso, accuses the intellectual and social scientific supporters of gun control of "cosmopolitan ethnocentrism." The rejoinder is evident in other chapters: Some of the most competent scholars working on the subject have been persuaded by the evidence that unilateral civilian disarmament is neither a workable nor potentially effective solution to interpersonal violence in America.

On the whole, the 17 chapters in this volume make a sound and important contribution to the ongoing policy debate over firearms control. It complements and extends in important respects the recent benchmark study by James D. Wright, Peter H. Rossi, and Kathleen Daly, *Under the*

Gun: Weapons, Crime, and Violence in America (Aldine Publishing, 1983). The present volume gives further credibility to a conclusion from the earlier study (p. 322): "unless we solve the problem of interpersonal hatred, it may not matter very much what we do about guns."

TED ROBERT GURR

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Uninformed Choice: The Failure of the New Presidential Nominating System. By Scott Keeter and Cliff Zukin. (New York: Praeger Publishers, 1983. Pp. xx + 247. \$29.95.)

Widespread reforms since 1968 have significantly transformed the presidential nominating process. *Uninformed Choice* evaluates the voter's role in this new system, drawing upon survey data from the *New York Times*/CBS News exit polls, the 1980 Center for Political Studies Election Study, and the Eagleton Institute's New Jersey poll. The study answers three important questions about public involvement in nominations.

First, how representative are primary voters? Keeter and Zukin found each party's primary electorate higher in SES than the adult population but demographically and politically similar to partisan voters in general elections. Because of their disproportionate influence, representativeness in the earliest primaries also was examined. Voters here were just as unrepresentative demographically and were *politically* distinctive as well. What states begin the nominating process does matter!

Second, what is the quality of public thinking about the candidates? Keeter and Zukin found that many Americans possess only superficial knowledge of the candidates. In 1980 most voters could not identify the major candidates' ideologies (e.g., a bare majority could place Carter on the left-right scale; even fewer could locate Kennedy or Reagan) or their positions on the central issues (e.g., by September the average citizen could position Reagan on only 2.5 of four issues). Citizen knowledge was greatest about candidate personal traits. In public thinking, style dominates substance!

Third, how much does the campaign itself contribute to voter education? Public knowledge of the candidates increased little during the campaign, especially on issue positions and ideology. The learning that did take place largely involved candidate personal traits. Even when the long campaign is viewed as an educational process, style predominates.

Voters learned the most from the early contests, but they learned little more than that a candidate was a winner. Like (because of?) the media, the

public paid more attention to the "horse race" than the substance of the campaign. Indeed, so weakly anchored in an information base were preferences that the candidates' national popularity seemed to be conditioned by their fate in individual state contests. That the public is looking for a winner lends even more strategic importance to the sequencing of contests. Quite ironically, this also suggests that voters may share with party leaders a desire to put forward their party's most competitive candidate.

These disquieting results lead Keeter and Zukin to conclude that the current system for nominating presidential candidates is seriously flawed. They recommend changes, some of them implemented by the Democrats for 1984, to overcome these deficiencies. But all in all, their diagnosis is more impressive than their prescription.

Uninformed Choice is an important book, must reading for all concerned with our presidential nominating process. Yet the study has significant limitations. First, the role of group identifications in a voter's calculus is totally ignored. Second, the analysis is based on only two election campaigns, mostly 1980. The 1980 contest provides a test of voter knowledge under circumstances conducive to an informed electorate, because the major candidates that year were so distinctive and prominent. What might we find in other years? Third, as the authors concede, they study public opinion during the nominating process rather than voter behavior in actual contests. Their limited evidence on reactions to real primary elections suggests that voter choice may be more informed. Obviously we need information about voter choices at the polls rather than public opinion in the polls. Finally, real benchmarks need to be employed in evaluating citizen thinking during the nominating process. Are Americans less informed about candidates for nomination than about the major party nominees in the general election? If not, then Keeter and Zukin are indicting unmediated public opinion in general, not just the new nominating system.

PAUL ALLEN BECK

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Politics and Religious Consciousness in America. By George Armstrong Kelly. (New Brunswick, N.J.: Transaction Books, 1984. Pp. xi + 312. \$29.95.)

Those who have read George Armstrong Kelly's provocative article on religion and politics in the Winter, 1982 *Daedalus* ("Faith, Freedom and Disenchantment," pp. 127-149) will be pleased with this elaboration on the motifs introduced

there. Kelly argues that the pathologies of American political institutions not only mimic the diseases of contemporary religious life, but also that "troubles in both spheres are more commonly related than is generally supposed" (p. 1). Conceding that the parallels are not always exact, he contends that the secularizing forces which have "reduced religion to privatized belief" are often the same sort "that challenge the profane community and the dignity of public life" (p. 30).

Although Kelly's work is especially timely, his analysis does not fit comfortably into any of the camps produced by the recent hostilities over the role of religion in politics. After delivering a much-deserved, if belated, slap at political scientists for their failure to take religion seriously, Kelly posits religion and politics as two distinct but overlapping control systems in human society. Although the domain of religion is the "sacred," and that of politics the "profane," the two systems are inevitably and constantly interacting, sometimes in tension and conflict, often in peaceful and fruitful collaboration.

Kelly examines this interaction in a learned and exceedingly subtle essay, taking off from reflections on American religion by observers as diverse as Tocqueville and Emerson, Alexander Campbell and Horace Bushnell, William James, and Robert Bellah. The topics covered are too numerous to detail, but several particularly valuable passages deserve mention: the analysis and critique of Tocqueville's view of the contribution of religion to civil liberty; the tracing of the "intricate and fragile" link between biblical prophecy and national eschatology; the delineation of parallel structures of "disenchantment" in modern religion and politics; and, the devastating critique of "civil religion," a notion so fashionable among American academics. Although Kelly's narrative will be most accessible to those of philosophic bent, the central theoretical contention and the many subsidiary themes invite—indeed, beg for—rigorous empirical investigation.

Kelly's conclusions, he admits, are "unbecomingly seditious in the company of Western humanism" (p. 31). Although secularization may have freed individuals from the bonds of tradition, it has not produced a public ethic superior to that of Judeo-Christianity. For Kelly, the idea of private morality so beloved by secularists is close to nonsense. Morality will always be "in politics," for it is "the perplexing minefield" in which we all live. "Traditionally, religion has animated it, law has given it a shell to inhabit, opinion has calibrated it, and politics has been domesticated by its force and persuasion" (p. 250).

Despite the inevitability of moral judgement—and hence, religion—in politics, Kelly is not optimistic that a new public ethic comparable to the

former Judeo-Christian one can be built in America. Today's mainline Protestantism, which contributed much to that earlier ethic, is so secularized as to provide the very model for the ailments afflicting both religion and politics. Sectarian fundamentalism is more vigorous, but alternates between creating "multiple moral territories" and complete privatization. Even American Catholicism, with a much longer tradition, "has embarked on its own versions of privatization and secularization" and is not a likely source for revival (p. 260).

Kelly is convinced, however, that "deliberate architects" will be required to rummage in the ruins of American religious and political traditions to find the materials to reconstruct a public ethic. Although Kelly does not begin this work himself, readers of this volume will be grateful to him for clearing away much of the debris and revealing at least some of the old foundations.

JAMES L. GUTH

Furman University

The Regulation of American Federalism. By Donald F. Kettl. (Baton Rouge: Louisiana State University Press, 1983. Pp. xviii + 195. \$20.00.)

This thoughtful and timely book examines the federal government's growing regulation of our subnational governments. Kettl defines his basic question as "which piper calls the tune when one government raises the money and another spends it?" (p. 154). He examines the relationship between fiscal dependence and political independence generally within the Community Development Block Grants program as a case study.

Kettl defines regulation as "a dynamic process with the potential for substantially altering the distribution of money and power, a political process in the largest sense of that term" (pp. 22-23). Regulation as the principal feature of federal attempts to control intergovernmental grant programs emerged in the 1970s after attempts to control by category of grant and control by application during earlier years. Although an individual regulation may have a laudable goal, the package of regulations—particularly cross-cutting rules that apply across the board to many programs—have created four burdens for state and local governments: high financial costs for compliance and creation of new agencies to keep records and prove compliance, uncertainties in planning projects, adversarial proceedings among governments, and token compliance to meet terms of the rules without conforming with their goals (pp. 8-14).

State and local governments are described as

"uncertain brides in the partnership of American federalism" (p. xv) who along the way collected something old, new, borrowed, and blue. In turn, these items are: sincere but not always effective attempts to streamline regulations, the strengthened role of the Office of Management and Budget in dealing with regulations, the use of block grants to loosen federal controls, and new programs that create "too many responsibilities supported with too little money" (p. 140).

Reagan, according to Kettl, is the "first American president to succeed in firmly establishing a public image of states and cities as regulated governments" and with success in "creating a new partnership" (p. 130). Reforms are difficult because the number of players in the political process of regulation has increased and includes an influential track dominated by professionals hidden from public view and a public track that is open and less important.

The heart of the problem for Kettl is that wider uniformity of federal regulation robs state and local governments of the discretion the programs promised. There are no uniformly good and bad guys in the study, but Kettl does spend more time on the results of regulation rather than on its causes.

Kettl sees the process of regulation as fitting naturally in the political process of federalism because choices have to be made between uniformity and diversity. In his examination of the decentralization alternative to centralized regulation, Kettl believes a recasting of the categories of federal aid would be a worthy stopgap reform, because 90% of federal grant programs account for only 10% of the money. Improved administrative mechanisms for reviewing state and local compliance with rules is a more basic reform, and more responsibility should be decentralized to field offices. Although these reforms neither will be easily achieved nor prove the solution, Kettl concludes that "Only ironically by renouncing the obsession with controlling administrative detail can federal control over those matters truly of national importance be strengthened" (p. 171).

This volume is one in a series on the American presidency sponsored by the Miller Center of Public Affairs at the University of Virginia. It is a worthy study and reflects an original approach to the problem of reconsidering the need for effective central leadership with the constitutional imperatives of limited government and divided but shared power. Kettl's analysis of the problems of regulation is more discerning than his approach for alleviating them, but the book has a message that deserves a wide and thoughtful audience.

GEORGE S. BLAIR

Claremont Graduate School

Agendas, Alternatives, and Public Policy. By John W. Kingdon. (Boston: Little, Brown, 1984. Pp. ix + 240. \$7.95, paper.)

How the national agenda is set is the most complex and least understood aspect of the policy-making process. Kingdon's volume is an important contribution toward a better understanding of this important subject.

Kingdon's study is based on 247 lengthy and detailed interviews with congressional staff, executive branch political appointees, and upper-level civil servants and presidential staff in 1976, 1977, 1978, and 1979. His attention is primarily devoted to two domestic policy areas, health and transportation. Faculty grants, a grant from the National Science Foundation, and a John Simon Guggenheim Foundation fellowship supported the research and writing effort. The interviews form a panel study, with tracking of the interviews and the content of the agendas over time. A snowballing approach was applied to expand the sample to reach influential or top decision makers in the health and transportation policy fields. Kingdon also conducted extensive research of government documents and written materials focusing on 23 cognate case studies in the health and transportation fields. Kingdon wisely concentrates on two policy areas, the breadth of each being substantial enough to convince me that he can make accurate generalizations about much of domestic policy-making. Moreover, the interviews and the case studies provide a formidable and convincing data base on which to develop this study.

Kingdon found that, although no one set of actors dominates any phase of the agenda-setting process, the president and his appointees have substantial influence in agenda-setting; Congress is effective in agenda-setting and alternative specification (the selection of alternatives from which the choice is made); interest groups, academics, researchers, and consultants tend to be influential at the alternative specification phase; and the media are less influential than expected, although they appear to have impact on public opinion, which in turn influences politicians.

Kingdon provides an insightful chapter (4) on theories of the policymaking process and then develops his own revised model. He concludes that the federal government "is seen as an organized anarchy," with more emphasis on "organized" than "anarchy." The remaining five chapters of his book provide convincing support for this theory. Kingdon observes three major "process streams" in federal policymaking: problem recognition, the formation and refining of policy proposals, and politics. These operate independently of each other and can be best understood by patterned (systematic) and sometimes

random events that open and close policy windows that link the various streams and couple agenda issues.

Kingdon has greatly improved our understanding of the policymaking labyrinth. This work is a major contribution to this field. This study is appropriately grounded in quantified field research, and it is thoroughly and exhaustively researched. The volume is well organized, lucid, concise, scholarly, well edited, and delightful to read.

CLIFFORD J. WIRTH

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The Heyday of American Communism: The Depression Decade. By Harvey Klehr. (New York: Basic Books, 1984. Pp. xiv + 511. \$26.50.)

Harvey Klehr's massive book fills a long-standing gap in the history of American communism. As part of a large-scale study sponsored during the 1950s by the Fund for the Republic, Theodore Draper published two brilliant volumes, *The Roots of American Communism* (Viking Press, 1957) and *American Communism and Soviet Russia* (Viking Press, 1960), which detail the history of the Party through the 1920s. He then abandoned the project and, although other studies have appeared dealing with the 1940s and 1950s, the story of the critical decade of the Great Depression has not been told in detail.

Fortunately, Draper deposited his valuable collection of research materials, plus drafts of several early chapters of a projected work on this period, in the library of Emory University. Building on this base, and adding much original research of his own, Klehr has provided an appropriate successor to Draper's now classic studies.

The history of the period falls into two parts, with a transitional stage coming between. The first phase, notable for its revolutionary radicalism, extended from 1929 to 1934 and is referred to in the histories of communism as the Third Period. The second phase ran from 1935 to 1939 and encompasses the more widely known Popular Front.

The book provides detailed accounts of Communist party activity among the unemployed and the intellectuals, the youth and blacks, agricultural workers, and in such states as New York, Minnesota, California, and parts of the South where its political success was greatest. Klehr says little about such episodes as the infiltration of Hollywood or the activities of the Abraham Lincoln Brigade in Spain on the grounds that they have been extensively written about elsewhere. Students of American political thought might hope for more about the intellectuals, but they

too have been written about at length, and Klehr handles this important subject very well given the space limitations of an already large book. Although he does briefly discuss the underground life of the Party, there is little here about espionage activities, and one wonders if this reflects Klehr's assessment of the relative importance of this topic which was to become so bitterly controversial during the 1940s and 1950s.

This book contains great quantities of detailed information. It is very well documented and possesses an outstanding index. Although a formal bibliography of the published sources used would have been useful, these qualities will make this book an invaluable reference source.

The Heyday of American Communism will no doubt remain the standard work on its subject for many years to come, although veterans of the movement may see some things in a different perspective. Thus, Hal Draper, in the course of a generally laudatory discussion, finds Klehr to be weak on the Third Period in general and to misunderstand the impact of the Franco-Soviet pact of 1935 in particular ("Pie in the Sky," *New York Review of Books*, May 10, 1984, pp. 25, 28-31). Nonetheless, the factual detail Klehr has amassed make this a very useful effort.

However, the sheer volume of information suggests the book's major weakness. The study is largely descriptive and lacks an interpretive framework adequate to the complexity of the subject. The history of the American Communist party in this period is part of the often bizarre history of Stalinism. Stalinism is in need of explanation—of a theory. Without such a theory, one can be swallowed up in detail. For such a framework, the prospective reader might well turn in advance to the brilliant and suggestive concluding chapter of *The American Communist Party* by Irving Howe and Lewis Coser (Da Capo Press, 1962). Such a perspective will help the reader to extract even more from Klehr's labors.

JAMES P. YOUNG

State University of New York, Binghamton

Money and Politics in the United States: Financing Elections in the 1980s. Edited by Michael J. Malbin. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. 324. \$12.95, paper.)

The conventional wisdom on campaign finance, particularly that held by journalists, is that political money threatens democracy by corrupting officials. Scholars who study campaign spending find, to the contrary, that political money furthers democracy by increasing electoral competition. In this vein, *Money and Politics* con-

cludes, one might say, that all money tends to root out evil.

Money and Politics responds to most of the complaints made about campaign finance. The most common critique is that elections cost too much. Herbert Alexander rebuts this charge in his survey of 1980 presidential campaign expenditures, concluding that campaign spending supplies necessary information to voters. In this regard American elections are underfunded, although less so than in the past. However, well-intentioned regulations often produce expenditures that provide less and less helpful information. Ruth Jones's survey of campaign finance in the states, and Richard Smolka's discussion of recent court cases, arrive at similar conclusions.

The fear of large expenditures results from the assumption that those who spend the most win elections. Gary Jacobson demonstrates the inadequacy of this assumption. In recent congressional elections, increased spending by incumbent candidates did not improve their performance at the polls; however, the more money challenging candidates spent, the more votes they received. Spending by both kinds of candidates mattered only in very close elections. Here funds from PACs and parties (particularly the Republicans) helped marginal incumbents stay in office. However, it was money from PACs and parties that made these elections close in the first place by funding challengers.

Fear of political money often stems from disapproval of those who provide campaign funds. Various critics dislike, in turn, well-financed parties, affluent interest groups, and well-heeled ideological PACs. *Money and Politics* contains good analyses of each of these sources of funds. David Adamany argues that campaign finance has helped create a strong, centralized, and professional party for the Republicans; the Democrats are attempting to emulate their success. The chief beneficiaries of strong parties are weak candidates, those most in need of financial assistance. But because of the funds supplied by PACs, parties do not dominate campaign finance.

PACs often reinforce the tendencies of the parties, however. Theodore Eismeier and Phillip Pollock find that interest group PACs are very diverse, and for the typical PAC, ideology is as important as self-interest. Although much PAC money goes to incumbents, PACs with ideological concerns contribute to many challengers. Margaret Latas finds that ideological PACs behave similarly. Left- and right-wing PACs imitate each others' strategy and tactics, including the strong backing of candidates in close elections.

Unfortunately, this volume does not include a study dealing with the allegation that money buys votes in Congress. Here too, the weight of schol-

arship suggests that money rarely influences congressional behavior, and when it does, it challenges the established order. Several of the above authors arrive at this conclusion indirectly, however.

Collections of essays are often disjointed, but thanks to Michael Malbin's excellent introduction and concluding essay, *Money and Politics* fits together well. Malbin deals principally with proposals for campaign finance reform, unmasking the notion underlying much of the foregoing criticism: Special interest money corrupts politics. He argues the opposite: Special interest money is an unavoidable, and indeed desirable, aspect of politics. Limiting political money will not hinder the contacts groups have with officials, only their electoral activities, particularly the desirable funding of risky candidacies.

Money and Politics is a well-conceived and executed book. I highly recommend it, particularly to political journalists.

JOHN C. GREEN

Furman University

Changing U.S. Military Manpower Realities.

Edited by Franklin D. Margiotta, James Brown, and Michael J. Collins. (Boulder, Colo.: Westview Press, 1983. Pp. xviii + 267. \$25.00.)

Paradoxes of Power: The Military Establishment in the Eighties. By Adam Yarmolinsky and Gregory D. Foster. (Bloomington: Indiana University Press, 1983. Pp. ii + 154. \$15.00.)

The two books under review utilize significantly different approaches but pursue a parallel course of attempting to evaluate the current U.S. national security system and its future in the 1980s.

In 1971 Adam Yarmolinsky published a sound analysis, *The Military Establishment: Its Impact on American Society* (Harper & Row). *Paradoxes of Power* is a recent revision, a well-written, concise overview of U.S. military policies that contrasts what Americans expect of their defense establishment and what the system delivers. The fact that the military fails to provide what is expected is not always their fault. Society's expectations often are unrealistic, and the American people through Congress do not always provide the capabilities for the military to meet these expectations.

This is a good book for the layman because, as Yarmolinsky notes, the military establishment "is so heavily and extensively involved in almost every aspect of American life that we cannot understand our own society without learning something about . . . this elemental component"

(p. 2). In subsequent chapters the book examines the military impact upon and interaction with government, the economy, and society. Concluding chapters examine arms control issues and four alternate scenarios of the future.

Four "paradoxes" underlie this analysis: the need to deter because defense in the nuclear age is impossible, the inability of superpowers to use the very force that made them such, the conflicting basis of civilian-military control, and the politically volatile debate between the best approach to peace—through enhanced security systems or through arms reduction.

The text provides a pragmatic, general discussion of how a thoroughly knowledgeable participant-observer of the Washington scene would explain to an outsider how government really works. There is very little new material here, but the book is well conceptualized. Yarmolinsky and Foster provide tantalizing snippets of issues, but without close examination or documentation. Perhaps this is necessary in a brief overview text, but the superficial treatment can be frustrating.

Changing U.S. Military Manpower Realities is principally a collection of in-house studies by scholars within or closely related to the military educational system. The first three chapters are the core of the text: the fundamental manpower availability issues and the impact upon defense planning. These issues grew out of the 1977 Conference on Changing Military Manpower Realities, which concluded that military manpower is the single most pressing defense issue in the 1980s and 1990s.

The theme of these chapters is that the All-Volunteer Military Force is not working in its present form, but the alternatives are costly. To Margiotta, this is due primarily to an identity crisis within the military and a nationwide "declining sense of the legitimacy of military service" (p. 13). This makes it difficult for Americans to accept the values, norms, and sacrifices of military service in peacetime. Similarly, "the average American finds it difficult to understand and support [arms limitation] and then commit himself or herself psychologically and emotionally to national defense" (p. 14).

The remaining chapters address a variety of manpower issues including recruitment and retention for ground forces, reserve forces, pilots, minorities, and women. The impact upon individuals and their families is examined, followed by a thoughtful concluding chapter by Morris Janowitz on the role of the citizen-soldier in a democracy.

The issues are valid, important, and generally well developed. The pessimistic conclusions of most of the book's manpower projections, however, have not proven entirely accurate because,

as Margiotta notes, "One must be cautious about predicting social trends." Precisely! The manpower projections of the late-1970s, based upon earlier standard demographic measures, did not materialize in the early-1980s because of shifting attitudes in America. Many of the dismal forecasts simply did not occur. Nevertheless, the problems will persist and cannot be ignored by the public.

The manpower studies are well documented, but principally from government or related sources. Some of these studies used invalid methods or assumptions, and so the conclusions in some cases must be questioned. The 1970 Gates Committee and the 1976 Defense Manpower Commission reports are somewhat unsophisticated and certainly outdated. The gloomy forecast for "the Orwellian year of 1984" (p. 11) seems out of place now that 1984 has come and gone. Like *Paradoxes of Power*, this text suffers from occasional technical errors and inaccuracies.

Paradoxes of Power is a thoroughly useful pragmatic overview that will survive for several years in the classroom. *Changing U.S. Military Manpower Realities* has a valid theme and useful data, but suffers in its ability to play the dangerous game of forecasting.

ABBOTT A. BRAYTON

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Federal Court Caseloads. By William P. McLaughlan. (New York: Praeger Publishers, 1984. Pp. xiv + 219. \$25.95.)

Federal Court Caseloads is a somewhat unconventional social science product. Although it might be tempting to search for causal variables that explain and predict the increasing demands that have been placed onto U.S. courts, McLaughlan's more modest purpose is to describe the growth of case filings in the federal court system. He deliberately has shied away from such a study and from statistical techniques which may or may not have borne that empirical fruit. He acknowledges that "description is not widely accepted as an analytic technique" (p. 17). Moreover, he employs exploratory data analysis (EDA), a statistical tool which might be unfamiliar to some scholars who are comfortable with traditional statistics. But EDA is most useful to detail the findings of this narrowly focused subject matter, because it "cannot be used for testing hypotheses in the traditional, confirmatory fashion" (p. 21). Scholars might at some future time employ more traditional, hypothesis-testing data analysis and answer some of the "un-

answered and interesting questions about court caseload" (p. 17) that appear in this work.

The book discusses separately the growth in case filings for each federal court level. It then draws some parallels between the district courts, U.S. Courts of Appeals, and the Supreme Court with regard to caseloads, case terminations, and pending cases. Some important descriptive data are discussed. For example, viewed from 1880 to 1980, Supreme Court caseloads have grown most rapidly only since World War II. McLauchlan refers to the mid-1940s as "perhaps the primary . . . factor in changing the caseload pattern" (p. 71). Plots of Supreme Court caseloads by decade, by chief justiceship, and by petition categories uncover no different trends.

McLauchlan shows that caseload growth has occurred in the last 15 years for the U.S. Courts of Appeals. He notes that the Fourth Circuit "has faced . . . a high proportion of civil appeals" (p. 108). This is surprising. Other circuits, the Second, Fifth, and Ninth, have major population centers that the Fourth lacks. McLauchlan displays other descriptive data for the circuits by judgeship, by three-judge panel, and by case type.

To explore the categories of cases likely to be found in district courts beyond aggregate data reported, McLauchlan shifts his concentration to an admittedly "non-random set of districts . . . [for] an intuitive examination" (p. 147). He looks at case filings in nine districts for the period from 1977 to 1981. Not surprisingly, he finds that "the causes of case filings are probably so idiosyncratic with each district that predictions or forecasts of future patterns have to be based on district-specific variables" (p. 164). Generally, after recent growth, district court case filings may be stabilizing currently.

McLauchlan notes that although "cases drop out of the system quickly and early in the process, . . . [courts] have to be ready to make that investment" (p. 199) of time and resources. He reminds us that due process of law is labor-intensive, and "even the frivolous suits must be treated seriously by the court" (p. 199). But few case filing patterns are easy to understand. For example, increases have been seen in reverse hierarchical order, "with the increase in the courts of appeals coming over ten years after the Supreme Court increase, and the district court increase appearing some five years after that" (p. 207). Elsewhere, the appeals circuits are "operating with increasing efficiency" (p. 197) in terminating cases, yet "have suffered the greatest increases in pending cases" (p. 197). In the final analysis, McLauchlan cannot suggest that "the most general kinds of influences—population, economic performance, or social settings—can be labeled as the causes of caseload" (p. 207).

The simple political solution to caseload growth calls for more judgeships at the district and appeals levels. Additionally, jurisdiction reforms such as purging the federal courts of diversity cases is often suggested. Interestingly, McLauchlan finds that for the district courts (and the appellate circuits) the number of diversity cases is dwarfed compared with federal question cases. The Supreme Court may be facing an "insurmountable" (p. 204) problem that no politically feasible reform can solve. The High Court appears to be "functioning at full capacity" (p. 196), and case filings have increased at a rate greater than the other two court levels.

Learning to read the book's unconventional EDA boxplots and stem-leaf diagrams takes some time, even for the experienced empirical student who is yet a novice to exploratory data analysis. McLauchlan is correct to warn the reader that the methods chapter describing the technique may not be enough and that further reading might be required. That caution aside, *Federal Court Caseloads* provides research hints that may sustain judicial process and court administration scholars. Future, more detailed studies will explain and predict caseload patterns both at the macro- and micro-level.

DAVID S. MANN

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The Policy Organization. By Arnold J. Meltsner and Christopher Bellavita. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 253. \$25.00, cloth; \$12.50, paper.)

This work grew out of the "Managing Information" series dealing with organization studies and decision making. It is Volume 5 of the series edited by Aaron Wildavsky, which concerns itself with information overload that managers and policymakers alike must deal with in a period of finite resources. The California State Department of Education pupil proficiency assessment program is used as a singular case study to illustrate the proposed design. Intended for managers and their policy analysts, the book is a collaborative effort of both groups to try to understand each other.

The first chapter focuses on communication and effective policy management. Chapter 2 places these factors in an organizational theoretical context, using a checklist of eight major categories that are basic fundamental administration.

The succeeding chapters are devoted to the individual accounts of the case study. The authors' approach of organizational categories is applied

as a way of clarifying policy decisions and the operations of existing organization. Each chapter studies in depth proficiency assessment, organizational goals, organizational membership, the environment, the structure or design for change, resources, tasks, decisions, and communications. Each of these categories is viewed as a design element. The authors conclude that policymakers are influenced when the research is of high quality, uses specific empirical questions of modest scientific complexity, and is provided by prestigious researchers who have credibility with decision makers.

However, it is inappropriate to use this approach in every case. For example, such a scheme should not be utilized on technical problems that have obvious technical solutions with few social or organizational implications or in a symbolic way such as initiating a study to postpone action. Although the approach of applying a list of organizational categories to illumine policy decisions and the operations of existing and proposed organizations can be useful, it is not designed to advance the science of administration. Rather it is intended to enlarge the vision and understanding of the individual manager and analyst.

Due to its anecdotal and storytelling approach, this study may be classified as a purely descriptive "how to" technique manual. It is a guide or reminder of what should be considered in dealing with organizational description. It appears to be a method to use, but guarantees no final success or possible solutions. It is how you get there that counts. The book relies on simple and basic fundamentals, using an elementary approach. The case study is interesting, but it isn't unique. Although proficiency testing almost sounds like a discipline or skill, no attempt is made to make it universally applicable. As a factual matter, the design elements surface as common sense. Witness the following statement from page 221 describing how to use the design elements:

We do not expect that many managers will want to do the work of describing their organizations themselves. Indeed, if they want an independent view of their organization, then they are better off not doing it themselves. If they did, they would see the organization through their own preconceptions and because of status differences would not likely get a straight story from members. . . . Ideally the manager should choose an outside person to undertake an organizational description. That person need not be a consultant to be considered an outsider just as long as the person is truly outside the boundaries of the policy organization.

Despite its simplicity, *The Policy Organization* has a good overview and discussion of organiza-

tions, a helpful bibliography, and would be useful to the uninitiated who wish to learn about organizational processes and description.

FRANK T. COLON

Lehigh University

Making Something of Ourselves: On Culture and Politics in the United States. By Richard M. Merelman. (Berkeley: University of California Press, 1984. Pp. xii + 274. \$15.95.)

This book asserts that the United States has "evolved . . . a loosely bounded culture" which undermines the democratic promise in America because it gives Americans "little real purchase on the massive, hierarchical political and economic structures that dominate their lives" (p. 1). This loosely bounded culture, Merelman argues, results in part from the decline of the Puritan, democratic, and class visions in America.

These cultural visions have common characteristics. First, they provide identities to their adherents and offer a means by which to understand and interpret society. Second, these visions mesh individual identity with group membership and stimulate organized group action. Third, they describe ways that groups can attain control of the structure of power in society. According to Merelman, the Puritan vision "bound together individual, church, community, state and God in a joint enterprise" (p. 6), but declined because the integral role it gave intolerance choked its supply of new adherents and because it collided with political theories that grounded individual rights and the state in non-Biblical justifications. The democratic vision, emphasizing tolerance as an imperative allowing the inclusion, "first under procedural guarantees . . . and then under substantive policy agreements" (p. 10) of diverse social groups, may not, Merelman notes, have ever been particularly powerful. He finds that it is much less compelling now than when first codified by political scientists in the 1950s and 60s. The weakness of class consciousness in America has cast doubt on the power of the class vision.

Merelman also discusses the roles of the frontier, the weakening of deference to age, the demise of the three-generation family living unit, ethnic immigration, and the automobile in sapping the power of those visions that give individuals an identity—often nonvoluntary and inescapable—as part of a group. Americans are left, then, with a culture that makes the liberated individual the basic cultural unit (p. 30) and leaves individuals to "make something of themselves" without help from cultural visions. Such individuals are freed from group identities—as

members of churches, unions, ethnic groups, and so on—that establish boundaries between persons of different groups. Individuals are consequently propelled into a world of personal relationships in which the values of “sentiment, rashness, spontaneity, equality, and transient cooperation” (p. 43) are key to getting along. Liberation from groups and their tight boundaries has, Merelman claims, a cost: It damages Americans’ ability to create cultural visions that empower them for political action in a public world dominated by “reason, prudence, calculation, inequality, and unending competition” (p. 43).

The first two chapters of this book, which are devoted to developing this view of cultural visions, are strong ones. They present an argument that will interest and challenge political scientists. The three following chapters, given to discussing how loose-boundedness is reflected and reproduced by television, advertising, and education, may be of less interest. They are laden with anthropological examples and anecdotes. Still, these chapters make their point: As refiners and purveyors of loose-bounded culture, television, advertising, and education give a deceptive view of America and encourage an “abstract individualism” that uproots the individual from the groups that shape each person’s identity.

Merelman concludes his analysis by considering whether the American polity can play a positive role as a cultural center that encourages citizen-empowering cultural visions. His findings—based on case studies of two government efforts in the field of culture creation—are not encouraging. In both cases, it is evident that American antipathy toward this role for government fosters resistance to its creations, and thus perpetuates loose-boundedness. And as loose-boundedness is perpetuated, so is the political impotence of the isolated abstract individual in America.

Although this book employs some slippery concepts and definitions, it makes an important contribution to our understanding of the cultural bases in America of political inaction and powerlessness.

W. RICHARD MERRIMAN

Berea College

Presidential Popularity and the Economy. By Kristen Renwick Monroe. (New York: Praeger Publishers, 1984. Pp. xxiii + 289. \$23.95.)

Many political analysts in 1980 felt it was the public’s reaction to Ronald Reagan’s rhetorical question about the economy—“Are you better off now than you were four years ago?”—that cinched Reagan’s electoral victory over incumbent

president Jimmy Carter. In other words, of all Carter’s political weaknesses, it was the personal effects that his economic mismanagement had on individual Americans that finally moved a largely uninspired and undecided electorate to put the nation on a new, rightward course. If I understand Kristen Renwick Monroe’s arguments correctly, however, it would be a mistake to hold too strictly to this interpretation of the 1980 election. A relationship between an incumbent president’s popularity and the state of the economy does exist, but within a complex matrix of qualifiers raising important questions about the depth of rational, pocketbook calculations by voters that Reagan’s questions to the public presupposed.

I must state my caveat about understanding Monroe’s arguments correctly because I found, as I believe many readers will find, those arguments presented with a certain lack of focus and pointedness. *Presidential Popularity and the Economy* as a title refers to much less than what the book attempts to deal with, and it is not quite the subject toward which readers will find themselves oriented at the end. Rather, the full range of Monroe’s concerns includes: the theoretical tension between economic models of human behavior (emphasizing rational intentions) and sociological models (emphasizing prerational dispositions); the accuracy of cyclical models of presidential popularity versus models stressing popular reactions to specific acts and events; the validity of the concept of sociotropic (or collectively oriented) voting; and the influence on presidential popularity of such key variables as partisan ties and social class. Trying to balance the findings she derives from pursuing all these concerns leads Monroe to the almost inevitable conclusion that the determinants of presidential popularity can best be understood through an “integrated rational model [that] incorporates critical nonrational factors in an analytical framework essentially rational in orientation” (pp. 87-88). Her closing words deal with her conviction that the development of this new analytical model to overcome the tension between the economic and sociological characterizations of behavior “will be the next theoretical breakthrough in political science” (p. 204). Ultimately, the topic suggested by the book’s title is overshadowed by reflections and recommendations dealing more with the theory and methodology of behavioral analysis in general.

Monroe’s suggestion that a necessary breakthrough synthesizing conflicting behavioral models has yet to occur may be partial explanation for the problems left behind by what she does say specifically about presidential popularity and the economy. She asserts that economic conditions and expectations play a primary role in shap-

ing the public's evaluation of presidents—the pocketbook voter has validity as a conceptual norm—but important constraints on this role must be recognized. Indeed, these constraints are so important that one may worry about the exceptions outweighing the rule. Monroe's central example is the "partisan filter" through which voters pass their expectations and perceptions, with some remarkable effects on their rationality. Monroe's data suggest, for instance, that the popularity of Democratic presidents increases as unemployment *increases*. Her too brief speculation is that this occurs because people view the Democratic party as most likely to solve the unemployment problem, even while it persists during a Democratic administration (pp. 175-176). Thus, to return to the example of the 1980 election, the implication is that Jimmy Carter might have had a better chance if a few more unemployed voters had been available to hear Reagan's query about personal well-being.

Explaining this kind of anomaly and other questions raised by the excellent opinion data assembled by Monroe (including previously unpublished Gallup poll data) is what should have been the more intentional thrust of this book, with less discussion of competing behavioral theories proposed by other scholars and the attendant methodological debates. If such explanation had been held at the center of Monroe's focus, she might herself have broached the theoretical breakthrough that she instead leaves us to ponder.

DAVID MENNINGER

University of California, Los Angeles

Making an Issue of Child Abuse: Political Agenda Setting for Social Problems. By Barbara J. Nelson. (Chicago: University of Chicago Press, 1984. Pp. xiv + 169. \$17.50.)

During the 1970s, policy-oriented political scientists focused primarily upon the implementation stage of the policy process. They attributed policy successes and, more likely, policy failures to implementation. Recently, political scientists, including Barbara J. Nelson, reexamined the agenda-setting process. Nelson's *Making an Issue of Child Abuse* provides a welcome addition to this literature.

Even a cursory reading of newspapers or watching of television stimulates one to question why child abuse is now considered a public problem. It is clear that child abuse and neglect occurred both in previous times and in other nations. Nelson asserts that "a social problem is a social construct" (p. 5). Thus, a social problem is not solely an empirical reality but is defined by organized in-

dividuals who wish to solve the problem and a society that recognizes it.

The natural rights ideology, civic education, and the rise in the bourgeois family created "protected childhood" beliefs in nineteenth-century America. These beliefs, the rise of protective societies, and media attention to cases of child maltreatment stimulated, at first, child rescue policies and, later, family rehabilitation policies. In the 1960s, society rediscovered child abuse due to equity beliefs, the research of radiologists and, later, pediatricians, and the media coverage of that research.

This book centers on the agenda-setting actors and processes from the 1960s to the 1980s. The Children's Bureau established child abuse on its agenda at the urging of physicians and the American Humane Association. This fortuitously occurred simultaneously with an uncommitted and large increase in the Bureau's research budget. With the new money, the Bureau established "a network of researchers whose careers were enhanced by child abuse research . . . [keeping] child abuse on its agenda and the public's agenda, in an ever-widening circle" (p. 47).

The media followed the research results, creating more news stories. In addition to this, the linkage with broader social issues, more issue differentiation, and soft news stories supported a longer than usual issue attention cycle for child abuse. The media attention, interest group activity, and the model law drafted by the Children's Bureau caused every state to pass child abuse reporting laws between 1963 and 1967. This rapid diffusion was due to "the symbolic and valence qualities of the issue, and the absence of monetary or political costs. . ." (p. 80).

On the national level, Senator Walter Mondale introduced the first adopted legislation on child abuse. Earlier, the Comprehensive Child Development Act, supported by Mondale, was defeated in Congress. But Mondale discovered the key to legislative success: "Not even Richard Nixon is in favor of child abuse" (p. 102). A symbolic issue supported by universal values, high public salience, low conflict, and a willingness to ride herd, produced the success that Mondale needed to both pass the legislation and appear candidate-like in his unannounced campaign for the presidency. By 1981, however, the perceived threat to family autonomy and Reagan's desire for block grants nearly killed the program.

In a moment of broad theoretical thrust, Nelson suggests that child abuse went from a valence to a controversial issue because the policy had the tensions of liberal reform. In order to obtain serious consideration, a problem must be defined as deviant, individual, and psychological with, hopefully, a medical solution. "The

scenario of having done too much and too little is one consequence of liberal reform" (p. 137).

This book has significance beyond the issue of child abuse. Nelson considers the issue cycles, the role of the media, the type of issue, the structural and individual factors in agenda-setting. This makes a neat case study with much theoretical import.

DAVID C. COLBY

University of Maryland Baltimore County

The American Presidency. By Benjamin I. Page and Mark P. Petracca. (New York: McGraw-Hill, 1983. Pp. xiii + 418. \$13.95, paper.)

Benjamin Page and Mark Petracca set out to do two things in *The American Presidency*. The first is to write a comprehensive text; the second is to assert several arguments of their own about the power of the presidency.

The authors have clearly succeeded in their first objective. Their diligence has resulted in a thorough text that covers the major topics considered in comparable works, plus a few that are often neglected. For example, they discuss the nature and features of political power early in the book and put the main themes of this discussion to good use thereafter. They also provide an organizing and explanatory device in a systems model of presidential power.

Their model puts the president amid interacting governmental institutions that respond to international, social, economic, technological, and demographic needs as interpreted and mediated by public and group activities. In responding, the president, Congress, and, to a lesser extent, the bureaucracy and courts serve to affect policy outcomes. The elements of their systems model supply and justify the book's major topics and thereby insure its comprehensiveness. Interest groups and courts, for example, are given the attention they deserve, whereas these are often slighted in other works. Also included is a chapter on the "Evolving Presidency," which discusses every president rather than just the "strong" or recent ones. One area that is somewhat underdeveloped involves the different models of information gathering and decision making, especially as these relate to presidential isolation, responsiveness, and control.

The second purpose of the book—that of pro-

viding new information and a set of perspectives on presidential power—is what distinguishes this work from most texts and makes it interesting to scholars. The authors argue that there is substantial truth to the view that the presidency is the most powerful office in the world as judged by its proximate influence on policy. They note, with many others, that such power has generally increased, albeit erratically over time, and that it varies across issue and policy domains. Although there is no news in that, Page and Petracca add to what is usually reported by supplying a rough hierarchy of presidential power in several domestic policy areas (e.g., social welfare, civil rights, macroeconomics, and public works).

There is, however, an all-important qualification to the authors' assertion of presidential strength. Their systems model serves to illustrate their premise that environmental factors, not presidents, are the ultimate cause of policy outcomes. Despite the president's proximate discretion, the presidency is essentially a conduit of societal needs made compelling primarily by the actions of the public and of groups.

Because of this view, Page and Petracca reject the "great man" perspective of Wilson, Rossiter, Pious, and others and generally deemphasize personality, whim, and individualism. Similarly, they reject the "bureaucratic politics" model that pictures the presidency as the central policy initiator and broker among competing institutions (à la Neustadt, Allison, and Halperin). Democratic and pluralistic pressures generally determine presidential initiatives and basic policy outcomes. Thus, they conclude that the presidency is quite powerful as regards specific policies, but quite weak as a determinate of continuity and change in basic governmental directions.

There is much to be said for this book aside from its substantive thoroughness and conceptual clarity. While essentially a text, it does take sides in scholarly debates. It also supplies some original research, as in its discussion of the congruence between policy and public opinion. Although systems analysis is sometimes considered trite these days, it can be used effectively as a heuristic device. In this book it helps clarify major themes and contributes to a generally logical, well-justified, and smooth presentation. The book is written in lucid, standard English with little reliance on jargon despite the systems approach and an infrequent reference to the president as an "intervening variable."

Page and Petracca have added new details to the outlines of what we already know and have argued convincingly in support for their perspectives. Thus, they have surpassed their goal of providing a comprehensive text. They have also taken large strides toward their second goal of providing

scholarly perspectives and new information on the American presidency.

JAMES A. DAVIS

Oklahoma State University

America's Unwritten Constitution: Science, Religion, and Political Responsibility. By Don K. Price. (Baton Rouge: Louisiana State University Press, 1983. Pp. xvi + 202. \$19.95.)

According to Price, the inability of accountable leaders to develop coherent policies and programs is at the heart of a continuing problem created by ingrained national prejudices found in America's unwritten constitution. He traces the origin of these attitudes to John Calvin and the Puritan founders. There is no system of political authority in existence with the capacity to effect a rational comprehensive policy or to carry out a coherent program. Power is dispersed, no single point of responsible control over the formulation of general public policy is to be discovered, and no long-range plans or policy priorities are set and followed. Constitutional reforms that imitate the parliamentary cabinet system are required to remedy this problem. Price provides the layman with a clear, concise summary of the status quo in administrative matters. These current difficulties, according to Price, are compounded by the continual conflict between Congress and the president over control of the administrative branch. Congressional subcommittees assisted by professional staffs control specialized program administration. Bureau chiefs acting in collusion with congressional subcommittees undermine the authority of the president and departmental secretaries. Further dispersal is caused by the power of special interest lobbyists, which accelerates the disintegration of party discipline.

The central message in this book is that the president should be delegated greater power to manage the formulation of general policy and to control the administrative branch. Price asserts that the administrative branch remains under the control of congressional committees. The president and departmental secretaries should have authority to provide long-range planning and to set policy priorities assisted by a strong career staff of responsible civil servants. Despite manifold efforts to reform the administrative branch in recent years, the attentive reader may infer that the situation first described by Woodrow Wilson remains resistant to substantial change.

Price suggests some specific institutional im-

provements. He thinks the executive office of the president should be smaller. The multiple complex congressional controls over the executive need to be reduced. The size and influence of congressional and executive office staffs should be diminished, and greater power to formulate general policies delegated to the president and departmental secretaries. However, no reasons are given why Price's suggested changes are any more likely to be adopted than the myriad similar reforms suggested by countless other academic experts. Yet the personal references and reminiscences included in the footnotes add a humane touch to this abstract discussion and present Price's interesting extensive experiences with influential men in high governmental positions.

The Watergate era reforms designed to reduce the power of the "imperial presidency" are replaced here with demands to increase the president's power to formulate public policy and to control the administration. Price returns to the old idea that it is the weakness, not the power, of the president that lies at the center of contemporary problems with the American system of government. Therefore, he claims that we need to restore a greater measure of confidentiality in the preparation of administrative policy and staff papers. The increased role of civil servants in formulating policy needs to be protected from excessive pressures for action by the media. The American and congressional prejudice against delegating power and responsibility for the formulation of policy to the president and civil servants needs to be eliminated.

Price's discussion of "Saints and Scientists" confuses seventeenth-century American puritanism and Calvinism with nineteenth-century southern fundamentalism. The venture into the dense woods of religion and philosophy by this noted student of administration neither illuminates nor enlivens his book. Little evidence is presented to sustain his imaginative hypothesis relating the ideas of dissenting religious groups and modern scientists. The presence of Joseph Priestly and Richard Price in America does not forge any necessary connection between dissenting religious groups and modern scientists. The ideas of John Stuart Mill as expressed in *On Liberty* are closer to contemporary intellectuals' demands for freedom than are the views of dissenting Protestant theologians. No doubt there is a serious theoretical issue involved in the modern scientist's assertion of a right to freedom from governmental control which deserves a more extensive consideration than it receives here. If the huge sums necessary to sustain research are being provided by public taxes, then the question of the scientist's right to private political views and the right of private universities to be free from governmental

control and accountability may be resolved by the old maxim: He who pays the piper calls the tune.

M. SUSAN POWER

Arkansas State University

Constitutional Law of the Federal System. By C. Herman Pritchett. (Englewood Cliffs, N.J.: Prentice-Hall, 1984. Pp. xiii + 382. \$17.95.)

Constitutional Civil Liberties. By C. Herman Pritchett. (Englewood Cliffs, N.J.: Prentice-Hall, 1984. Pp. x + 406. \$17.95.)

Pritchett is the dean of public law specialists in political science. The books under review are the product of his life's work. Together they are the most informed, insightful, and lucid presentation of American constitutional law I have read.

The subject of each book is the subject of a traditional political science course in constitutional law. *Constitutional Law of the Federal System* begins with a short history of the events leading to the Constitutional Convention and with a brief analysis of the way in which the Constitution allocates political power. In addition to chapters on judicial review, intergovernmental relations, and the organization and jurisdiction of the federal judiciary, Pritchett includes a chapter on the major theories of constitutional interpretation. This is an important discussion, often omitted from constitutional law casebooks. The U.S. Constitution has been amended only 26 times, in part because it is always being reinterpreted by the Supreme Court. An understanding of these theories of interpretation including the intention of the Framers, the original meaning of constitutional language, logical analysis, "experientialism," and "interpretivism" is necessary for an understanding of the history and development of American constitutional law.

Constitutional Civil Liberties, the companion volume to *Constitutional Law of the Federal System*, follows the syllabus for most political science courses in civil rights and liberties. Pritchett examines the proposal and adoption of both the Bill of Rights and the Fourteenth Amendment before turning to the way in which the Court has interpreted these provisions throughout its history. His incisive discussion of the most important First Amendment decisions gives even the casual reader a clear exposition of the Court's treatment of the freedoms of speech, press, association, and religion.

With respect to the application of the due process clauses of the Fifth and Fourteenth Amendments to state and federal criminal prosecutions, Pritchett properly devotes more space to the Fourth Amendment's protection against unrea-

sonable searches and seizures than to Fifth and Sixth Amendment rights. In recent years the Court has been inundated by search and seizure cases, but it has not developed a consistent and predictable standard of review for these cases.

Pritchett devotes two chapters of *Constitutional Civil Liberties* to the equal protection clause of the Fourteenth Amendment. The first of the two chapters concerns racial discrimination; the subject of the second is what Pritchett calls "the new due process—equal protection." This second chapter focuses on how the Court reviews various laws which are challenged as denials of equal protection. These laws classify citizens on the basis of sex, wealth, legitimacy, mental or physical handicaps, and age. This is one of the most interesting areas of constitutional law and one on which Pritchett sheds considerable light.

Economic classifications challenged as violations of equal protection led the Court to define its most permissive equal protection test—the rational basis test. However, when laws classify citizens with respect to such fundamental rights as the right to travel, to receive public education, and to marry or to use contraceptive devices, the Court has employed more restrictive tests.

The most widely used of these tests has been the suspect classification test. Race was the original suspect classification. Whether wealth, sex, alienage, illegitimacy, age, and physical and mental handicaps are also inherently suspect classifications requiring the closest judicial scrutiny is the subject of many of the most important decisions made by the Burger Court. The consensus of the Court seems to be that race is in a class by itself. Classifying citizens according to their race and treating them differently because of their race is almost always a denial of equal protection. How to review laws classifying citizens on the basis of factors such as sex and age is a problem for which the Burger Court has not yet found a cogent solution.

The last chapter of *Constitutional Civil Liberties* deals with elections and the right to vote. Pritchett discusses both constitutional and statutory limitations on state regulation of the franchise. He tells the story of how federal courts have gradually eliminated state restrictions on the right to vote and at the same time have replaced state officials as *de facto* election supervisors.

Pritchett's work in these books demonstrates the truth of the axiom that the only way to understand the Supreme Court is to read and comprehend its opinions. This does not mean that he rejects quantitative methods. In fact, Pritchett pioneered their application to Supreme Court decision making in the 1940s. He has the expertise to evaluate the extent to which public law policy-making can be quantified. His work is required

reading for any political scientist whose constitutional law research is more than an exercise in refining empirical methods.

STEPHEN A. GRAHAM

Indiana Central University

Third Parties in America: Citizen Response to Major Party Failure. By Steven J. Rosenstone, Roy L. Behr, and Edward H. Lazarus. (Princeton, N.J.: Princeton University Press, 1984. Pp. viii + 266. \$25.00, cloth; \$8.95, paper.)

In most courses or texts dealing with U.S. political parties and elections, the subject of third parties is given only cursory treatment. *Third Parties in America* serves to remind everyone of the crucial impact third parties have on the development and performance of the party system. As the authors note in their introduction, "the power of third parties lies in their capacity to affect the content and range of political discourse, and ultimately public policy, by raising issues and options that the two major parties have ignored" (p. 8).

The first half of the book is a discussion of the considerable obstacles that discourage third-party activity. In addition, there is an excellent review of the history of third-party movements during the nineteenth and twentieth centuries. The authors conclude that the "important challengers of the nineteenth century came from relatively stable minor *parties*, whereas those of the twentieth century came from independent candidates" (p. 119). During this century, once the candidates stop running the parties rapidly deteriorate. In this respect, modern third parties seem to be reflecting the more candidate-centered politics that is so prevalent today.

The second half of the book is devoted to a more ambitious task. Using time-series data from the 1840 to 1980 presidential elections and survey data from the 1952 to 1980 elections, Rosenstone, Behr, and Lazarus construct models to explain why both voters and candidates are willing to sustain the considerable costs involved in such activity. They argue that significant third-party activity results from the failure of the two major parties. When the Republicans and Democrats "fail to provide prosperity, responsive policies, and competent, trustworthy leadership" (p. 150), voters are much more inclined to support third parties. Furthermore, if such parties offer "nationally prestigious" candidates, third-party voting is boosted substantially. The authors also find a positive relationship between the age of the party system and third-party voting. As the party system ages, partisan attachments weaken in part because older, more loyal voters are replaced by

newer, more independent ones. Not as firmly anchored by partisan attachments, these new voters are more susceptible to the message of a third party.

The explanation of why candidates are willing to bolt the major parties is somewhat different. As one might expect, third-party efforts are much more likely when either of the major parties have been racked by serious factionalism. Virtually all nationally prestigious third-party candidates first have been rebuffed by their own parties. Because they cannot pursue their goals within the major parties, they make use of third parties. Also, third-party candidates are often running against incumbent presidents with whom they have serious disagreements. These candidates are not so much running to win, but to deny victory to the incumbent (and thereby demonstrate the importance of the issues and constituencies they represent). Another important factor in a candidate's decision is his perception that the major parties are weak and thus vulnerable. Once again, the age of the party system is an important factor in this calculation. For example, the authors argue that the increase in third-party voting during the past 20 years is attributable to increased intraparty factionalism, the failure of the "major parties to realign around more salient issues," and the increasing numbers of post-New Deal voters who have negative evaluations of the party system (pp. 224-225).

This is an excellent book that will almost certainly become the standard in the field. The writing is crisp, and the theories on third-party voting are carefully spelled out and impressively supported. In short, the volume makes a significant contribution to our understanding of the party system in America. Finally, the book is important because it persuasively argues that third-party efforts make an important and positive contribution to American democracy.

JOHN W. EPPERSON

Simpson College

Congress and the Presidency in U.S. Foreign Policymaking: A Study of Interaction and Influence, 1945-1982. By John Rourke. (Boulder, Colo.: Westview Press, 1984. Pp. vii + 441. \$30.00, paper.)

Despite its title, this book focuses more of its analysis on the role of the Congress than of the president in foreign policymaking. Its basic message is that too often Congress has been overlooked as a source of American foreign policy in the postwar years. Although there was increased

congressional activism in the 1970s, the resulting policy influence was not markedly different than that exercised by the Congress in the years immediately after World War II. Furthermore, between these two periods, the Congress played a continuous, if less noticeable, role in foreign policy. At present, Congress seems to be returning to its more normal secondary role in the foreign policy process.

The book is divided into three parts and nine chapters, roughly organized chronologically from the Truman years to the present. The first three chapters focus on the early Truman years and the crucial role of Congress (especially that played by Senator Arthur Vandenberg of Michigan) in the shaping of postwar and cold war policies. The second part begins with Truman's second term and runs through the Nixon administration, but this part also includes three chapters that examine the Congress more directly. The first of these analyzes Congress' influence on policy toward various regions of the world (e.g., Europe, Asia, Middle East); the second on specific issues (e.g., trade policy, foreign assistance, the selection of executive personnel); and the third on "congressional values" in international affairs, treating such items as congressional deference, the role of partisanship, the impact of ethnic groups, and the influence of public opinion, among several others. The last part of the volume, consisting of two chapters, discusses congressional activism in the 1970s and early 1980s and assesses the role of the Congress versus the president in the future.

The technique of analysis in the book is generally to identify various foreign policy decisions and to delve into the interaction process between the executive and legislative branches. Extensive use is made of the manuscript and oral history collections from the various presidential libraries and the private papers of former congressional and executive officials, among others. In addition, of course, much secondary source material is employed: The footnotes are especially extensive and useful, running to 98 pages in all, whereas the bibliography consists of another 21 pages. The result is a volume rich in quotations—some familiar, some less so—from important political actors, and one that organizes in a generally understandable way both familiar and new information on the role of the Congress in foreign policymaking. The most comprehensive analysis, however, is really confined to the early postwar years. As the book gets closer to the present, the analysis becomes more selective. Moreover, even though the title indicates coverage through 1982, there is relatively little on the Reagan administration.

Despite the wealth of material in the volume, the book does have several shortcomings that weaken its overall utility. First, the book attempts

to use the conceptualizations of Abraham Kaplan and Harold D. Lasswell on the notions of "values" and "means" of influence to interpret the process between the Congress and the president. Unfortunately, this approach does not really work very well; instead, it appears to be simply tacked on, rather than integrated into the work. The book would have been better served by relying on a chronological development only. Second, although one comes away from the volume convinced that Congress has not been given enough credit for shaping American foreign policy, the extent of this influence is still not wholly clear. Rarely does Rourke demonstrate that the president did not get the basic elements of his foreign policy, and the author ultimately does endorse Aaron Wildavsky's "two presidencies" argument near the end of the volume. Perhaps a more systematic approach to evaluating the Congress might have provided a firmer answer. Third, the volume needs a careful proofing. It is marred by several key misspellings and factual inaccuracies. Moreover, at one place (p. 236) almost an entire paragraph is repeated. Although these problems are not an insurmountable encumbrance to appreciating the entire volume, too often they distract the reader's attention.

On balance, though, the book provides some good information on the Congress in foreign policymaking, and it could be a useful reference for those interested in congressional-executive relations.

JAMES M. MCCORMICK

Iowa State University

Making Economic Policy in Congress. Edited by Allen Schick. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1983. Pp. xii + 282. \$19.95, cloth; \$10.95, paper.)

Political scientists traditionally tended to study in bits and pieces how Congress made economic policy. An earlier generation of scholars sought to explain behavior in the institutional centers for creation of particular sorts of economic policies. Richard Fenno's *The Power of the Purse* (Little, Brown, 1966) and John Manley's *The Politics of Finance* (Little, Brown, 1970) are examples of this approach. The budget process served as the topical vehicle for more comprehensive assessments of economic policymaking; Aaron Wildavsky's *The Politics of the Budgetary Process* (Little, Brown, 1984) remains unsurpassed among studies of this scale.

The last decade has witnessed far-reaching changes in both economic policy substance and

process in Congress, presenting political scientists with much theoretical and empirical catching-up to do. Fortunately, books rich in empirical description have addressed these changes, such as Larce T. LeLong's *The Fiscal Congress* (Greenwood Press, 1980) and Allen Schick's exhaustive and indispensable *Congress and Money* (Urban Institute, 1980). Schick has now edited a volume of readings that complements our theoretical and empirical understanding of this subject.

All students of Congress should also be students of its economic policies, given the Constitutional and institutional importance of these responsibilities and their impact upon national life. A principal analytical task here entails explaining interrelationships between congressional behavior and a wide range of economic situations such as fiscal policy, income distribution, business regulation, and international trade. Many of the selections in this volume successfully embark on this task.

Catherine Rudder demonstrates that the increasing irrationality of tax policy is related to changes in the personnel and processes of the Ways and Means Committee. John Ferejohn reveals how congressional structure accounts for the diversity and limited effect of income transfer programs. The deleterious policy effects of frequent congressional intervention into administrative regulatory processes are explained by Mark Nadel. Robert Pastor shows how Congress has resisted protectionist legislation over the past 50 years far more frequently than is commonly thought.

The book's most important conceptual contributions are found in an essay by John Ellwood on the budget process and in Schick's concluding essay. Drawing upon earlier work by Theodore Lowi, both authors demonstrate that changing member perceptions of the substance of specific tax and spending decisions have brought about marked changes in congressional fiscal policy behavior. As Schick puts it:

The ability of Congress to define redistributive policies in distributive terms is an important determinant of its capacity to legislate. . . . But Congress has found it increasingly difficult to behave in this manner; hence its legislative output has declined. (p. 260)

Ellwood notes that this resulted because of the new congressional budget procedures and sluggish macroeconomy of the last decade. The legislative success of Reaganomics in 1981, he argues, occurred because it was based upon economic theories that promised an escape for Congress from the contentious redistributive politics of the late seventies. That promise has yet to be realized.

A major lesson from the studies in this volume is that scholars can benefit greatly by studying lawmaking in Congress within the substantive policy context of its operation. A complex and important set of interrelationships between economic conditions and congressional processes is evident here, indicating how the recent growth of interest in political economy in our discipline promises to enrich our understanding of institutional behavior.

This book is essential reading for students of Congress. Throughout, the authors examine not just congressional politics and processes, but also the important interrelations between the perceptions of policy substance and behavior of members. The selections are carefully written and in aggregate contribute a more comprehensive portrait of institutional operations in the context of the economy. Let us hope such good work continues.

STEVEN E. SCHIER

Carleton College

Constitutional Faiths: Felix Frankfurter, Hugo Black, and the Process of Judicial Decision Making. By Mark Silverstein. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 234. \$24.95.)

Biography maintains links between belief, action, and social life that are sometimes ignored in more scientific forms of inquiry. The tradition of judicial biography running from Carl Brent Swisher through Alpheus Mason to J. Woodford Howard has placed the lives of justices like Field, Brandeis, Stone, and Murphy in the context of the institutions and ideology of the law. Recently, there has been a resurgence in biographical work. The new studies are characterized by methodological diversity while (perhaps fortunately) they focus on some of the same individuals. Silverstein's study of Frankfurter and Black looks at justices who have been popular with contemporary biographers. Like the other modern efforts, this is more than a simple life story, and it ranks with the best of the new work.

Although we may not need this book to learn about these two justices, the study is built on good biographical material and the writing is a pleasure. In the tradition of the biographer, it contains original research. New sources chart the intellectual cross currents bearing on how these men viewed judging. For Frankfurter, correspondence with Secretary Stimson (p. 69) shows his early deference to the legislative process. In Black's case, there are notes in the margins of

draft opinions to document the move toward absolutism on First Amendment issues (p. 182). In addition, there is effective and insightful synthesis of secondary sources and public records.

However, the reason for going to the book, and the most exciting basis on which to evaluate it, is Silverstein's promise to build on the political jurisprudence of the last 40 years by examining "how and at what stage" (p. 17) the values and attitudes of a justice operate in the decision-making process. The issue is an important one, and Silverstein has given us some valuable source material. His answer has two facets that are worthy of attention.

One is the suggestion that beyond the familiar notion of "role" is one of "institutional norm" (p. 19) or "process" (p. 211). In the behavioral context, the role variable was treated as an internalized standard of behavior. Although as such a standard role was a necessary corrective to the excesses of behavioralism, it still reflected a limited understanding. Now we are beginning to see, in work like Silverstein's, that norms about judging depend on belief. In the context of an institution, norms are real and consequently more significant for knowing an institution like the Supreme Court than the concept of role had been. Silverstein has deepened our understanding of how confrontations like those between Black and Frankfurter are mediated by institutions.

The second dimension of the book is an explicit interest in the influence of ideas on judicial thought and action. This is the special contribution of the political scientist. Silverstein handles historical theory well, and he has equal facility with contemporary accounts of politics. It is from this background that he interprets the impact of Senator Black's extensive reading of the classics. "From this early reading came the first evidence of the conflict theory that was to dominate his view of the polity" (p. 104). Here, the technical language of social science is used to describe ideas that become the basis for judicial action. Silverstein gives us a glimpse of ideas constituting how Black and Frankfurter saw themselves and their place in the institution. The ideas about democracy and how judges should behave are central to the story of these two very different liberals.

Throughout his book, Silverstein has avoided a focus on outputs or votes in favor of attention to what he calls the "work product" (p. 22). This is an outstanding aspect of the study, and it is essential for understanding the link between process and ideology. Bench memos, briefs, and other internal documents effectively portray the politics of law as battles over interpretations. As a perceptive biography, *Constitutional Faiths* links the interpretations to long-standing beliefs about the world.

Mark Silverstein has captured the strains of constitutional jurisprudence that raise its practitioners above the free play of interests without losing sight of the inevitably political character of this activity.

JOHN BRIGHAM

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Committees in Congress. By Steven S. Smith and Christopher J. Deering. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xiv + 291. \$10.25, paper.)

Smith and Deering boldly assume a substantial task: to describe and explain the dynamics of congressional committee politics, especially during the past 15 years. Not only has this been an era of change in congressional behavior, but also during the same period political scientists have produced an intimidatingly large literature relevant to congressional committee behavior.

Smith and Deering's model for change in individual committees is simple, but it provides an adequate and comprehensive framework for their analysis. In addition to a data base of interviews with 187 committee members and staff from 1981 to 1983, they have ably drawn upon the rich literature of the discipline and have innovatively gathered interesting data from primary sources, for example, the percentage of bills managed on the floor by chairs of full committees. The data not only provide comparisons across the committees, but also selectively show comparisons for certain years over about a 20-year period. Although the overall pattern is one of decline, trends for committees are far from uniform. Indeed, the pattern of data obtained for the Senate in the 86th Congress (1959 to 1961) would probably surprise most political scientists.

The authors' historical review, in less than 20 pages, of committee institutionalization from the early nineteenth century to the 1960s is an admirable accomplishment. They provide a good review of the committee reforms during the 1970s in another 20. This section crisply sums up the effects of the 1970s' Legislative Reorganization Act, the Budget and Impoundment Act, and the accomplishments associated with the Hanson, Bolling, and Stevenson committees.

However, once Smith and Deering get into the object of their inquiry—to cogently differentiate the patterns of behavior in matters such as committee agendas, the constraints upon the various committees from environmental pressures, member goals in committee activities, and contexts of committee and subcommittee leadership—the immensity of their task becomes evident. To deal

with the House they must comprehend 19 to 22 committees over the period, with as many as 140 subcommittees (94th Congress); at the same time, the Senate had 15 to 18 committees with a peak of 127 subcommittees (93rd Congress). Smith and Deering have difficulty articulating generalizations because there are so many interesting variations among their specific findings. For example, in a discussion of the "interbranch environments" of the committees, four types are distinguished. But even within the types there are obvious differences among similar committees, so a disproportionate share of this discussion describes differences between the House and Senate appropriations committees, both of which are included in a single type.

The authors' proclivity to report details will make the book difficult for students. There are some other deficiencies on that score as well. In an important discussion about jurisdictional fragmentation, the data for a helpful analysis are recorded in the appendixes of the book. However, why the numbers indicate fragmentation is not well explained. Fragmentation is later related to committee staff size, and there (Table 7-1) a footnote suggests that one of the authors' tables concerning fragmentation was deleted in publication.

Smith and Deering are generous in providing data tables, but in certain instances the data bear more discussion than the book provides. For example, they report the number of witnesses appearing before the three types of committees (executive, congressional, and public) for the 91st and 96th Congress. Little inference is made about the substantial increase in the appearances by public witnesses, and whether or to what extent the multiplication of subcommittees has enhanced the access of the public to Congress.

Perhaps the most disappointing aspect of the book is its abrupt ending. After 270 pages of discussion and analysis, Smith and Deering conclude with the timid assessment that "We have found that committees still matter . . . [and] their role in the legislative process is assured." The authors' analysis provides them with data about the persistence of an institutional form, but they choose not to offer substantive judgments about its effectiveness.

JACK R. VAN DER SLIK

Sangamon State University

Louis D. Brandeis: Justice for the People. By Philippa Strum. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xv + 508. \$27.50.)

Philippa Strum's biography of Louis D. Brandeis is the latest in a series of attempts by legal

scholars to chronicle the depth and breadth of Brandeis's remarkable life and career. The task is a formidable one; even the most prominent in public life today pale in comparison to Brandeis. A leader in the Progressive movement, Brandeis was a tireless advocate in support of almost every reform movement of his age including women's suffrage, trade unionism, trust busting, and hours and wages legislation. At the turn of the century, he was one of America's most successful attorneys, earning more than \$2 million before his appointment to the Court in 1916. At the same time, he was known as the "people's attorney," often eschewing fees when advocating causes in the public interest. He was a trusted advisor to Wilson's New Freedom and Roosevelt's New Deal. Although never a religious Jew, he was, nonetheless, the spirit and unifying force behind the Zionist movement in America. During the more than 20 years in which he served on the Supreme Court, he earned a reputation as one of America's greatest jurists. Small wonder that among his friends and admirers he was known as "Isaiah."

Through a detailed study of Brandeis's pre-Court career, Strum attempts to highlight the development of the values and ideas that shaped his life. She succeeds in this task admirably. Brandeis's efforts to reform the life insurance business, to achieve efficiency through scientific management and worker participation, to enforce antitrust laws, to mold the development of the law through an appreciation of the facts of modern life are chronicled in extraordinary detail. In a fascinating section, Strum connects Brandeis's study of ancient Greece with his zeal for Zionism; in Palestine, Brandeis sought to recreate a modern version of the Greek *polis*. This is biography at its best, and our understanding of Brandeis as well as the Progressive era is increased immeasurably.

Sadly, the same cannot be said for the chapters on Brandeis's Court career. One is left with the distinct impression that Strum's admiration for Brandeis precluded a realistic discussion of the contradictions within his jurisprudence. Brandeis never reconciled the detachment demanded by judicial restraint with his zeal for reform and social change. Hence, the typical opinion of Brandeis the jurist was not unlike the famous "Brandeis brief" of Brandeis the advocate; in both instances the emphasis was on an empirical analysis of the merits of challenged legislation. His colleague and friend Justice Holmes was content to conclude that the legislative choice was not precluded by the Constitution. Brandeis assumed a far more activist position in an effort to show the legislation affirmatively sanctioned by the Constitution and necessary in the quest for social justice. In the hands of a judge like Brandeis, this

method inevitably resulted in a progressive decision. But the method is not judicial restraint, and Strum's efforts to portray Brandeis as the apostle of restraint obscure what might have been an important analysis of how ideas and values shape concepts of judicial role.

This objection notwithstanding, *Louis Brandeis: Justice for the People* is well worth reading. Its portrait of the early Brandeis is rich in detail and analysis. Strum has succeeded in tracing the development of many of the values and attitudes that shaped Brandeis's career; in light of his enduring impact on American life and law, that is justification enough for yet another book on Justice Brandeis.

MARK SILVERSTEIN

Boston University

A Review of Reform and Punishment: Essays on Criminal Sentencing. Edited by Michael Tonry and Franklin E. Zimring. (Chicago: University of Chicago Press, 1983. Pp. viii + 210. \$25.00.)

Although it is not easy to determine what part of the criminal justice system is most likely to be in need of reform, criminal sentencing is currently viewed as a part of the system that is in dire need of change. Several factors may explain this observation. Criminal sentencing is complex, controversial, and does not appear to be attaining its goals. It is also a subject of contemporary concern not only in the United States, but also abroad. The editors of this interesting book recognize this important concern and provide an international perspective on the issue.

The editors, relying on a comparative analytical approach, intend this work to be a basic reference for students of issues in criminal sentencing and are successful in that purpose. A sense of urgency for change in the criminal justice system—a radical rethinking of criminal punishment—pervades the book. This is understandable in view of the existence of historical and contemporary inequities in criminal sentencing procedures found in the United States. The authors of the book's essays may differ somewhat in their professional background and style of writing, but they all share a common concern about the controversial phenomenon of criminal sentencing that is an integral part of the criminal justice system.

A Review of Reform and Punishment: Essays on Criminal Sentencing is divided into three main parts. Part 1 focuses on "Lessons from Abroad" and represents a commentary on the Scandinavian and West German sentencing systems compared to that found in the United States. For example, in

the Scandinavian countries, disparities in sentencing seem less common and less serious than that found in the United States; in West Germany, trials and sentencing hearings are unified, whereas in the United States trials and sentencing hearings are kept separate. As Thomas Weigend observes, in the United States, "judges seldom need to give reasons for their decisions. German judges must account for their decisions both orally and in writing" (p. 38). An obvious and important implication suggested in Part 1 is that the European criminal justice system has much to offer its counterpart in the United States—especially in the area of criminal sentencing.

In Part 2, "The American Experience," various factors affecting criminal sentencing in the United States are discussed. An argument is made for legislative guidelines to be used in sentencing, along with appellate judicial review of these guidelines. Franklin E. Zimring correctly suggests that the next generation of lawyers should be in a position to learn from the experiences of criminal sentencing procedures of the past. "But the draftsmen of the next generation of criminal codes must proceed from a perspective that is more worldly, more comprehensive, and more principled" (p. 119).

"Issues for the Future" is the subject of Part 3, which is composed of two final essays that each focus on a contemporary phenomenon characteristic of the criminal justice system. Norval Morris identifies some of the factors involved in sentencing the mentally ill and suggests that certain considerations should affect the type of sentence. The concluding essay by John C. Coffee and Michael Tonry recognizes the difficulty in achieving a consensus relating to the purposes of sentencing. Such a difficulty results in part from the fact that lawyers, judges, and other judicial officials have a vested interest in the status quo. There is a recognition that "a fundamental tension exists between the goals of sentencing equity and procedural fairness that necessitates difficult and value-laden trade-offs" (p. 155).

The various sources used for the essays not only lend credence to the validity of each author's particular view regarding criminal sentencing, but also may serve as a useful guide for those individuals who are seeking valuable bibliographical information about a particular approach to the subject.

A Review of Reform and Punishment has several benefits and should appeal to a wide variety of individuals interested in the criminal justice system. A reader quickly becomes knowledgeable about the importance of criminal sentencing in foreign countries as well as in the United States. In addition, one becomes aware of some similarities and differences characteristic of the criminal

justice system. Such an awareness provides the opportunity not only for reflecting, but also for perhaps a rethinking of one's own value system regarding the sentencing of criminals. Hence, this book may become a valuable source for new ideas that could form the basis for suggestions needed to improve the American criminal justice system.

WILLIAM E. KELLY

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Environmental Policy in the 1980s: Reagan's New Agenda. Edited by Norman J. Vig and Michael E. Kraft. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xi + 377. \$11.95, paper.)

Did the election of Ronald Reagan to the presidency in 1980 signal a precipitous decline in the nation's environmental quality? The contributors to this volume stop short of an unequivocal "yes." However, the evidence they amass suggests that the Reagan administration's efforts to reverse the environmental policy enactments of the 1970s certainly possess the potential for environmental degradation. Reagan's environmental policy agenda became clear early on when the transition task force report of the moderate wing was rejected in favor of the Heritage Foundation's ideologically driven recommendations. This set into motion a series of administrative moves (personnel changes, budget cuts, regulation relaxation) intended to change radically federal environmental policy. What Reagan and his closest advisors envisioned was a redesigned environmental policy system in which the private sector and subnational governments assumed expanded roles while that of the national government shrank. The new test of worthiness for environmental programs was to be their effect on the nation's economy. Yet, as the contributors explain, the administration has been forced to retrench its original objectives. Public sentiment, the intense glare of media attention, and a digging in of congressional heels made their pursuit politically risky.

Environmental Policy in the 1980s pulls together the talents of 17 leading environmental scholars to examine the Reagan impact on the environment. The collection opens with Vig and Kraft's theme-setting essay that identifies policy legitimization and institutional capacity as relevant criteria to guide the evaluation. The volume is organized into four parts, the first of which addresses agenda-setting. The message of the first part is clear: Despite Reagan's victory, there was no voter mandate for environmental policy reversal. The administration widely misperceived

public opinion. In the second part, the focus shifts to institutional behavior. According to the contributors, an aggressive president armed with a negative policy agenda used unilateral administrative tactics to impose his will. Congress, in this struggle, represented a reactive force as it sought to defend environmental protection laws. The courts continued their support of environmental issues, but their traditional deference to administrative expertise meant that several of the administration's deregulation goals were accomplished. Under Reagan's appointees, the primary agency involved, the Environmental Protection Agency, became an agency stripped of its constituency support. Lettie McSpadden Wenner's comment aptly describes the result. "The only litigant left to argue the environmental point of view in some lawsuits between industry and government were private intervenors who appealed cases lost by government" (p. 196).

To document further the relentless attack of the administration, the third section of the book deals with specific policy areas. In both air and water pollution control, for example, the Reagan administration pursued administrative adjustments to long-standing policy. However, in two newer policy areas—acid rain and toxic waste—an administration strategy of regulatory avoidance fell flat. In fact, allegations surrounding the mismanagement of the toxic waste program led to the massive EPA housecleaning in 1983, a major blow to the administration's plans. The persistent threat of a despoiled environment lurks in other policy areas as well. The negative impact of the Reagan administration in energy conservation, in wilderness land management, and in global environmental cooperation will be disquieting even to a marginal environmentalist. "Nonconservationist" appears to be an accurate descriptor of the policy stances taken by administration officials.

The last section of the volume is primarily a recapitulation and summary assessment of Reagan's performance. In a larger sense, it questions the ability of a popular president to recast consensus policy. Despite the rebuff of Reagan's initiatives, the vision of the immediate future is not filled with unbridled optimism. Environmental protection, amid competing budgetary priorities and plagued by scientific uncertainties, faces an arduous challenge.

Criticisms of the book are minor. Only infrequently does the data-rich analysis slip into prescription. Contributors are careful to avoid polemicism, going to lengths to point out aspects of the Reagan plan that may have produced environmental benefits. With 17 contributors, there is some repetition. For example, the virtual demise of the Council on Environmental Quality

is recounted several times as is William L. Ruckelshaus's admission that the administration had misread public opinion regarding environmental protection. Too, the book lacks an index. These shortcomings fade however alongside the volume's significant contribution to the environmental policy literature.

ANN BOWMAN

University of South Carolina

School Desegregation Plans that Work. By Charles Vert Willie. (Westport, Conn.: Greenwood Press, 1984. Pp. xi + 239. \$29.95.)

This book offers an equity-centered approach for measuring school desegregation efficacy. Past the thirtieth anniversary of *Brown v. Board of Education*, desegregation remains a controversial issue with parents, school administrators, politicians, and political scientists debating results of what has and has not been done to create unitary school systems. The debate remains unresolved among political scientists at least partly because of disagreement on what constitutes acceptable evidence. Some political scientists link effectiveness to lack of intergroup conflict and white flight. Charles Vert Willie puts a greater emphasis on examining educational attainment criteria. For Willie, desegregation works if the plan attempts to achieve equity among pluralistic school populations, attain teacher and administrator diversity, and develop ways to enhance the teacher's ability to work with children and a curriculum with multicultural concerns.

Willie's perspective leads him to be more optimistic than most of the researchers who concentrate on conflict and white flight. He argues that many current plans do work in educational equity terms. To illustrate this, he presents a comparative examination of desegregation planning and implementation in Boston, Milwaukee, Seattle, and Atlanta—four cities with very different demographics, politics, community relations, and local power structures, but that are similar in that their desegregation plans increased attainment of Willie's educational equity criteria.

Willie buttresses his argument with descriptive chapters on each city written by key participants in local desegregation efforts. Some of the authors are: David Bennett, deputy superintendent of the Milwaukee Public Schools; Alonzo Crim, superintendent of the Atlanta Public Schools; Robert Dentler, court-appointed expert in the Boston school case; and William Maynard, Seattle's desegregation director. The Boston chapter is particularly interesting because Dentler seeks to overcome popular stereotypes and the assess-

ment of some prior research. Most accounts of Boston concentrate on the negative aspects of Judge Garrity's intervention. Dentler argues instead that the Boston plan "achieved its racial equity goals. It also upgraded facilities, reformed the integrity of the board and administration, and brought excluded minorities into the circle of system concerns" (p. 78).

This revised appraisal of the Boston case continues in a subsequent chapter in which Willie and research assistant Michael Fultz compare Boston's white flight with patterns in Milwaukee, where students have greater school assignment choice. Willie and Fultz argue that if mandatory school assignment causes flight, Boston's loss of white students should have happened at a faster rate and with significantly greater magnitude. Finding few differences in pace or extent of white evacuation, they challenge the conventional wisdom that white flight in Boston occurred because of mandatory student assignment. For Willie, the rallying cry, "No bussing. We don't want another Boston," represents a serious misreading of the situation.

The book's equity perspective makes it important reading for those concerned with educational politics. To understand some parameters of its place in the academic debate on desegregation, one should read it in conjunction with the chapters on defining equality in David L. Kirp's *Just Schools: The Idea of Racial Equality in American Education* (University of California Press, 1982).

HINDY LAUER SCHACHTER

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Monetary Politics. The Federal Reserve and the Politics of Monetary Policy. By John T. Woolley. (New York: Cambridge University Press, 1984. Pp. xii + 280. \$37.50.)

Political scientists have devoted considerable research to analyzing how the pursuit of electoral success has resulted in the manipulation of the economic environment. Although not all scholars concur as to the regularity or periodicity of these politically generated business cycles (or even to their existence), they do agree that as an electoral strategy manipulating the economy makes a great deal of sense. Strategic intent, however, does not ensure performance. *Monetary Politics* is a welcome addition to this literature. Scholars searching for the politically induced business cycle must not only find motive, but also identify opportunity. Woolley's examination of the Federal Reserve Board (FRB) expands our knowledge in both directions.

Much of the existing research on political business cycles focuses on fiscal policy. Not at all a bad choice, considering that it is the structure of budget outlays and receipts that is controlled most directly by the president or Congress. However, one need not be a monetarist to recognize that monetary policy has an important impact upon inflation and the rate of growth of the economy. Yet because of the Federal Reserve Board's statutory independence, too little attention has been paid to monetary aspects of political business cycles.

Into this relative vacuum steps *Monetary Politics*. Woolley examines the Federal Reserve Board as a policymaking institution rather than searching the data for relationships between electoral timing and monetary policy innovations (although he does a bit of this too). This permits Woolley to trace the steps of policy development. The heart of this book examines the relationship between the Federal Reserve and each of the actors or interest groups that might circumscribe the FRB's independence. The pressures upon the Federal Reserve Board to respond to political considerations are intense. Articulate and powerful lobbies argue strenuously on both sides of the unemployment versus inflation issue. Monetary politics describes the Fed's careful balancing act between economic growth and inflation. Does the Fed make its decisions solely on the basis of technical considerations, or does it bow to political persuasion?

In successive chapters Woolley analyzes the relationship between the Fed and the banking community, economists, the president, and Congress. Woolley analyzes both the incentives and the capacity of each major actor to influence the Fed. Is there sufficient unanimity of opinion in the Congress to permit it to direct the Federal Reserve? Do economists have control over the opinions of Board members? How influential is the president in setting FRB policy? Woolley shows that none of these groups are able to control the

actions of the Federal Reserve Board. In a tightly argued chapter, Woolley shows that Congress neither has the inclination nor the purpose to direct monetary policy. The independence of the Board is a functional arrangement that allows individual congressmen to stay afloat in the murky waters of monetary economics while appearing to satisfy constituent demands. The chapter on presidential-Fed relations shows that despite intermittent attempts at jawboning, the president also fails to control monetary policy. Woolley believes the historical similarity between presidential positions and Reserve Board policies reflect a shared consensus on the correct course of action. When no such consensus exists, Woolley views the positions taken by the Federal Reserve Board as institution preserving; the Board assumes policy positions in an attempt to reduce political threats from the outside.

Monetary Politics is a valuable addition to the literature because of its attention to the monetary side of the political business cycle and for its institutional analysis. When balanced against its strengths, the weaknesses of this book are small. I think fault can be found with the type of "class analysis" that views Board members graduating from the same elite schools as holding a set of common interests. Further, what little statistical analysis presented is not always convincing. Evidence of a monetary cycle should probably focus on presidential election years versus nonelection years rather than the odd/even pattern that Woolley uses (p. 128). In addition, as Woolley recognizes, pair-wise analysis of interest groups may not capture actors acting in concert. Finally, I wish the research in this volume had included the Reagan-Volker period. I think the current set of monetary policy issues would have made valuable grist for the Woolley mill.

MICHAEL WOLKOFF

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Comparative and Other Area Studies

Spain: Conditional Democracy. Edited by Christopher Abel and Nissa Torrents. (New York: St. Martin's Press, 1984. Pp. ii + 198. \$24.50.)

Democratic Politics in Spain: Spanish Politics after Franco. Edited by David S. Bell. (New York: St. Martin's Press, 1983. Pp. xiii + 203. \$25.00.)

There has been far too little analysis of Spain's unique transition from authoritarianism to

democracy after the death of Francisco Franco in 1975, and an even more unfortunate paucity of qualitative research on the remarkably stable democracy that has since emerged. Taken together, these two edited volumes are an invaluable addition to English-language scholarship in the latter area. Both books have assembled a collection of articles that will be invaluable for specialists and accessible for all students of comparative politics.

The contributions to the Bell volume emphasize Spain's similarities with West European democracies. Spanish politics are, as Bell argues in his preface, surprisingly un-Latin. The disintegration of the party of the transition, the Union of the Democratic Center, convincingly explained by José Amodia, did not lead to a polarization of Spanish politics as many had feared. Elizabeth Nash's chapter examines the rapid moderation of the Spanish Socialist Workers party, an important factor obstructing such a polarization. Bell's contribution attributes the self-destruction of the Spanish Communist party, another ingredient of the centripetal nature of the party system, to the unresolved contradictions of Eurocommunism and the intransigent authoritarianism of the party leadership. Bruce Young contributes an informative account of the historic elections of October, 1982 that produced Spain's first ever single-party majority government and the young democracy's first alternation of power. What is lacking from the discussion of the major political parties is a chapter on the conservative Popular Alliance, now Spain's second largest party. The Popular Alliance is clearly one of the keys to the future of Spanish democracy, and a party whose democratic credentials have at times been called in question.

The Bell volume also contains two chapters treating areas in which Spain's politics continue to diverge from its West European counterparts. Mike Newton's chapter on the peoples and regions of Spain provides a cogent overview to an extremely complex aspect of the Spanish polity. Pedro Vilanova is, not surprisingly, the most pessimistic of the contributors in his consideration of the Spanish armed forces, which continue to pose the most formidable threat to parliamentary democracy. The three remaining contributions fit into the collection more awkwardly. Peter Holmes provides a straightforward description of relations between Spain and the EEC. Most readers will find the chapter on Spanish social structure, written by José Cazorla Perez and J. Mentabes Pereira, too descriptive and somewhat less useful. The book's major weakness is the inclusion of Pierre Subra de Biesses's piece on constitutional norms and central administration. It is dry, overly technical (much in the legalistic tradition of Spanish political science), and out of place in the volume.

The overall tone of the Abel and Torrents book is less favorable toward Spanish democracy. A number of the contributors question the profundity of the democratization process and point to the regime's inability to control the military, redistribute income, secularize politics, and narrow the gap between political participation of the sexes. The contributions of *Spain: Conditional*

Democracy overlap with those of *Democratic Politics in Spain* in three areas. There is a superb chapter on ethnic nationalism by Salvador Giner and a valuable essay on the Spanish Army by Paul Preston. In addition, Angel Viñas has provided an important analysis of the international dimension of Spanish politics, specifically, relations with the United States and NATO. *Spain: Conditional Democracy* places much less emphasis on party politics and focuses more on social and cultural aspects of Spain's new democracy. Audrey Brassloff provides a competent treatment of the role of the Catholic Church in post-Franco Spain. Nissa Torrents explores the renaissance in cinema and the media since the death of the dictator, and Juan Masoliver documents how the political change has influenced Spanish literature.

A particularly provocative contribution is Monica Threlfall's examination of women and political participation in the new democracy. Her chapter reveals a disturbing absence of women in positions of influence in the governing Socialist party and a less surprising lower voter turnout. There are two general treatments of the transition. Shlomo Ben-Ami's chapter on the legacy of franquism provides some interesting insights into Spain's transition from above, but Juan Antonio Ortega Díaz-Ambrona's highly descriptive synopsis of the Spanish transition does not add as much to the volume. Like its counterpart in the Bell collection, the contribution on the economy and popular movements, written by a team of scholars from the Universidad Central de Barcelona, seems too cursory to be of much use. To its credit, and in contrast to the Bell book, the Abel and Torrents volume includes a bibliography and index.

On the whole, both works are needed additions to a much understudied area of comparative politics. *Democratic Politics in Spain* would be better for classroom use, and is, on the whole, more informative and rigorous. *Spain: Conditional Democracy* treats an interesting series of topics not previously written about and is an excellent complement to the Bell volume. Despite their different emphases, both works underscore the considerable achievements of Spanish democracy, as well as the persistence of obstacles and limits. So far, Spain has managed to avoid the political instability of neighboring Portugal or the paralysis of Italian democracy. All the contributors would likely agree with Vilanova's assertion that Spain is a "weak, not particularly effective democracy, but a democracy nevertheless" (p. 147).

DONALD SHARE

University of Puget Sound

The Jewish Community in British Politics. By Geoffrey Alderman. (New York: Oxford University Press, 1983. Pp. ix + 218. \$34.95.)

The roles of Jews in British politics in the modern era raise a variety of issues deserving of distinct accounts in their own right. These include the part played by British Zionism in influencing the Middle East policies of the major political parties, especially in the wake of the Holocaust and the Suez crisis, and the contribution of both high-born Jewish intelligentsia and poorer immigrant Jews to the development of Labour and the trade unions. However, Geoffrey Alderman's well-crafted and lucid book, although inevitably touching on these and other matters, for the most part offers a more general and introductory guide to Jewish political involvement in Britain. The specific theme that integrates the book is the relationship between the political system and those British voters who are Jewish. The question raised is whether there is any recognizable tendency on the part of Jewish electors to react in a uniform way or as a distinct bloc, or both, to political issues.

Given the long-established and proud declarations by leaders of the Jewish community of the successful assimilation of British Jews in British social and economic life, Alderman clearly raises a sensitive issue. There is a strong tradition among Jewish leaders, from the Marranos of the seventeenth century onwards, of seeking invisibility, which has also extended on occasions to opposing more Jews coming into the country, particularly the pauper Jews from Eastern Europe at the end of the nineteenth century, on the grounds that this could raise anti-Jewish prejudices. It was this striving for assimilation that lay behind the campaigns for emancipation in the last century, although the removal of civil disabilities affecting British-born professing Jews, including the prohibition on a parliamentary career, was not supported by the more orthodox Jews mainly from Europe, who felt that it would lead to a loss of religious identity. Consequently, Alderman's conclusion that a Jewish vote exists and that a uniformity in Jewish electoral behavior can be traced to the last century may well upset parts of the Jewish community.

Alderman persuasively shows how the perception of a recognizable Jewish vote by politicians helped to sustain it. Politicians became and remained susceptible to Jewish claims once they realized that it could profit them, which in turn reinforced a distinctiveness in Jewish electoral behavior. There may be a lesson here for Britain's black population and its leaders, for Alderman's account suggests that it is in no groups' interest to be in the pocket of one party, for it leads to an in-

ability to advance interests by threatening to switch allegiance. The black community's long-standing and overwhelming support for the Labour party may have contributed to the largely feeble Conservative attempts to attract their votes. At various times, however, the Jews in Britain have supported the Liberals, Conservatives, and Labour in large numbers, and all the parties currently have pro-Israeli groups within their organizations.

One of the difficulties in the book in considering the proposition that an identifiable Jewish vote exists lies in disentangling the varying causes. For example, to what extent is the current Conservative allegiance of the Jewish vote the consequence of "non-Jewish" factors such as the increased middle-classness and suburbanization of the Jewish community? Alderman provides preliminary electoral and other data to indicate that "Jewish" issues, particularly the Middle East, have provided at least additional support for the Conservative party above that which could have been expected from socioeconomic status alone. Ironically, an appeal by a leading Jewish Cabinet Minister to Jews to vote Conservative to help contain immigrants had a marked success. Many Conservative MPs hold constituencies that are crucially dependent on Jewish electors who largely have a strong attachment to Israel and expect the member to recognize it. Yet the proposition for a Jewish vote, despite the *prima facie* case made by Alderman, may require more subjective data before its full validation. Unfortunately, Alderman appears to lack a comprehensive set of interview data from Jewish electors that would have provided a fuller understanding of their electoral allegiances and their bases. However, *The Jewish Community in British Politics* is a well-written, thought-provoking, and informative piece of work that provides the basis for further detailed scholarship.

ROGER KING

Huddersfield Polytechnic (England)

The National Question in Yugoslavia: Origins, History, Politics. By Ivo Banac. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 452. \$35.00.)

This book represents a prodigious effort to deal with a complex subject. In Part I, Banac discusses nation and nationality, lands and identities of peoples inhabiting the Balkans, their historical fates, and factors that shaped their mentalities and national outlooks. Despite some dubious statements, this part of the book is perhaps the

most valuable. In Parts 2, 3, and 4, Banac deals with political parties and movements. In Part 5, he considers the work of Yugoslavia's Provisional Parliament and the Constituent Assembly. In Part 6, there are some interesting reflections on the things that stood in the way of a successful union.

Banac is convinced that the Yugoslavs made their greatest mistake when they adopted a unitary system. For this he holds the Serbs chiefly responsible, although he admits that the "Croat political elite, middle classes, and most intellectuals were . . . committed to Yugoslavist unitarism" (p. 128). What is less clear from his presentation is that those representing Croatia in the Provisional Parliament were politically annihilated in the 1920 elections for the Constituent Assembly by the followers of Stjepan Radic, who refused to take part in constitution-making. Banac does point out that "Radic's private correspondence and public espousal of a Croat republic would seem to belie a supposed commitment to a federated Yugoslavia . . ." (p. 235).

It is clear, however, that the Serbs are Banac's *bête noire*. When he quotes Serbian politicians it is to reflect adversely on Serbs and Serbia, but not the other way round. He is hesitant to recognize such Serbian achievements as parliamentary democracy. Although he says that the matters he discusses "are still subject to widely varying interpretations," he either tends to ignore works that offer a different one from his, or he is selective in what he uses. By quoting Ante Trumbić, a leading Croatian politician, Banac suggests that the Serbs received "a position similar to the primacy of Prussia" (p. 133), but does not say that the Yugoslav constitution, unlike that of Germany, reserved no special position for Serbia. He writes of Serbian predominance in ministerial positions, but does not mention that Croat abstentions from parliament largely made that possible. Banac does demonstrate that the Serbs were fragmented into several parties, and therefore, unlike the Croats, could not speak with one voice.

The book contains a few factual errors (e.g., Ivo Andrić regarded himself a Serb and not a "Croat writer," many of the economic statistics that Banac takes from Rudolf Bićanić have been demonstrated to be seriously flawed, and the standing orders of the Constituent Assembly required an absolute majority for adoption of the constitution and not a "simple plurality").

Although the book supposedly ends with the adoption of the 1921 constitution, there are vexing assertions throughout concerning happenings in the 1920s and 1930s that lack context or evidence. Value-laden expressions (e.g., "Mouse King") tend to detract from the analysis. Ironically, in much of Banac's criticism there is evidence of a good deal of freedom, especially of

the press, in the early years of Yugoslavia's history.

Banac gives proper credit to Stojan Protić for his realistic view of the Croatian problem, and he rightly recognizes that Svetozar Pribičević played the role of a spoiler. The inconsistencies of Yugoslav communists on the nationality question are set forth clearly. And it is pleasing to note Banac's use of Great Serbia and Great Croatia (correct translations) rather than the modifier "Greater," which has come to have a pejorative connotation.

Despite its flaws, *The National Question in Yugoslavia* will be regarded as an important, although not final, word on the subject.

ALEX N. DRAGNICH

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The Islamic Threat to the Soviet State. By Alexandre Bennigsen and Marie Broxup. (New York: St. Martin's Press, 1983. Pp. iii + 170. \$27.50.)

Bennigsen and Broxup have collaborated to produce this brief survey of the relations between the Russians and the Islamic peoples before and after the Bolshevik Revolution. The book focuses primarily on the Central Asian peoples within the Russian/Soviet state, although some attention is given also to the Transcaucasus.

Despite what the title may imply, the book does not probe in any depth possible external threats from the Islamic world. Only one chapter is devoted to Soviet relations with the Muslims outside its borders, and even that chapter emphasizes the interaction of Soviet Muslims with the Islamic world. Although the authors posit that the Middle East has been destabilized since 1978 because of events in Iran and Afghanistan, they pursue the implications of destabilization only insofar as it affects the Islamic population within the USSR. The central focus of the book is the internal threat that Muslim loyalties pose to the ideological unity of the USSR. Since 1917, the Soviet Union has attempted to create the new "Soviet man" in a homogeneous Soviet culture. The Central Asian peoples with their strong Islamic heritage present a major obstacle to Sovietization.

The relationship between Russians and the Islamic peoples within Russia goes back to the ninth century and was intensified during the Mongol period in Russia. According to the authors, the Soviet image of the Mongol period remains a negative one, a time in which virtually no benefits accrued to Russia from their relationship with the Mongol hordes. Although oversimplified, over the centuries this image has condi-

tioned Russia's attitude toward Central Asia. Because of their historical perceptions, the Russians felt inferior to the Central Asian peoples whom they conquered (p. 8). Although on the one hand, they have regarded the Central Asians as backward people whom they must civilize, they have not forgotten that the ancestors of those very barbarians once ruled Russia. Conversely, the native Muslims do not feel they are inferior to the Russians. According to the authors, the Russian hatred toward the Muslims "makes the cultural or biological symbiosis between . . . [them] . . . a hopeless dream" (p. 9).

Bennigsen and Broxup outline the diverse approaches the Soviet state has taken to solve their Islamic problem, ranging from elimination to integration of the various Muslim peoples. They also chronicle the religious loyalties of the present-day Islamic population to organized Islam and informal Muslim groups. The authors conclude that the goal of nineteenth-century Islamic reformers, a "modern Muslim secular society" (p. 149), is near fruition within the USSR.

Although their arguments are well defended, the reader may speculate whether the authors overestimate the intensity of the Islamic loyalties within Central Asia and underestimate the efficacy of more than 60 years of Soviet influence. Central Asia has, to some degree, resisted the process of Sovietization and the Europeanization that it implies. Bennigsen and Broxup posit two major problems that will arise by the end of the century as a result of changing demographic patterns in the USSR: the "nativisation of the borderlands" and larger number of Central Asians in the armed forces, the so-called "yellowing of the Red Army" (pp. 131-135). The army and the borderlands will be populated by peoples less than totally loyal to the Soviet regime. Bennigsen and Broxup project that the Muslim nationalities may total as much as 25% of the overall population of the USSR by the year 2000 (p. 130). Will this be a serious problem for the USSR? Soviet sources tend to dismiss concerns about the rapid population growth in Central Asia, whereas various Western sources have suggested that the situation in Central Asia, combined with the stagnation of population growth in the European areas of the USSR, will pose significant problems in the future. Are Bennigsen and other Western scholars correct in assessing the probable impact of these trends?

This brief work is tightly packed with information of interest to Sovietologists and Islamicists. The advanced student will find this a valuable reference source. The book's scholarly value is only slightly diminished by the authors' tendency to skip back and forth through the centuries. Although strict observance of the chronology of

events might be ill-advised in describing the complexity of Russian-Islamic relations within the USSR, greater attention to the sequence of historical events would enhance the readability of this work for the nonspecialist. The glossary can help the reader decipher the numerous Islamic terms introduced in the text, and the bibliography will assist those who wish to read further on this topic.

NORMA C. NOONAN

Augsburg College

Democracy and Elections: Electoral Systems and Their Political Consequences. Edited by Vernon Bogdanor and David Butler. (New York: Cambridge University Press, 1983. Pp. ix + 267. \$39.50.)

Having written about electoral reform in Britain in *The People and the Party System* (Cambridge University Press, 1981), Vernon Bogdanor now joins with David Butler to present a volume dealing with the effect of electoral systems in EEC countries and Japan. Based on a 1982 Nuffield College seminar, the 13 original essays display unusual coherence. Narrowly equating democracy and elections, the authors ask if and how variations in methods for translating votes into seats in legislatures affect the character of governments chosen. Using history, they emphasize the importance of political choice and social context in the structuring of electoral systems and, in so doing, take an important step beyond Maurice Duverger, Seymour Martin Lipset, and Stein Rokkan.

Bogdanor identifies three methods for converting votes into seats: plurality, majority, and proportional systems. The plurality system, linked to territorial representation, provides stable and effective government while producing disproportional representation. Majority systems eliminate the possibility of a candidate winning on a minority vote. Proportional systems (PR), like the Irish single transferable vote (STV) and the continental list system, provide more accurate representation of preferences. Richard Rose points out that the differences between the methods is a matter of degree, while emphasizing that politicians' choices about constitutions also determine representation levels, the character of the legislative assemblies, the eligibility of voters, and the conduct of campaigning.

Surveying variants of the British plurality model, David Butler argues that the effect of the first-past-the-post system depends on circumstances. Even so, plurality systems usually foster two-party systems, penalize minor parties, and encourage moderation. According to David Goldey

and Philip Williams, the effect of regime crises and modernization in France since World War II resulted in a bipolar party system that has enabled the second ballot system, which had contributed to instability during the Third Republic, to produce stable governments under the Fifth. Peter Pulzer argues that the post-World War II German electoral system was less important to the development of stable government than the emergence of a bipolar party alignment and coalition politics.

Turning to PR systems, Christopher Seton-Watson argues that Italy's complex list rules, while perpetuating a multiparty system, unstable government coalitions, the dominance of party, and factionalism, promote a search for consensus among deep ideological and social divisions. Bo Sarlvik contends that PR systems in Scandinavia were an adaptation to the requirements of multiparty systems that achieved stability through the dominance of Social Democratic parties until the 1970s. Dick Leonard's study of the Benelux countries shows that PR systems with more members elected from each district produce more proportional results and more instability. While Paul McKee establishes that Ireland's STV system is popular and effective, Richard Clogg adduces that Greece's "reinforced" PR system favors the Right. As J.A.A. Stockwin demonstrates, Japan's multimember constituencies without a transferable vote reinforce its cultural conservatism, fragmenting the opposition and fostering local and personal factionalism. Finally, Michael Steed discusses direct elections to the European Parliament.

The volume's appreciation of the complexity of political and social context demonstrates the fallacy of Duverger's claim that plurality systems foster two-party competition and stability, whereas proportional representation produces fragmentation and instability; its emphasis on political choice adds to Lipset and Rokkan's use of political archaeology to explain party attachments. However, its use of the notion of democracy is surprisingly uninformed by democratic theory, which focuses on elite competition (Joseph Schumpeter and Robert Dahl) or links participation to popular influence (Rousseau and J.S. Mill). To identify electoral systems with democracy and then to make them the object of political choice simply ignores how they can transmit or contain electors' interests. Because democracy is obviously more than a ritual serving elite's interests in stability, the book would benefit from considering how electoral systems contribute to interest representation and leadership accountability.

JOEL D. WOLFE

Georgetown University

The Western University on Trial. Edited by John W. Chapman. (Berkeley: University of California Press, 1983. Pp. xiii + 238. \$24.50.)

The theme of this important volume is that the Western university has undergone changes during the past two decades that are inimical to its greater purpose, the pursuit of truth.

The book is based on papers presented by scholars in 1981 at the Third International Conference of the Council on the Future of the University held in Lisbon. The authors largely concentrated on experiences in their own countries, but their conclusions are nearly indistinguishable. Virtually all recognize that student protests during the 1960s about civil rights, free speech, and the Vietnam War became the genesis of student participation in the running of universities and the ceding of power by professors and administrators. An ethos of egalitarianism and democratization challenged the traditional hierarchy of power.

In an impassioned critique of American universities, Allan Bloom associates the lowering of standards with "the three cardinal sins of the egalitarian creed—racism, sexism, and elitism." Universities have sought expiation through grade inflation and affirmative action but, says Bloom, "no one who is thinking about such things can be thinking about education and scholarship" (pp. 157-158).

Descriptions of conditions in Europe are worse. Peter Kielmansegg observes that in countries where the government dominates the system of higher education, parliaments have been able to "turn the universities upside down" (p. 47). John Passmore points out that in "German mass universities there is no mechanism for singling out the brightest and best students" (p. 75).

Even more dismaying is Arend Lijphart's essay about university democracy in the Netherlands. The University Governance Reorganization Act of 1970 abolished exclusive control by professors over teaching and research. The act mandated that such activities be determined by councils composed of students, nonacademic staff, and representatives of society as well as faculty. This has led to politicization of the university and a decline of academic standards. Courses are frequently given "an ideological (usually Marxist) slant," while students act like trade unionists "fighting for less work, fewer hours, and easier advancement" (pp. 220-222).

Worrisome as the descriptions are, some appear dated, and related propositions are inadequately explored. In recent years, in the United States at least, competitive universities have reinstituted core curriculum requirements, and students have taken academic responsibilities seriously.

Although several authors yearn for a competitive system based exclusively on merit, there is little discussion about what the term means or how it is to be assessed. Grades and test scores seem to be the authors' favored indicators, but unquantifiable criteria like creativity, initiative, or perseverance are virtually ignored. Moreover, although several authors condemn affirmative action as incompatible with a merit system, no consideration is given to the possibility that a student's education is enhanced when the backgrounds of his peers are ethnically and regionally diverse.

Another unexamined contention concerns the propriety of regulating scientific activities. Kielmansegg labels such restraint as unjustified interference with "the free pursuit of truth," and Martin Bulmer writes similarly about institutional boards that review research on human subjects (pp. 52, 115). Yet neither author discusses the public's right to protection from hazardous scientific activities, and neither notes the past abuses of human experimental subjects that prompted the need for oversight. Although such imbalances are unfortunate, they do not impair the book's principal and compelling argument—that a proper university cannot be a participatory democracy.

Many contributors are pessimistic. They lament that the trends they deplore are unlikely to be reversed, especially in the European universities. They may not have discerned that in some countries changes were already underway at the time of the conference. "In the United States," Robin Winks observed then, "the push, in education as in politics, has been leftward" (p. 185). That was, of course, before the Reagan years.

LEONARD A. COLE

Ridgewood, New Jersey

Theories of Development: Mode of Production or Dependency? Edited by Ronald H. Chilcote and Dale L. Johnson. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 256. \$25.00, cloth; \$12.50, paper.)

Political Development Theory. By Richard A. Higgott. (New York: St. Martin's Press, 1983. Pp. xiii + 124. \$18.95.)

Both of these volumes are primarily directed to specialists in the field of development studies, and each in its own way makes an important contribution. They nevertheless are two quite different kinds of works, and thus will attract substantially different audiences.

Political Development Theory is basically a historiographic reconstruction of the debates in both establishment political science of develop-

ment literature and radical development of underdevelopment literature over the last 20 years. Higgott argues that although the political science of development approach has moved away from the grand theory of its behavioralist phase to a more specifically focused policy science orientation in the current postbehavioralist phase, both share the same assumptions about the nature of political development and thus lead to the same kinds of normative prescriptions. For Higgott, the pessimism about the prospect of development in the Third World that gripped the field of political science in the 1970s and led to perspectives that focused almost exclusively on the maintenance of order and ignored the question of imperialism is still evidenced in the current public policy approach to third-world development. He attributes this to the political science of development's need to maintain its professional image by adopting "the predominant trends within the mainstream of the political science discipline" (p. 43).

In much the same way that he chronicles the evolution of the political science of development approach, Higgott recounts the debates within the development of underdevelopment approach that have led from dependency theory's dominance in the early 1970s to the current mode of production perspective in the 1980s. Here, Higgott's efforts pale in comparison to similar and more comprehensive works like Albert Szymanski's *Logic of Imperialism* (Praeger Publishers, 1981). He nevertheless provides a fairly accurate account of how the more Marxist productionist mode of production thesis arose out of leftist critiques of circulationist dependency theory. However, while chiding dependency theory for ignoring classes and class formation within the context of dependency and underdevelopment, Higgott completely ignores the seminal effort in this regard of Fernando Henrique Cardoso and Enzo Faletto in their *Dependency and Development in Latin America*, first published in Spanish in 1969, long before the mode of production thesis appeared on the scene.

Perhaps the most interesting section of the book is chapter 4, in which Higgott argues that because of these recent revisions in political science of development and radical development theories—policy science and mode of production—they now share a common ground. Both, he argues, share a focus on the micro-political or economic level, and both see the importance of the state in the development process. If both can only put aside their normative assumptions and prescriptions, Higgott feels that a common research program may be possible.

This would certainly be a triumph for apolitical political science but would in effect lead to the sterility of both approaches. What

motivates each approach and guides its direction are just these normative assumptions and prescriptions—the political science of development and policy science approaches are inherently conservative and antirevolutionary, whereas the dependency (regardless of what some on the Left might say) and mode of production approaches are inherently radical and revolutionary. One may question whether or not each meets its mark, but there is no question as to their intent.

Theories of Development: Mode of Production or Dependency? clearly raises the dependency approach from the grave in which Higgott so neatly buried it. The volume is a collection of papers given at a 1979 Congress of Americanists panel that debated the issue of dependency versus mode of production theory. On balance, what clearly stands out from these chapters is that although some few still reject one or the other approach for ideological reasons, the trend is for dialogue and reconciliation in an effort to move on to substantially more meaningful analysis of development in the Third World.

Chilcote contributes an excellent introductory overview of the various essays that is followed by two chapters, one by Aijaz Ahmad and the other by Carlos Johnson, that are basically polemics that go over old ground in the dispute between these two approaches and offer nothing new to the initiated. The second section of the book contains two pathbreaking essays by Anibal Quijano on Peru and Norma Stoltz Chinchilla on Guatemala that are efforts at going beyond the theoretical debate. Quijano's essay more closely follows the *dependencia* formulation of imperialist penetration of Latin America but overcomes its narrowness by arguing that an articulation of monopoly capitalist (of external origin) and pre-capitalist (of internal origin) relations of production have created a situation of semicolonial accumulation in Peru. Chinchilla's contribution on Guatemala is somewhat more searching. Although she ultimately prefers the mode of production approach, Chinchilla never loses sight of the dialectic between the internal and external determinants of underdevelopment and class formation. What is most welcome in her essay is the systematic way she evaluates each approach—modernization, evolutionary Marxist, dependency, and mode of production—with regard to their adequacy in explaining the Guatemalan reality. She concludes by arguing that although the Guatemalan social formation was the result of the Spanish conquest, its subsequent development and transformation can be understood primarily as a result of the internal processes of class formation, accumulation, and politics (the state). The third section of the book includes papers by Andre Gunder Frank, Henry Veltmeyer, and Dale

Johnson. Frank's essay is a reworking of his recent works in which he further collapses every form of production into capitalism—now even socialism, because it has been “relinking” (trading) with the capitalist world, is defined as capitalist. Veltmeyer's contribution deals specifically with the problem of analyzing class formation in the Third World. He concludes that the processes of class formation and capital accumulation in “peripheral” societies are fundamentally different from those in the advanced capitalist countries. Dale Johnson adds an excellent conclusion that argues that the two approaches be joined in a two-level approach—one that addresses the question of the international accumulation of capital and class formation (macro-level) and internal class formation and relations of production (micro-level).

DOUGLAS FRIEDMAN

The College of Charleston

Khrushchev's Party Reform: Coalition Building and Institutional Innovation. By Barbara Ann Chotiner. (Westport, Conn.: Greenwood Press, 1984. Pp. x + 313. \$35.00.)

The Soviet Party-State: The Politics of Ideocratic Despotism. By Carl A. Linden. (New York: Praeger Publishers, 1983. Pp. xii + 174. \$24.95.)

The Khrushchev era is often regarded as a transitional period between the horrors of Stalinism and the conservatism and status quo orientation of the Brezhnev years. Yet it would be quite incorrect to evaluate Khrushchev's influence upon Soviet history solely in terms of personal ambition, for he was an innovator in several respects. First, via the political and theoretical demotion of Stalin at the Twentieth and Twenty-Second Congresses of the Communist Party of the Soviet Union (CPSU), Khrushchev was able to legitimize his own authority and thus prepare the way for new approaches. Second, Khrushchev sought to advance the power and influence of the Party and its apparatus, which had stagnated under Stalin. Third, Khrushchev's theory of the nature of the Party was his primary contribution to ideology; specifically, that the Party had already become a party or vanguard of the whole people, and no longer a party or vanguard of the ruling class, the proletariat. Last, Khrushchev reorganized the Party in November, 1962 into two separate hierarchies, one charged with practical economic leadership in industry and the other in the agricultural sector.

Khrushchev's Party Reform is a study of Soviet policymaking that examines the 1962 bifurcation

of the Soviet Community party from its earliest origins through its implementation and eventual repeal following Khrushchev's removal from power. Effectively utilizing the model of policy-making developed by Zbigniew Brzezinski and Samuel P. Huntington in their *Political Power: USA/USSR* (Viking Press, 1964), which suggests the existence of four decisional phases—initiation, persuasion, decision, and execution—Barbara Ann Chotiner expands this background for policy initiation by offering a fifth phase in the decisional process, a germination stage. By incorporating relevant features of coalition theory, outlined in William Riker's *Theory of Political Coalitions* (Yale University Press, 1962), and the ideational scheme, elaborated in Homer Barnett's *Innovation: The Basis of Cultural Change* (McGraw-Hill, 1953), which looks at the genesis of innovations, Chotiner's work offers several analytical advantages and a superior methodological thoroughness.

Comprised of nine chapters, Chotiner's book opens with the earliest indicators of Khrushchev's views towards Party work and the institutional debates that would precede any substantive organizational change. Indeed, Chotiner's evaluation of the innovative characteristics of the Party's restructuring scheme, and the politics surrounding the approval for discussing the bifurcation recommendations outside the Presidium during the initiation phase, is among the best on these topics to be found in Western analyses. Having received Presidium acquiescence in discussions of his party bifurcation proposal, according to Chotiner, Khrushchev could move more vigorously to persuade his associates in the ruling body and lower-echelon officials in the Party and government hierarchies (p. 111).

Regarding the overall effectiveness of the bifurcation, Chotiner notes the following:

Since many narratives about party aid presented the details of intervention as exemplifying correct production leadership after the bifurcation, the accounts furnish further proof that many *partkoms* concretized their style of work in a manner acceptable to the central CPSU bodies. Furthermore, summations of the local party organs' activity during 1963 and 1964 in the five categories of industrial and agricultural management have indicated the CPSU agencies' increased tendency to help economic leaders implement decisions. The existence of such a trend is another indicator that party apparatchiks' administrative methods became more operative after the November 1962 Plenum. Indeed, the division of the Community Party apparatus into industrial and agricultural components did seem to be accompanied by noteworthy changes in the new party organs' style of work. (p. 265)

In the final analysis, according to Chotiner, as political hierarchs seek to maintain the CPSU's influence, preeminence, and centrality in Soviet society, their ability to build coalitions behind various conceptions of the Party's role and character will depend upon the ability to place their ideas in the context of an exchange of policy promises, patronage, and perhaps even threats (p. 289).

The Soviet Party-State: The Politics of Ideocratic Despotism is, as its title implies, a work that focuses on the ideological bases of the Soviet party-state, without, however, losing sight of the barrier that the Marxian ideology—reinforced by its Leninist content—places before the Soviet leadership and the practically oriented, nonideological needs of Soviet society.

The term *ideocratic*, coined by Berdyaev to identify the Soviet regime, is, according to Linden, peculiarly apt as the classification of a regime that rests its claim to rule on an explicit and totalist ideology. The term *ideocracy* is suggestive of a secular analogue to theocracy, with its close tying of ideas as dogmas and power (p. xii). According to Linden, who is quite correct in his analysis, the structure of dictatorial power that arises out of Leninized Marxism stands out in bold relief, especially when looked at in juxtaposition to the actual role that Marxist-Leninist ideology plays within the Soviet party-state:

Is it the animating source pervading its activity or is it something of an elaborate pretext for the actual operation and self-perpetuation of the system of despotic power? Is it, in brief, its rationale or rationalization? The question already indicates the two broad schools of thought that emerge out of the long and sometimes exacerbated debate among students of Soviet and communist party-states around the nexus of ideology and power. Not surprisingly, the one identifies elements of connection, the other disconnection between the theory and the practice, the ideology and the reality of the party-state system. (pp. 32-33)

Comprised of six full chapters, Linden's book returns to the primary ideological source of the Soviet party-state to the original tenets of Marxism itself, and examines, among other things, the source of Soviet despotism—Russia or Marxism?—the choice of revolution, and the nucleus of the communist dictatorship, a nucleus of knower-intellectuals, the "communists" of the *Manifesto* (p. 23). Linden's chapters on "Marxism-Leninism and Ideocratic Despotism" and "The Ideocratic Party-State and Faction" are characteristic of the book's scholarship and commitment to analytical detail. Indeed, despite its official dedication to monolithism and its projec-

tion of an image of internal unity, the long history of internal cleavage within the rulership makes evident the fact that the Soviet party-state is not by any means a seamless entity (p. x). The last two chapters are concerned with the contemporary party-state, and examine the sharp counterpoint that developed between the Khrushchev and Brezhnev eras.

Linden concludes on the following note:

The Ideology that rules the rulers suffers from a critical defect. It lacks any practical wisdom or guidance on how rulers can go about extending political power beyond themselves with a hope of success and without fear of losing it all themselves. In its utopian vision of an ultimate liberation of man from domination, it speaks not of the distribution of political power but its radical disappearance. . . . The question then arises whether a ruler or rulership of the party-state can emerge capable of correcting the practical defect in its creed and if so can it be done without a basic change in the regime of the party-state? (pp. 159-160)

Linden's *The Soviet Party-State* and Chotiner's *Khrushchev's Party Reform* are works which will appeal to students of Soviet politics, comparative communism, and communist ideology.

JOHN M. CARFORA

Dartmouth College

The National Question in Marxist-Leninist Theory and Strategy. By Walker Connor. (Princeton, N.J.: Princeton University Press, 1984. Pp. xvii + 614. \$47.50, cloth; \$14.50, paper.)

Most works on nationalism in communist systems tend to focus either on the resistance of national minorities to the regime's assimilationist policies or on the expression of a nationalist outlook on the part of a ruling party, particularly in its relations with the Soviet Union or with neighboring states. Walker Connor deals with both of these questions, but places them within the broader context of a comparative analysis of how Marxist-Leninist theory regarding nations, nationalities, and nationalism has been applied in practice by various communist parties in their efforts to gain and to maintain power in multinational states. To do this, he has closely examined the variations in the nationality policies of the communist parties of the USSR, China, Yugoslavia, Vietnam and, to a lesser extent, those of Czechoslovakia, Rumania, and other nations.

After first looking at the basic tenets of classical Marxism on the question, Connor concentrates

on the "strategic Marxist" approach developed by Lenin in order both to combat the force of nationalism and to manipulate it on behalf of the international movement. Lenin's solution was the formula which called for national self-determination for ethnic minorities with the right to secession from a federation which would provide them with the forms of national autonomy. This was intended to be a dialectical strategy designed to mollify nationalist sensitivities by allowing the full flowering of national cultures while the centralized and internationalist Communist party organization maintained control of the content and provided the basis for the ultimate elimination of nationalist feelings through an eventual merging of nations.

One major section of the book is devoted to tracing how Lenin's formula in some form was utilized by each of the parties in their efforts to gain power. Using rich historical detail, Connor indicates how, despite the difficulties in adapting it to each party's particular circumstances, the formula generally did prove to be a useful temporary stratagem for achieving their aims.

The other major section deals with how the Leninist formula was adjusted or warped to the needs of various parties after they had achieved power. In those cases where the forms of national autonomy weren't completely abandoned, as they were in Vietnam, Connor shows that all the ruling parties, including Lenin's, have felt the need to adopt policies designed to provide a considerable hedge against any real expression of minority nationalism. Special attention is given to the implementation of policies aimed at promoting the language of the dominant nationality, controlling the recruitment and purging of minority personnel, and the gerrymandering of nationality areas to reduce even the forms of autonomy. Connor carefully traces the changes in intensity of application of these assimilationist policies over time in each country.

In arguing that the "Leninist solution" has either not been given a fair chance or has failed as a method for gradually eliminating nationalism within the multinational states, Connor provides a very insightful discussion of several contradictions in Lenin's analysis of nationalism which have made this failure virtually inevitable. Among these are Lenin's failure to recognize that national forms breed nationalist content and his failure fully to appreciate the power of the nationalism of the dominant nationality. It is the latter that has subverted the internationalism of the vanguard parties which Lenin believed would be the guarantors of the success of his policy.

There are several problems with the book. Although it is a thoroughly documented study replete with extensive explanatory footnotes,

Connor's attempt to encompass so many countries has meant that he is compelled to rely overwhelmingly on English-language materials. In addition, the arrangement of his analysis leads to a considerable amount of repetitiousness as he re-examines the policies of each party from various angles and at various stages of their development. Nevertheless, this is clearly an outstanding work of comparative analysis and a major contribution to the understanding of one of the most serious problems confronting the communist-ruled states.

DAVID L. WILLIAMS

Ohio University

Conflict in Northwest Africa: The Western Sahara Dispute. By John Damis. (Stanford, Calif.: Hoover Institution Press, 1983. Pp. 214. \$19.95.)

John Damis's book constitutes a valuable contribution to the growing literature on the Western Sahara. It may come as a surprise that such a "God forsaken scorching desert tract half the size of France with little water and less people" (p. 1) commands so much attention, but the accidents of history have made the Western Sahara a major bone of contention. The former Spanish colony is a major battleground between different nationalisms (Greater Moroccan, Greater Mauritanian, and Saharwi), between Morocco and Algeria who compete for hegemony in the Maghrib, between radical and conservative states in the Arab world and Black Africa, and between East and West. John Damis's book is a solid piece of work on the historical, legal, and ethnic dimensions of the conflict; on the rapid developments in the 1960s and 1970s; and on the shifting positions of the major actors: Morocco, Algeria, Mauritania, the political and military organizations of the Saharwi, Libya, Spain, France, the United States, the USSR, the OAU, and the UN.

Damis's book stands out for two reasons: Contrary to other books on the subject, his analysis is not one sidedly anti-Moroccan, and his balanced nonpartisan approach leads him to offer ideas for a resolution of the conflict, ideas which are realistic although complex.

The Moroccan case is well presented. Damis demonstrates the historical connection between Morocco and the Western Sahara from the times of the Almoravid dynasty to the present. He analyzes the ethnic ties between Southern Morocco and the major tribes of the Western Sahara and explains why it is difficult for Moroccans to understand why it was legitimate to absorb the Spanish protectorates in Northern and Southern Morocco, Tangier, and Ifni without a

referendum, and why the same procedure is illegitimate for the Western Sahara. He shows that because Moroccans view the Western Sahara as an integral part of historical Greater Morocco, they refuse to apply the principle of national self-determination within colonial boundaries in this case. Damis also emphasizes that the conflict is a truly national struggle and not one between reactionaries and progressives, because both the Moroccan nationalists and socialists (and even the communists) support the Moroccan claim to the Western Sahara as much as the king and the conservative forces. On the other hand, Damis does not dispute the existence of widespread anti-Moroccan sentiments among the Saharwi, the growth of a new Saharwi national identity, or the legitimacy of the demand for national self-determination.

Damis's compromise proposals are the most interesting part of the book. He feels that the total victory of one party and total defeat of the other should be avoided for the sake of peace and security. He looks for formulas that will, at least partially, satisfy all the parties concerned: Moroccans, Saharwi, Algerians, and Mauriticians. Damis spells out three formulas for the partition of the Western Sahara between Moroccans and Saharwi and border changes that will assure Algeria a secure and recognized boundary and Mauritania more territorial depth along the vital railway from Zouirat to Nouadhibou. The twentieth-century experience in the Middle East, the Malayan peninsula, India, and Ireland has demonstrated that partition is often the only pragmatic and humane alternative to an apocalyptic all-or-nothing approach. John Damis should be congratulated for his deviation from scholarly analysis and for his readiness to come forward with realistic compromise formulas.

BENYAMIN NEUBERGER

University of Pennsylvania

Terrorismo E Violenza Politica Tre casi a confronto: Stati Uniti, Germania e Giappone. Edited by Donatella della Porta and Gianfranco Pasquino. (Bologna: Societa editrice il Mulino, 1983. Pp. v + 263. L. 15.000, paper.)

On August 2, 1980 neofascists detonated a bomb in the waiting room of the railroad station in Bologna. The explosion killed more than 80 people and left several hundred injured. It was the worst single terrorist incident in the history of republican Italy. In the aftermath of the event, the regional government of Emilia-Romagna, which is headquartered in Bologna, agreed to sponsor a program of research aimed at understanding the role of ter-

rorism in contemporary Italian political life. The Istituto Cattaneo was given the responsibility for carrying out this project.

Terrorismo E Violenza Politica is the first of several volumes to be derived from the research. The purpose of this initial book is to establish a comparative context within which the recent Italian experience may be understood. To this end, the editors brought together scholars from the United States, West Germany, and Japan (Ted Gurr, Irving Fetscher, and Hiroshi Kawahara) so that they could review, for an Italian audience, the experiences of terrorism in their countries. Their contributions, along with a chapter by Charles Tilly on the history of collective action in England and France, constitute the bulk of this book. The case studies are introduced by Donatella della Porta's review of the major themes in terrorism research and concluded with Gianfranco Pasquino's assessment of their relevance to the Italian situation.

The choice of cases selected for comparison with the Italian reflects a most similar systems orientation. Like Italy, the other nations exhibit advanced industrialized economies and are ruled by democratic political regimes. Again, like Italy, they are nations where causes of a separatist-nationalist type have not been the major stimuli of terrorist activity. On the other hand, all four nations have histories of right-wing violence and were sites for the mobilization of left-wing protest movements during the 1960s.

Since the editors' intent is to use the American, German, and Japanese cases to shed light on the Italian, it seems appropriate for the book to be reviewed on these terms. What distinguishes terrorism in Italy from that found in the other countries? Pasquino specifies two features. First, although it is true that terrorism of a fascist, vigilante, or right-wing character has been present in the other countries, as the three case studies make clear, Italy stands out by virtue of the intensity of neofascist violence as well as by its covert encouragement by elements within the state apparatus. Illustratively, in none of the other countries was there a serious possibility that these elements would try to exploit events to initiate a rightist coup d'état as was true in the Italy of the early 1970s. Second, although all four countries experienced significant waves of terrorism beginning in the late 1960s and continuing into the 1970s, Italy's did not crest until the present decade. Left- and right-wing terrorist groups in Italy showed a capacity to endure, reorganize, and recruit new members that their counterparts in the United States, West Germany, and Japan did not display.

If these are the presenting symptoms, what are their causes? Presumably this question will be ad-

ressed thoroughly in the other volumes in this series. Pasquino's preliminary judgment has to do with the interaction between the formation of mass movements demanding sweeping change and willing to take to the streets to achieve it, and a blocked, change-resistant political system. Yet he also recognizes that these ingredients were not totally missing in the other countries.

A final point worth calling to the reader's attention is that conspicuously missing from the accounts of terrorism in the three country studies as well as in the conclusion is any attempt to ascribe common personality attributes to the terrorists as a way of explaining their behavior. The analyses are historical, social, and political, not psychological. The tone is set in the introduction by della Porta, who notes that attempts to stipulate a terrorist personality fail because available evidence does not indicate signs of abnormality. Further, she argues that efforts to attach psychological labels to terrorists serves, deliberately or otherwise, a political purpose. They divert attention from the social circumstances in which terrorism arises and from the performance of the governments against whom the terrorists act.

The individual case studies are of high quality and deserve more attention than they have received in this review. In short, *Terrorismo E Violenza Politica* represents an auspicious beginning to an important series of books.

LEONARD WEINBERG

University of Nevada, Reno

How to Compare Nations: Strategies in Comparative Politics. By Mattei Dogan and Dominique Pelassy. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. 185. \$11.95, paper.)

Hubert M. Blalock, in *Basic Dilemmas in the Social Sciences* (Sage Publications, 1984), has recently cataloged a series of "defects in our intellectual culture" which undermine the scientific credentials and academic integrity of social science research. These defects—terminological confusion, excessively detailed description, technical and ideological intimidation, and the failure to eliminate inadequate theory—afflict comparative studies as much as any of the myriad sub-disciplines now encompassed by the term social science. It is a serendipity, therefore, that no sooner has the malady been diagnosed, than the cure is at hand.

In *How to Compare Nations*, Mattei Dogan and Dominique Pelassy have constructed a succinct and unconventional guide to the conduct of comparative analysis and the construction of social science theory. They begin with a general

discussion of the purposes of comparative analysis—stressing the need for cumulative theory-building, stating their own preference for a functionalist theoretic framework, and arguing that overgeneralization is a minor evil compared to overspecificity. They would have comparative analyses begin in some theoretic context. Some of the possible starting points are described in eight chapters, each devoted to a principal analytical category of comparative studies. Among these are the concepts of social class, political culture, political clientelism, consociational democracy, and developmental crises. What is refreshing is that Dogan and Pelassy do not purport to say anything new about any of these concepts. Rather, they provide a series of systematic and comprehensive literature reviews, exploring each concept as it has been refined through successive applications of the comparative method. They craft insightful syntheses that highlight fruitful lines of inquiry and expose the sterile and unproductive. This is a timely remedy for a field overburdened by disconnected generalization and excessive neologism.

The remainder of the work addresses alternative strategies for structuring international comparisons and the derivation of conclusions and refinements of theory. This, too, is richly documented with illustrative examples and constitutes an excellent discussion of inductive reasoning and the interrelationship of theory and research.

Although their work envisions substantial reforms of conventional approaches to comparative studies, Dogan and Pelassy refrain from pointed criticism. The result is an appropriately liberal definition of the intellectual core of comparative studies; something which should represent a significant contribution to the further development of the field.

The book is an adaptation of *Sociologie Politique Comparative* (Paris: Economica, 1982). Although in the Forward the authors disparage their own ability to handle the English language, their style of writing and the general organization of the text are most pleasing. *How to Compare Nations* should be required reading for all first-year graduate students; its use at the undergraduate level would be a sign of educational professionalism.

GARY M. KLASS

Illinois State University

Soviet Local Politics and Government. Edited by Everett M. Jacobs. (Winchester, Mass.: George Allen & Unwin, 1983. Pp. 225. \$28.50.)

The 12 essays in this book cover selected aspects of political phenomena at the local level in the Soviet Union. They are revised versions of papers read in 1980 at the 2nd World Congress on Soviet and East European Studies. The 11 authors are from three disciplines in six Western countries.

The book's strength lies in the diversity of its resource base: traditional institutional analyses are complemented by election studies, sophisticated survey data of third-wave emigrants, first-hand field observations by Western scholars in the field and their use of the increasingly sophisticated studies by Soviet scholars. The book shows the field of Soviet local politics coming of age methodologically and analytically.

The book's strength also lies in shedding new light on old questions such as the Party's role in the Soviet political system. The traditional thesis of Party versus governmental organs appears prominently in five of this book's essays, the ones by Jacobs, Robert Hill, David Cattell, Carol Lewis, and Bohdan Harasymiw. Although the authors do not agree on the precise lineup of forces, they all see tension between Party and state as a central question, and Jacobs himself raises the possibility of a system breakdown if the tension is badly managed.

But other authors see a different relationship between Party and state at the local level. Speaking of mass political participation, L.G. Churchwood starts a different tack by observing, in the most theoretical chapter in the book, that "evaluation . . . by Western political scientists has largely depended on their ideological orientation . . ." (p. 45). If the "Soviet version of the Soviet model" is given some credence, then there may not be a contradiction between the simultaneous expansion of influence among Party, Soviet, and social organizations.

The two essays by Theodore Friedgut—"The Soviet Citizens' Perception of Local Government," based upon survey research among third-wave emigrants, and "A Local Soviet at Work: The 1970 Budget and Budget Discussions of the Oktyabr Borough Soviet of Moscow," based upon first-hand observation—are in general accord with Churchwood's admonition that what you see is partly a function of what you look at and how you look at it. A fuller discussion of Friedgut's model of Soviet political participation can be found in his *Political Participation in the USSR* (Princeton University Press, 1979), a work regarded as seminal by other authors in the volume under review. Stephen

Sternheimer's skillful development of a "propositional inventory" for a communications model of Soviet local politics is also in this vein. Sternheimer tests Western-derived totalitarian, pluralist, and bureaucratic models and finds each wanting in its own way.

The book's most significant weakness is the absence of a chapter comparing and contrasting data sources, methodologies, models, and how given issues are analyzed in different ways. In a rapidly developing field, it could define the field and orient further research efforts. As good as many of the chapters are, an integrating chapter would have realized the book's potential to be more than the sum of its parts. Even without that chapter, however, Jacobs's intentions of providing an introduction to Soviet local politics for the general reader and student and also of providing material interesting to the specialist have been amply achieved.

ARON G. TANNENBAUM

Lander College

Women, the Family and Peasant Revolution in China. By Kay Ann Johnson. (Chicago: University of Chicago Press, 1983. Pp. ix + 282. \$23.00.)

This is a well-written and meticulously researched account of women, the family, and Chinese Communist party (CCP) policy in China. Kay Ann Johnson begins her study from the premise that "marriage, family and kinship practices have defined and shaped women's place in Chinese society more than any other single set of factors" (p. 3). She then proceeds to present a chronological survey of CCP policy toward women and the family in the Chinese countryside.

The book is divided into four parts. Part 1 is a discussion and analysis of the position of women in Chinese society in the prerevolutionary era. Part 2 presents CCP policy toward women and the family during the Chinese revolution from the founding of the CCP in 1921 to the establishment of the People's Republic of China (PRC) in 1949. Part 3 focuses on family reform in the PRC during the early years from 1950 to 1953 with a detailed look at the implementation of the 1950 Marriage Law. Part 4 considers three selected events—collectivization, the Cultural Revolution, and the Anti-Confucian campaign—in the period after 1953, with an analysis of their consequences for rural Chinese women.

Johnson argues that the history of CCP policy toward women and family reform has been characterized by contradiction, compromise, and neglect resulting in at best only limited progress

toward the realization of women's rights and gender equality. Although theoretically committed to a Marxist platform of gender equality, the CCP found it expedient from the very beginning to softpedal women's issues, subordinating its program of marriage reform to the preeminent need to gain the support of a traditionally minded male peasantry. After consolidating power, the CCP remained disinclined to push for a fundamental restructuring of family roles. The zenith of CCP reform efforts, the Marriage Law campaign of 1953, evoked controversy within the highest levels of the Party and was met with widespread cadre resistance to implementation at the grass-roots level. Johnson's depiction of the dilemma faced by a Marxist-Leninist party seeking a socialist revolution in a peasant society might be seen as yet one more vindication of the Menshevik position. But Johnson is critical of the classic Marxist view of women and marriage, particularly in what she terms as its Engelsian variant, as "extremely inadequate for understanding and trying to alter the dynamics of women's subordination" (p. 231). Labeling the Marxist perspective deterministic and mechanistic in its one-sided identification of productive labor as the key to female emancipation, Johnson maintains that the Marxist position ignores the influence of culturally defined patriarchal attitudes and an oppressive family structure as independent variables inhibiting the transformation of women's roles. Although not denying the existence of economic and structural constraints, Johnson argues, persuasively in my opinion, that political and ideological forces can affect the dynamics of societal change and that the CCP, rather than accommodating with traditional elements, might have chosen alternative policies that placed a higher priority on the goal of gender equality.

Women, the Family and Peasant Revolution in China is of interest to the political scientist as well as to the China specialist and the student of women's studies. Johnson does a skillful job of connecting women's issues to the twists and turns of CCP policy, demonstrating her knowledge of the Chinese political scene in the process. Johnson is also to be commended for her efforts to integrate material from political science, sociology, and women's studies with a view to making cross-disciplinary and cross-cultural comparisons. One wishes, however, that Johnson had pursued the (admittedly obscure) question of CCP decision making in more depth. One learns a great deal about the content of policy; less about the dynamics of the decision-making process. What factors, for example, motivated the CCP to launch the Marriage Reform campaign of 1953, and why did the promoters of the policy emerge ascendant over their detractors? Part 4 of the

book, moreover, is not as well developed as the preceding sections. The chronological format is partially abandoned for a selective issue focus that provides less of an interpretative framework and assumes some prior knowledge of Chinese politics. In addition, Johnson's concluding comments are by now out of date due to the sweeping policy changes instituted in the Chinese countryside in recent years (although her prediction of the return of infanticide has been confirmed by subsequent events). Nonetheless, judged as a whole, it is unlikely that this book will soon be outdated. Johnson's research makes a valuable contribution to our understanding of the relationship between women, the family, and CCP policy in China.

JEANNE L. WILSON

Wheaton College

Interest Groups and Development: Business and Politics in Pakistan. By Stanley A. Kochanek. (New York: Oxford University Press, 1983. Pp. xiv + 393. \$19.95.)

Stanley Kochanek is one of the few South Asia specialists who have succeeded in doing research on a topic in both India and Pakistan. The present volume is a companion to, and logical extension of, his *Business and Politics in India* (University of California Press, 1974). The subject has been neglected by political scientists, who have left it to the economists with their quite different orientation.

For his paradigm, Kochanek applies the structural-functional framework of development theory pioneered in the 1950s by Gabriel Almond. This drives him to rather negative conclusions about the degree of modernity and efficacy of Pakistani business associations:

Although the external forms of organization in Pakistan are based on western models and appear to be similar . . . their internal structure and behaviour tend to be quite different . . . characterized by highly personalized styles of leadership, intense internal conflicts based on primordial ties, inadequate resources, a lack of professionalization . . . and low levels of mobilization, participation and institutionalization. . . . As a result [they] have lacked internal cohesion and suffered frequent organizational breakdown . . . which have enabled the government bureaucracy to penetrate, direct, and control their activities. (pp. 87-88)

Big business has tended to avoid the Chambers of Commerce and Industry and rely on individual contacts with bureaucrats and politicians in order to protect its interests.

All of this would be not very surprising to students of third-world development except for the few, tantalizing contrasts that Kochanek provides of Pakistani versus Indian business organizations. If India has been more successful in overcoming the barriers of caste, region, and language to fashion a modern mode of access to, and representation in, government, can the difference be ascribed to culture, or is it largely the contrast in the two political systems, with India's being largely democratic and Pakistan's authoritarian for long periods? Kochanek does say that although the epoch of the elected Bhutto regime (1971 to 1977) saw business of all sizes coalescing to better defend themselves from the Pakistan People's party's nationalization measures, they subsequently reverted to the personalistic mode of access under General Zia ul-Haq's rule. The fact that the Karachi edition of the book omits chapter 16, "The Zia Interregnum," raises the important issue for scholars and publishers of how far different editions should be tailored to the perceived preferences of regimes in power. Perhaps an alternative resolution of this dilemma would have been for Kochanek to reduce the number of broad judgmental statements about successive Pakistan regimes and focus the book more narrowly on business organizations themselves.

A more basic consideration has to do with the appropriateness of the structural-functional framework in analyzing and evaluating non-Western social and political institutions. The book does not reflect the deluge of both academic and political criticism of the single-path prescription for political, economic, and social development in the past two decades. If one does not wish to accept the Marxist, Maoist, or environmentalist attacks, at least the findings of Myron Weiner's *Politics of Scarcity* (University of Chicago Press, 1962) could be incorporated if an author proposed to deal with the wider context of interest groups and development in Pakistan. Functionalism itself was designed in part to make cross-cultural comparison more valid, but Kochanek does not ask how—and how well—the functions normally performed by business organizations in the West are done in Pakistan, and what the results for the whole system are.

These questions notwithstanding, the book is extremely well researched in a country where a lot of the basic data are not as easily obtained as in the United States or India. No scholar's or library's collection on Pakistan will be complete without it.

THEODORE P. WRIGHT, Jr.

State University of New York at Albany

Political Support in Canada: The Crisis Years.

Edited by Allan Kornberg and Harold D. Clarke. (Durham, N.C.: Duke University Press, 1983. Pp. xvi + 448. \$36.75.)

This work collects 14 scholarly essays that assess Canada in recent years. Appraised are such topics as political support and group dominance, national identification and political support, the political socialization of political support, political support and the mass media, media agenda-setting in the 1979 nationwide balloting, the failure of the national party system, popular attitudes and intergovernmental shifts in health program responsibility, political support and environmental policy, the resurgence of Canadian nationalism, nationalist ideology in Quebec (with particular reference to the Roman Catholic religious variable), and the Quebec "independence" referendum of 1980.

Of special interest is the book's longest essay, Alan C. Cairns's examination of the 1980 through 1981 developments climaxing with the patriation to Canada of a constitution with a bill of rights (the Charter of Rights and Freedoms) and comprehensive amending formulas:

This package is portrayed as a matter of honor, as the federal government's response to the vague commitment for renewed federalism given by Ottawa to the Quebec people in the referendum campaign. Not only did the referendum fail to further Parti Quebecois constitutional objectives, but by a profound irony Trudeau has turned the referendum on its head. From his own participation and that of other federalists he has extracted from the victory of the 'no' forces a mandate to implement the precise reforms of Canadian federalism he has been pursuing for more than a decade. (p. 438)

Former Prime Minister Trudeau, Cairns recounts, long opposed the fostering by provincial governments of provincial cultures and economies for which they could speak exclusively. Rights entrenched in the Charter and enforced by a federal Supreme Court "inevitably will be employed as a resource by a future prime minister in the never-ending power struggle between federal and provincial governments" (p. 432).

Cairns records that the extent to which the Trudeau government deliberately sought this consequence was unclear, but adds that "Trudeau has long and patiently awaited the opportunity to implant his vision of Canada on the fragmented country he inherited" (p. 443).

The volume overall presents a worthwhile body of work.

GEORGE STEVEN SWAN

St. Thomas University

Islam in the Political Process. Edited by James P. Piscatori. (New York: Cambridge University Press, 1983. Pp. 240. \$37.50, cloth; \$14.95, paper.)

The essays in this volume stem from a 1981 conference at the Royal Institute of International Affairs at Chatham House, England. The book is the result of a research project funded by the Ford Foundation on the impact of Islam on the international system. The 10 essays, the Introduction, and the Conclusion examine the role of Islam in the domestic politics of 11 selected Muslim countries: Algeria, Egypt, Indonesia, Iran, Iraq, Pakistan, Saudi Arabia, Senegal, Sudan, Syria, and Turkey. In most cases, well-known scholars look into the circumstances and historical background that have made Islam significant in those countries.

Islam, the religion of the overwhelming majority of the people of the countries under study, has a guiding and often decisive influence on the conduct of governments and individuals. It is significant that the non-Islamic world has belatedly discovered the Islamic revival only after the Iranian militants sought the late Shah's return to face Islamic justice. The fact that the Shah's supplanter seems to have succeeded by appealing to religious sentiment has astonished Western onlookers, who take it as axiomatic that religion and politics have little to do with one another.

In addition to editing the volume, Piscatori contributes the Introduction and chapter 4. In the Introduction, he delineates the various approaches to the study of Islamic politics, conservative and scientific. A 10-page introduction is at a definite disadvantage when it purports to examine various aspects of a world religion, yet Piscatori has managed to write a useful outline of the 10 essays on the internal political dynamics of the various nations.

The essay on Egypt argues that religion is emasculated by the state. The chapter on the Sudan traces the evolution of a "consensus" calling for the establishment of an Islamic state. The essay on Saudi Arabia focuses on the blending of tradition and modernity; that on Syrian and Iraqi politics maps out the various norms and values of Islam in the two countries. The essay on Algeria examines how Islam is used by both the government and its opposition. The chapter on Senegal narrates Sufism as a mode of Islamic devotion. The seventh chapter shows that religion is shaped simultaneously by social, political, and economic factors in Turkey. The essay on Iran highlights Khomeini's concept of "guardianship of the juriconsult." The next essay deals with the Islamization process as a bond of unity in the historically unstable country of Pakistan. The tenth chapter

describes the various problems that Islam faces in consolidating Indonesia.

The concluding essay summarizes the common themes of Islamic politics: the urbanization of Islam, the use of the religion in the political jargon of both governments and their opponents, and the role of religious institutions such as the mosque and the religious school as intermediary between the individual and the state.

There is no single approach in these essays, and the authors have called upon the insights of various disciplines. However, all of the authors are convinced that the idea of Islamic politics needs to be specific if it is to be made more meaningful and creditable in the contemporary world.

Throughout this detailed and thoroughly documented study, the reader finds an erudite and intricate exegesis of Islamic principles and doctrines. Although an integrating essay would have improved the volume, it is a serious study meant for scholars and advanced students of Islamic politics and government.

SHEIKH R. ALI

North Carolina Central University

Juan Peron and the Reshaping of Argentina.

Edited by Frederick C. Turner and Jose Enrique Miguens. (Pittsburgh: University of Pittsburgh Press, 1983. Pp. xii + 268. \$24.95.)

This collection is a serious attempt to assess dispassionately (if in a friendly fashion) some of the key aspects of the nature and consequences of the Peronist governments and movement. Contrary to its title, the book is primarily about Peronism and not Peron. Most of the authors, both Argentines and North Americans, are established scholars with considerable background on Argentina. They present both reviews of the existing literature and original research and insight.

The articles in this collection have more cohesion than is generally the case in edited works. However, a weakness of the book is the failure of the editors to make that cohesion more explicit. At least three themes stand out. The first is rationality—in contrast to so many works on the movement. Peronism is seen as growing out of and reflecting the forces shaping and dividing Argentina. The persistent support of the urban working class is seen as a rational response to Peron's incorporation of them into the political process and his attempt to raise their real income. Evita is presented as hard working and successful, at least in the short run, in aiding those most in need of help. Peron himself is portrayed as an able politician rooted in Argentine traditions—albeit, also, a very human man with many flaws.

A second concern is the composition of the Peronist movement. Jose Miguens uses survey data from the 1973 elections to demonstrate the strong support of the working class for Peronism in preference to the parties of the Left. His data also show that although Peronism draws its greatest support among the working class, it is in fact a multiclass movement. In an exceptionally well-done essay, Manuel Mora y Araujo and Peter H. Smith use aggregate data to demonstrate that after 1946 a significant negative correlation developed between level of development and Peronist vote. In industrial areas, the Peronist vote is strongly class-based, whereas in less-developed regions, poor social conditions and traditional political elite influences contributed to Peronist success.

Third, Peronism is seen as both a source and a reflection of the conflict that has racked Argentina for the last 40 years. The most noticeable source of instability is the inability of successive governments to come to grips with Peron's inclusion of the working class as an active participant in the political and economic systems. In a very perceptive essay, Gary Wynia compares the incomes policy strategies used by military, democratic non-Peronist, and Peronist governments after 1955 and concludes that each of three strategies—autocratic, bargaining, and cooptation—achieved modest short-term success but eventually failed, particularly for political reasons. Antonio Donini shows Peronism reflecting Argentina's religious divisions.

The weaknesses of the book are more in its editing than its substance. The introductory chapter and the Epilogue do too little to pull the collection together. Although the thrust of his argument seems sound, Miguens's critiques of the misinterpretations of Peronism tend to be too brief to be convincing or are just as strained as the argument criticized. Roberto Guimaraes's analysis of public support for terrorism as reflected in survey research raises serious questions about several common explanations of terrorism such as relative deprivation. However, the article has several weaknesses that could have been eliminated easily. For example, on page 197, Guimaraes improperly adds the variance explained by two separate multiple regression equations but then on the following page essentially corrects himself.

The book as a whole will be of value to professionals and graduate students interested in Argentina, Peronism, and populism, and several chapters would make excellent assigned reading for undergraduate or graduate courses—especially the chapters on Evita, incomes policy, religion, and voting patterns. Contemporary historians should find a useful source in Wayne

Smith's highly detailed account of the events in 1971 through 1973 leading to the return of Peron. The book contains endnotes, an index, a chronology of Peron's public career, photographs, and tables.

ROBERT E. BILES

Universidad de los Andes (Bogota)

Brazil in Transition. By Robert Wesson and David V. Fleischer. (New York: Praeger Publishers, 1983. Pp. iv + 200. \$24.95, cloth; \$10.95, paper.)

Brazil in Transition is the latest volume in the Hoover Institution's series on politics in Latin America. Like the other volumes in the series, it seeks to provide an overview of Brazilian politics for those with little background in Latin American studies.

Brazilian politics have undergone monumental changes in the past decade, and this volume provides a useful analysis of contemporary political events. As such, the volume is very useful for those individuals who have not kept abreast of the rapidly evolving Brazilian political scene.

The book's weakest point lies in its lack of an explicitly theoretical approach. The brief historical introduction provides little more than a glimpse of the complex forces shaping Brazilian political history. For example, the authors discuss Brazil's national political styles in terms of *coronelismo*, clientelism, and mass political styles, but fail to carry these useful conceptualizations through the rest of the book.

What Wesson and Fleischer lack in theoretical perspective, however, they make up for in their impressive display of knowledge of the contemporary Brazilian political scene. As the title of the volume indicates, they are primarily concerned with exploring the prospects and forces for political change. The vagaries of the *abertura* process are presented in fascinating detail, with particular attention to the complex electoral maneuvering of the military government in attempting to control the democratization process.

Wesson and Fleischer successfully blend historical and current material in their treatment of both political parties and the bureaucracy. Contending political and institutional goals of the bureaucratic agencies, for example, are discussed in greater depth than most introductory analyses of Brazilian politics. Perhaps reflecting the authors' years of experience in Brazil, the reader is left with the impression that they are well versed with the nuances as well as the formal political structures of Brazilian politics.

The volume is also useful in its treatment of dif-

ferences among military regimes. Paralleling current trends in scholarship on Brazil, the armed forces are not portrayed as a monolithic entity; rather, Wesson and Fleischer focus on the more interesting questions of divergence between military factions and regimes.

Brazilian-U.S. relations are analyzed in the historical context of U.S. support for and participation in the 1964 military coup. The ebb and flow of the U.S.-Brazil axis are traced carefully, with attention paid both to areas of convergent interests between the two nations and to the numerous cases of disagreement between the two governments. Wesson and Fleischer explain Brazilian foreign policy as "pragmatic, frankly self-interested, and resolutely nonideological, identifying neither with the West nor the Third World" (p. 164). Unfortunately, only minimal attention is devoted to Brazilian foreign policy toward the Third World—an area of growing political and economic significance to the government.

In the final chapter, Wesson and Fleischer underscore the volume's theme: *Abertura* is the inevitable result of the pressures generated by both the economic growth of the early 1970s and the recession of the early 1980s. The interrelationship of political and economic pressures is carefully and sensitively drawn.

Overall, this is a well-written book that will be most useful to both beginning students of Brazilian politics and to scholars who have not revised their views of Brazil since the *abertura* process acquired its own dynamic several years ago. The volume's sketchy historical treatment and theoretical superficiality preclude its emergence as a pioneer in the field of Brazilian politics. Although this volume will never have the impact of, say, Peter Evans's *Dependent Development* (Princeton University Press, 1975), it serves an important function for those wishing a sophisticated overview of the Brazilian political system.

ELIZABETH G. FERRIS

Lafayette College

The Labour Party in Crisis. By Paul Whiteley. (New York: Menthuen, 1983. Pp. ix + 253. \$25.00, cloth; \$12.95, paper.)

Paul Whiteley begins his study on an ominous note: "The Labour Party is in serious trouble, and faces a series of crises greater than it has experienced since the 1930s" (p. 1). Although his book was at proof stage when the 1983 general

election was called, the outcome of this most recent British election simply serves to confirm Whiteley's pessimistic diagnosis. As he points out in a postscript analyzing the 1983 outcome, Labour's share of the popular vote fell to its lowest level since 1918, and the party won just 20.6% of the registered electorate.

The first part of this book is devoted to a close analysis of the series of crises to which Whiteley refers at the book's beginning. The first of these is the persistent but recently sharpened ideological crisis between left and right within the party that precipitated the formation of the breakaway Social Democratic party in 1981. Within the context of this discussion, Whiteley offers a particularly useful analysis of ideological patterning and constraint among Labour supporters and the Labour elite, charting a growing shift to the Left within the party. The second of the crises concerns individual party membership, which Whiteley suggests has been falling at an average annual rate of 11,000 over the postwar years. The third crisis results from the more immediately evident electoral decline of the party, particularly since the 1960s.

All three crises are clearly interrelated, and Whiteley's central thesis is that it is "failures of policy performance" (p. 1) that lie at their heart. The second half of the book accordingly sets out to analyze those failures, focusing in particular on the economic and welfare record of Labour in government.

Whiteley's overall argument is a plausible one. Throughout his analysis of the crises facing Labour he emphasizes the instrumentality or contingency of the party's support, suggesting a model of British voting in which the central assumption is that "voters support a political party because it brings them objective and subjective benefits at the individual and collective levels" (p. 90), although later speaking of "an instrumental electorate motivated by performance rather than promises" (p. 104). By then drawing attention to Labour's failures in office, Whiteley manages to construct a convincing explanation of the party's contemporary malaise.

It is not Whiteley's intention to suggest that poor policy performance is the one and only source of Labour's decline. Rather, by moving away from traditional sociological explanations, he acts to place the responsibility for decline more squarely on the shoulders of the party itself instead of seeing it as the more or less inevitable outcome of socio-structural transformation. What Labour does then becomes at least as important as what happens in the society around it. In this sense, Whiteley's study is a valuable attempt to redress the balance of explanations of British political change and as such, should be read by

the practitioners as well as the students of Labour politics.

PETER MAIR

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National Communism. By Peter Zwick. (Boulder, Colo.: Westview Press, 1983. Pp. x + 249. \$28.50, cloth; \$12.00, paper.)

Peter Zwick treats in this book the relationship between nationalism and communist movements. The first third of the volume is devoted to this relationship from Marxism's inception to the dissolution of the Comintern in 1943; the remainder deals with the rise of "national communist" regimes and parties since World War II.

Zwick displays a lively prose style and command of a remarkably broad variety of source materials. Certain sections, such as those on Marx's wrestling with the national question and the Yugoslav road to communism, are particularly insightful. But these merits are probably outweighed by the book's obvious flaws, some of which are attributable to its overly ambitious scope.

Zwick's thesis is that "national communism" is the only viable form of communism and that this has been true from the origin of Marxism. However, the connection between nineteenth-century Marxism and the behavior of contemporary communist party-states is not entirely clear. Moreover, the analysis in the early chapters poses a problem for the entire volume. Despite an exhaustive examination of communism and nationalism, these key concepts remain ambiguous.

More troubling than ambiguity of the two key concepts is treatment of the next level of concepts. Zwick seems mostly to accept at face value the rhetoric of communist spokesmen, and such terms as imperialism, exploitation, and national liberation are used uncritically. True, there is a late clarification on "imperialism" (p. 218) and an acknowledgment that communist systems can also be imperialistic (p. 227), but by then the damage has been done. Generally, the approach to communist systems comes across as rather naive. Pre- and noncommunist regimes are described in emphatic terms as oppressive, but there is little commentary about communist oppression, particularly in regard to national liberation movements.

Notably, Zwick attributes the dominant role of Kim Il-sung in North Korea, where communist totalitarianism is probably more pronounced than anywhere else in the world, solely to that leader's popularity (p. 181). Further, it is rather curious that Zwick's main authority in his interpretation

of Castro is Herbert Matthews. Surely the latter's murky role as a legitimizer of Castro in the 1950s should raise some questions about his reliability as an appraiser of the Cuban leader in the 1960s.

In general, Zwick places too much emphasis upon the factor of popular support in communist systems and devotes insufficient attention to the central role of coercion in *all* party-state regimes. There are also two particularly puzzling problems in his wide-ranging survey. First, although Soviet nationality policy under Lenin and Stalin is covered in detail, there is no consideration of nationality conflicts within China. Second, on Zwick's showing, Nicaragua should not be included under the rubric "national communism," but it is nevertheless so included, and it is not clear why this is done.

The dichotomy between nationalism and international communism surely does not fully explain communist behavior. Zwick omits factors in communist party-state politics other than nationalism,

and the argument on the singularity of national communism does not account for obvious linkages and conditions of dependence. Finally, a consideration of contemporary Soviet integration theory, strongly reasserted in the 1970s and apparently without theoretical limits, would have been most instructive.

One can only suspect that a stronger lead from readers and editors on matters of scope and direction would have resulted in a vastly different and better book. Zwick possesses a truly encyclopedic knowledge of the history of Marxism and, given free rein to develop his thesis without spatial limitations, or alternatively, compelled to adopt a more narrow behavioral focus, he might have fulfilled the expectations aroused by the dazzling brilliance of the book's opening passages.

R. JUDSON MITCHELL

University of New Orleans

International Relations

Arab Petropolitics. By Abdulaziz Al-Sowayegh. (New York: St. Martin's Press, 1984. Pp. 207. \$25.00.)

The thesis of this book, that "the oil crisis is rooted in the Palestinian question rather than in the seeming imbalances between the supply of and demand for oil" (p. 192), is outlined under four subjects: (1) the relationship between the Arab-Israeli conflict and Arab use of oil as a political instrument; (2) the development of Arab oil strategy; (3) the determining factors in the use of oil power; and (4) the opportunities for, and limitation of, the use of oil power.

In *Arab Petropolitics*, Al-Sowayegh attempts to examine the relationship between oil and politics through a case study of the Arab-Palestinian-Israeli conflict. He contends that this political problem and the oil crisis are synonymous. This is an erroneous assumption because the Palestinian identity did not occur recently, nor is it yet complete. It has been vague ever since the Romans coined the term in the second century.

Equally puzzling is Al-Sowayegh's conclusion that the oil crisis is linked inextricably with the Arab-Israeli conflict, a conclusion that ignores the roots of the two incompatible nationalisms—Palestinianism and Zionism—which produced the troublesome problem for Palestine. It is important to remember that the direct Arab interests in Palestine began to develop only with its conquests

more than 13 centuries ago when the Palestinians became Arabized.

It is well known that the petroleum industry began to expand in the nineteenth century when none of the Middle Eastern countries even knew that oil as a commodity existed in that part of the world. Al-Sowayegh is right when he says that contemporary oil politics reveals a pattern in the control of oil in the Middle East as functional to the power politics of the day. In this regard, he focuses on the major forces and issues concerning oil power and deals with the three-way jousting among the oil-producing countries, oil companies, and consumers.

Al-Sowayegh indicates that today's energy crisis in the world was precipitated, but not caused, by the Arab-Israeli War of 1973 and the resultant embargo. The geometric growth in demand for energy in the world had drawn far too little attention as the finite supplies dwindled. When the Arabs reduced production and imposed a world-wide oil embargo they also, as Al-Sowayegh points out, stepped up their efforts through diplomatic channels to have Israel withdraw from the Arab-occupied lands. Since then, the Middle East has continued to be the battleground among the powers of the world.

In an attempt to vindicate the Western charges that the Arab oil "embargo contradicted every existing economic rule" (p. 136), Al-Sowayegh abandoned his own position by saying that

"although the cutback in oil exports was exclusively a political decision, oil price rises were a mixture of both political and economic factors" (p. 136).

A fear of Israel and a desire to prevent radical incursion into the Gulf region prompted the Arab countries to emerge from their insularity by joining the militant Arabs in the use of oil as a weapon in the Arab-Israeli-Palestinian conflict. This is patent when Al-Sowayegh concludes his study by saying that "the main threat to the Arab nation is Israel, and for this reason the Palestinian question will remain the most important security issue for the Arabs and the world" (p. 193).

Despite imperfections, Al-Sowayegh has written an easily understood account of Arab oil politics that should be useful for those wanting to know more about the Arab view of Middle East turbulence and the world oil crisis.

SHEIKH R. ALI

North Carolina Central University

African Security Issues: Sovereignty, Stability, and Solidarity. Edited by Bruce E. Arlinghaus. (Boulder, Colo.: Westview Press, 1984. Pp. x + 229. \$25.00.)

The majority of the chapters in this volume stem from a panel on Inter-African Security Issues at the 1982 Annual Meeting of the African Studies Association. Three major security issues are addressed: (1) sovereignty, that is, the "ability to achieve economic and political self-sufficiency free from foreign or domestic interference"; (2) stability, "the creation and maintenance of a viable political environment in which orderly and progressive social and economic change may take place"; and (3) solidarity, "the development of intraregional interdependence and cooperation to foster both sovereignty and stability as well as a more significant and beneficial role for Africa in world affairs" (p. ix). These clearly are three crucial issues, although the point about "foreign or domestic interference" in the context of national sovereignty seems somewhat muddled, and the topics of interregional interdependence and sovereignty are somewhat at odds.

The sovereignty issue is examined by Arthur J. Klinghoffer in "The Soviet Union and Superpower Rivalry in Africa," and by I. William Zartman in "Africa and the West: The French Connection"; Wayne A. Selcher and Pauline H. Baker present studies of Brazil-Africa relations and of Nigeria. A contradiction emerges. On the one hand, the suggestion is strongly advanced by some of these authors that superpower rivalry

somehow is imposed upon unwilling countries anxious to preserve their sovereignty. On the other hand, Arlinghaus reminds us (p. 2) that if African leaders opt "to mortgage their future for a modicum of security today, it . . . is *their* decision to make." Precisely. Moreover, the immense and spreading corruption and the mismanagement accompanying arms transfers, neither classifiable as security measures, are not sufficiently reflected in this volume. One must also question whether fulfillment of genuine security needs from superpower arsenals is as dire a threat to African sovereignty as several of the authors seems to assert. Were not Soviet military and civilian personnel unceremoniously expelled from at least three African states whose armed forces had been built up with Soviet assistance?

Ambassador Deng, noting the disutility of the OAU Charter when it comes to African security, contends that the drain on scarce resources caused by security needs inevitably leads to diminution of national sovereignty. Does that necessarily follow? But Deng is correct: The OAU Charter definitely does not protect the sovereign independence and national security of a steadily increasing number of African states. Klinghoffer, in a very informative chapter on Soviet interests and motivations, finds that Moscow's tactics in Africa are "reactive rather than primary" (p. 21), a point shared by numerous observers throughout the world, but not necessarily accepted by Washington.

In a richly documented, somewhat theoretical contribution, "Social Incoherence and the Mediatory Role of the State," Donald Rothchild draws attention to the wide range of options beleaguered governments actually have when security requirements must be met from scarce resources. Treating the military as a political force capable on occasion of genuine ideological concerns, Claude Welch examines the bearing of civilian-military relations on political stability. Victor Olorunsola and Dan Muhwezi address "Security and Stability Implications of Ethnicity and Religious Factors." Their finding of the now-familiar fact that "ethnic heterogeneity in some countries is not conducive to stability" (p. 154) does not seem worth the effort. Likewise, it is also well known that "intense competition for power and public goods and the often resulting maldistribution of those scarce resources have created conditions that may easily lead to conflict" (p. 154). Is there a country on this planet where that is not the case?

On the subject of African solidarity, Annette M. Seegers sees "South African Liberation" as the "Touchstone of African Solidarity." Is it really? To her credit, Seeger recognizes that agreement among African states at the level of rhetoric

does not necessarily spell agreement on precisely how or when that liberation is to be achieved. Finally, D. Katete Orwa, in "National Security: An African Perspective," concludes that a prime threat to the national security of a growing number of African states is the growth of their armed forces. Pessimistically, he foresees that the pressure emanating from that direction may overwhelm what conflict resolution capability the OAU may have left in its progressively weakened state. From this, as he sees it, arises an urgent need for return to professionalism in domestic and foreign policymaking, in particular a turning away from the "non-professionals" (p. 210), presumably politicians "in and out of uniform." A faint voice in an expanding wilderness.

On balance, given the global strategic implications of security and stability on the African continent, this volume is a welcome addition to the growing body of national and international security studies.

HENRY L. BRETTON

State University of New York College at Brockport

Arms for Africa: Military Assistance and Foreign Policy in the Developing World. Edited by Bruce E. Arlinghaus. (Lexington, Mass.: D.C. Heath, 1983. Pp. xiv + 233. \$26.95.)

For the past 10 years, since the publication of Robert Harkavy's *The Arms Trade and the International System* (Ballinger, 1975), there has been a small but steady stream of books examining the arms trade, particularly as it involves major power suppliers and third-world recipients. Relatively few authors, however, have explicitly addressed the extent to which governmental arms transfers serve as an instrument of foreign diplomacy, particularly through providing the supplier with direct or indirect influence over recipient state actions and attitudes. Equally rare is a focus on transfers to sub-Saharan African states.

The importance of this edited volume, which began in 1981 as a panel at the African Studies Association meetings, is that the authors of the individual chapters have taken on the challenge of these two tasks. Included are six case studies of arms transfers policy—Soviet (Edward J. Laurance), Eastern European (Roger E. Kanet), Chinese (George T. Yu), French (Edward A. Kolodziej and Bokanga Lokulutu), German (Regina Cowen), and United States (Joseph P. Smaldone)—as well as more general discussions by David E. Albright and Cynthia Cannizzo and an analytic framework set out by Arlinghaus in the introductory chapter. Not all the chapters are equally enlightening; however, on balance this is

an interesting and worthwhile contribution to the arms transfers and foreign policy literatures.

For the most part, the chapters are solid descriptions of the motivations of suppliers in providing military assistance to African countries. Several contain brief discussions of specific supplier-recipient relationships (e.g., China/Tanzania; USSR/Zimbabwe; France/South Africa); many report the extent and types of military assistance provided by supplier states. The balance between "push factors" and "pull factors" is also addressed.

In addition to examining supplier incentives, several authors speculate on the probable future of arms transfers to Africa. Conclusions are mixed; however, the general consensus is that although African states will continue to import arms, the rapid growth in acquisition which was evident in the 1970s has probably ended, restrained by a combination of domestic variables such as high interest rates, the high level of African debt, the problem of underdevelopment, a slight lessening in racial conflict in Africa (which was part of the reason for early arms transfers), and the possibility of a Pan-African Defense Force. In addition, there is likely to be a reshuffling of suppliers, with secondary European states and third-world countries gaining a greater portion of the market than was true five or 10 years ago.

Although there is much that is valuable in this collection, the book is not flawless. One weakness is that several of the chapters do not directly focus on the supposed theme—the question of influence—but instead limit themselves to the far simpler task of general discussions of supplier patterns and motivations. A greater degree of "connectedness" among the separate pieces would have been helpful. A more serious disappointment is the very limited use of quantitative research on the issue of arms and influence. None of the chapters breaks out of the descriptive/analytic mold, and only rarely do the authors even report relevant empirical studies conducted by others (Smaldone being one exception). To be sure, problems with obtaining reliable arms transfer data makes such research more difficult; nonetheless, at least some attention to the insights of this mode of analysis would have strengthened the book considerably. Despite these reservations, this book is well worth the attention of scholars of arms transfers and foreign policy, for whom it should stimulate further debate and research on a much needed general theory of arms transfer policy.

DEBORAH J. GERNER

Hamilton College

Linkage Politics in the Middle East: Syria between Domestic and External Conflict, 1961-70. By Yaacov Bar-Siman-Tov. (Boulder, Colo.: Westview Press, 1983. Pp. ix + 176. \$18.50, paper.)

Bar-Siman-Tov examines connections between Syria's external and internal conflict through his development of Rosenau's concept of "linkage politics." In this study, Syria's "linkage groups" have ties to one or more of her neighbors and affect the relationship between their state's external and internal conflict (e.g., increased domestic conflict leads to increased foreign conflict).

The period of Syrian politics under study runs from the September, 1961 breakup of the Egyptian-Syrian Union to the Asad assumption of power in November, 1970 and is subdivided into three segments: the Separatist regime from 1961 to 1963; the Ba'th regime from 1963 to 1966; and the neo-Ba'th regime from 1966 to 1970. During this period, Syria was a polity with chronic severe political instability whose governments either created or sought to use existing or imagined foreign conflict in order to reduce their country's domestic conflict, although usually with little success. This study gives no support to leaders who hope that conflict abroad will bring long-term unity to a badly divided country.

Bar-Siman-Tov argues persuasively for concentrating on a single state as well as for relying on a blend of quantitative and qualitative (historical) analysis when studying external-internal conflict relationships and their connection with linkage groups. Bar-Siman-Tov's use of Syrian documents, prior research, and press and radio accounts provide an apparently adequate factual base for his descriptions of event patterns and delineations of group activities. Although specialists on Syrian politics may question some of his points, impressive evidence is presented for his four variants of the external-internal conflict relationship as well as for the critical role played by linkage groups in them. After reading this book, the results of the multinational quantitative studies that show little or no relationship between external and internal conflict appear even more dubious than ever. Scholars interested in external-internal conflict and/or linkage politics should pay careful attention to this study.

Nevertheless, this short volume is mildly disappointing. Despite a solid although not very original critique of the lack of theory in the multinational, quantitative studies, Bar-Siman-Tov has created what is closer to a descriptive research outline than a well-developed theoretical framework. His "proposed method of research" chapter not only lacks explicit hypotheses for future testing, but also appears to have been

created at least as much to suit his Syrian example as for its future utility. Furthermore, without evidence, he brushes aside as hopelessly impractical even the possibility that the conditions that determine the links between external and internal conflict can be stated beyond the point he has reached. Although this book correctly identifies and intelligently discusses a number of the elements of a possible theoretical framework, it is not as developed in theoretical terms as it could have been.

Also, it is regrettable that apart from a discussion of Syrian politics and, to a lesser extent, of Egyptian-Syrian relations, the material is not used to shed much additional light on Middle Eastern politics. Thus, this volume contains relatively little to attract persons with a general interest in the area.

Finally, the last chapter is perfunctory and presents only a brief, sketchy summary of points dealt with earlier. It appears to have been added for the sole purpose of giving the book a formal conclusion.

Despite these limitations, this is a valuable, original study. It should be noted that it contains an index and complete footnotes, but no separate bibliography.

JOSEPH M. SCOLNICK, JR.

Clinch Valley College, University of Virginia

The Politics of Nuclear Balance: Ambiguity and Continuity in Strategic Policies. By William H. Baugh. (New York: Longman, 1984. Pp. xii + 276. \$27.50, cloth; \$17.95, paper.)

William Baugh has written a valuable road map for the reader interested in understanding the welter of indexes, statistics, and models that mark the intellectual landscape of the nuclear arms competition. These measures comprise what is typically termed the strategic balance. The analytic elegance of these techniques, however, frequently masks a far more vital question. Can we accurately ascertain an imbalance in the relative performance and strength of the Soviet and American arsenals? The answer is no. The reason, according to Baugh, is the lack of agreement "either within or between governments on how it [the balance] is to be measured" (p. 2).

Baugh examines the range of methods with clarity and insight. Beginning with simple measures such as numbers of weapons and total megatonnage, he proceeds to explain such performance parameters as silo hardness and warhead accuracy. He then turns his attention to various figures of merit, for example, Total Equivalent Megatonnage (EMT), a gauge of area destruction,

and Hard Target Kill Potential, a calculation of the vulnerability of missile silos to attack. Finally, Baugh critiques various nuclear effect models used to calculate the number of residual weapons surviving a notional nuclear exchange. Such models offer a dynamic way of testing weapon performance and alternative targeting strategies. Of the three models discussed, NEMATODE 2.2 was designed by the author and serves as the basis for his subsequent examination of the growing vulnerability of land-based ICBMs to a preemptive attack. Baugh concludes, on the basis of his calculations, that the "window of vulnerability" that opened for the United States in 1980 and 1981 will open for the Soviet by mid-decade even without the MX. Significantly, this judgment only makes sense within the context of a counterforce scenario. In Baugh's view, there is no "open window" for either superpower in maintaining some minimum level of deterrence.

The value of this book is less certain for those specialists in the field who are interested in the mutual interaction between Soviet and American nuclear strategy. The reason can be traced as much to Baugh's conceptual approach as to his almost complete reliance on secondary sources for his analysis. His approach is tethered to an elementary exercise in game theory—the prisoner's dilemma. In this highly simplified game of strategy, two opponents are more likely to pursue a new weapons program rather than practice restraint because neither trusts the other and each wants to cover every contingency. The logical outcome of this interplay between fear and technology would seem to be a growing convergence in the strategic doctrines as well as the force postures of the superpowers. In Baugh's words, "having reached what is generally regarded as rough nuclear parity, the two superpowers have gravitated to the same region of doctrinal space" (p. 74). In support of this proposition, he cites a 1971 essay by Roman Kolkowicz. In fact, Baugh relies almost exclusively on this essay whenever he attempts to explain the Soviet perspective. The proposition that fear and technology has led to doctrinal convergence may be valid, but the assertion demands far more evidence than is presented in this book. The absence of a well-researched discussion of the Soviet perspective on military doctrine and strategy leads Baugh to erroneously ascribe an "evolved [triad] doctrine" (p. 87) to the Soviets and to ignore the differences in the Soviet and American approaches to the problem of civil defense.

Baugh concludes his book with a speculative look at the future prospects for arms control. He envisages a series of small cooperative steps as the most profitable approach to the problem of reducing the nuclear threat. Regardless of the

magnitude of the step, progress will depend upon some semblance of agreement between Moscow and Washington on the nature of the strategic balance. This will require an even greater effort to understand the Soviet calculus for the nuclear equation.

PAUL MICHAEL KOZAR

Washington, D.C.

Thinking about National Security: Defense and Foreign Policy in a Dangerous World. By Harold Brown. (Boulder, Colo.: Westview Press, 1983. Pp. ix + 288. \$16.95.)

This is a thoughtful and persuasive analysis of American national security policy. Although occasionally technical, the book is designed for the general audience interested in foreign and defense policies.

Brown attempts to cover 16 issues in 280 pages, so some of the analysis is rather sketchy. But the strengths of the book clearly outweigh its weaknesses.

Throughout the book, Brown effectively expands upon his definition of national security: "Security must depend on the nation's internal political and economic strength; the will of the people and their ability to persevere in a given course; the quality of U.S. education and technology; the state of national leadership; and the degree of confidence the public has in that leadership" (p. 262).

One crucial issue that has impact upon the security of the industrialized democracies is the crisis of political authority. Brown attributes the cause to the attitude that peace and steady improvement in living standards are inevitable. "This attitude makes the industrialized democracies fragile in the face of adversity, and a lack of resolve could well erode the willingness of their people to make sacrifices to preserve the democratic system and the market economy" (p. 25). This argument is persuasive as far as it goes, but surely the issue is more complex than that. Other causes relate to the dispersion of political influence, increasing inefficiency of government, citizen alienation, and the internationalization and externalization of policy responsibilities.

Brown's discussion of the all-volunteer force and the possibility of conscription is thought-provoking. He argues that: "It is important to begin working now on plans for possible substitutes for the all-volunteer forces, especially those that would involve obligatory service, to ensure that they meet concerns about equity, universality, constitutionality, and need. This will enable us to avoid unfairness and mass divisive-

ness should such a program prove necessary" (p. 259). To those who propose universal service for young men and women, Brown suggests a "defense tax," which would impose some sacrifice upon the entire population and would help to justify to young people the service required of them.

Brown supports one line of reasoning that is clearly popular, but it seems quite problematical. That is: "A military balance viewed by the U.S. Government and public or by U.S. allies and friends as unsatisfactory—or even as becoming less favorable—raises the possibility of explicit or implicit Soviet political intimidation" (p. 265). Perhaps this is characteristic of many "truisms" in political science. But such generalizations are difficult to prove in concrete circumstances, and their use is primarily polemical.

ROBERT H. PUCKETT

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South Africa in Southern Africa: The Intensifying Vortex of Violence. Edited by Thomas M. Callaghy. (New York: Praeger Publishers, 1983. Pp. viii + 420. \$45.00.)

For roughly the past quarter century, South Africa has attracted international attention, as well as opprobrium, primarily because its ruling elite attempted to divert what British Prime Minister Macmillan, in his address to both Houses of Parliament in Cape Town, referred to as the "winds of change." This oligarchy has proven resilient, innovative, resourceful, and willing to use coercion to sustain its style of privilege and affluence which it masks either in terms of Christian guardianship, or in Kiplingesque phrases concerning the trusteeship responsibilities of the white body politic. Its detractors regard the regime as the apotheosis of racism, or even the best friend the Soviets and their allies ever had in terms of a seedbed for disaffection and possible cataclysmic revolution. Those who find the Manichaean approach unappealing look at the regime in Pretoria as a modernizing oligarchy, or as a regime which faces, and can contain, low-intensity conflict. Others operate in a larger continental or global frame of reference.

In the fall of 1980, Pennsylvania State University sponsored a conference on Southern Africa just before the annual meeting of the African Studies Association in Philadelphia. The timing enabled the University to attract a wider group of participants than might otherwise be possible. Thomas Callaghy undertook the arduous task of editing the conference papers for publication. It is not clear from the Preface (p. v) whether all of the

papers presented at the 1980 conference are included in the edited volume. Callaghy divides the book into two parts, the first titled "Regional Orientation and Impact," the second, "Black States and Groups in Southern Africa." The authors are well-known, established Africanists, most of whom are teaching in American universities.

Not surprisingly, the bulk of the 14 essays concentrate on one nation-state, or on bilateral relations between South Africa and one or more of the states, homelands, or territories in the region. Some of the essays treat the region as a whole, for example, the Southern African Development Coordination Conference (SADCC), or deal with nonstate actors, such as the South African liberation groups that operate primarily from exile havens against the South African regime. The great strength of the volume lies in the superior quality of editing, the fastidious documentation, the skillful juxtaposition of carefully assembled data, and of the pertinent theoretical perspectives and literature.

Although it would be banal to review each separate chapter, it is possible to draw attention to some novel approaches or challenges to conventional wisdom about Southern Africa. Jeffrey Butler writes an exceptionally exciting historical overview of the region and demonstrates very cogently the utility of longitudinal analysis. This in itself has great heuristic value. Larry Bowman, Michael Bratton, and Rukudzo Murapa provide a remarkably lucid demonstration of how to disaggregate the concept of dependence to ascertain the nature, as well as the direction, of change of the asymmetrical pattern of linkage. Their sophisticated analysis furnishes the requisite dynamic element that is often missing from the more flamboyant and deterministic application of this analytical device to third-world situations. Finally, Callaghy draws attention to the fuzziness of the boundaries of the Southern African regional system and points out how Mobutu's Zaire has been included, as well as excluded (in the case of the SADCC), in the turbulent politics of the 11-member subsystem.

RICHARD DALE

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Force and Statecraft: Diplomatic Problems of Our Time. By Gordon A. Craig and Alexander L. George. (New York: Oxford University Press, 1983. Pp. xiv + 288. \$19.95, cloth; \$9.95, paper.)

This eloquent book begins with a historical overview of the international system from the

emergence of the great powers in the seventeenth century to the present, proceeds to an analysis of force as an instrument of statecraft for maintaining and managing the modern international system, and concludes with a discussion of the role of ethics in world politics. Written by an eminent historian and political scientist, this work successfully integrates the two modes of thought to create a coherent whole.

A set of critical interrelated assumptions provides the underpinnings of *Force and Statecraft*. First, Craig and George deem a "viable international community" (with an accepted body of law and custom) to be a central prerequisite for minimizing international violence (pp. ix-x). Second, the requirements for this viable international system are (1) an agreement, based on dominant values, among the principal nations about objectives; (2) a structure tuned to the number of interacting states, the scope of the system, and the distribution of power and status among states; and (3) procedures that nations commonly accept for achieving objectives (p. x). Third, the European concert of powers in the nineteenth century is the "experiment" which has come closest to fulfilling these requirements (p. x). Finally, the collapse of this experiment in the twentieth century results from the nineteenth century system's inability to adapt to the rapid and sweeping changes that have created a "diplomatic revolution" in the modern period (p. xi). These assumptions appear quite reasonable, but may mask controversies about placing such a heavy emphasis on the nature of the international system (Kenneth Waltz's "third image") as an explanation of interstate violence and on accelerating change as an explanation of system failure.

The linkage between force and legitimacy—in terms of feasibility, effectiveness, and acceptability—is at the heart of the book. In the context of discussions of force-related aspects of statecraft—including deterrence, coercive diplomacy, crisis management, and war termination—detente stands out as a divergent approach. The American strategy of detente, which granted more political legitimacy to the Soviet Union and to its sphere of influence and enhanced the spirit of reciprocity between the superpowers (pp. 134-136), ultimately failed due to adverse international events (the 1973 Arab-Israeli War and the 1975 Soviet-supported Cuban intervention in Angola) and to the lower public tolerance for the ambiguous and complex detente strategy compared with the powerful "devil-image" Cold War logic (pp. 137-138). If one accepts the authors' somewhat controversial contentions that the Europeans felt detente was a success (p. 147), that the popular threat-based deterrence approach "is often at best a time-buying strategy" (p. 187), and that threats

of force in general are decreasingly efficient in settling superpower conflict (p. 151), then a superpower attempt to return to legitimacy-based strategies like detente rather than force-based strategies might seem appropriate.

Craig and George conclude their book with a telling reminder that, despite rhetoric to the contrary, policymakers often place expediency—what works—over moral purity (p. 277). With the greater difficulty in the modern period of avoiding military moves which communicate escalation toward war (p. 217), this standpoint can be especially dangerous. *Force and Statecraft* is, then, an important guide explaining the presumed obsolescence of both global community and global morality and posing approaches to the seemingly insuperable challenges confronting modern diplomacy.

ROBERT MANDEL

Lewis and Clark College

North/South Relations: Studies in Dependency Reversal. Edited by Charles F. Doran, George Modelski, and Cal Clark. (New York: Praeger Publishers, 1983. Pp. xx + 257. \$27.95.)

Given the enthusiastic and largely uncritical reception of dependency theory by American social scientists in the early 1970s, it is not surprising that a reaction eventually set in. In recent years even the most sympathetic observers of North-South relations have become uncomfortable with the all-or-nothing character of many formulations of dependency theory, with its lack of attention to practical strategies designed to improve third-world countries' standing, and with the vagueness of its proposed alternatives to the existing international order. In addition, many have called attention to the difficulty of applying the more dogmatic versions of dependency theory to the enormous and growing diversity among third-world countries and to the problems that arise from the inclination of many dependency theorists to, in the words of George Lichtheim, "make imperialism rhyme with capitalism" (*Imperialism*, Praeger Publishers, 1971, p. 39).

The contributions to this collection attempt to come to grips with issues such as these, focusing on the possibility of third-world countries' achieving what the authors call "dependency reversal," which "offsets and transforms some of the imputed consequences of dependency" through processes that "may be at work internal to the developing polity and external to it within interstate relations" (p. xi). As is almost inevitable in an enterprise of this kind, the essays are quite diverse, representing a wide range of ideological

and methodological orientations—in this they are only an accurate reflection of the field as a whole. Aside from their diversity of approach, the essays vary considerably in quality: The contributions of several authors, although creditable, are clearly not representative of their best work.

In many ways the most interesting essay is that of Robert Packenham, who offers a spirited defense of what he calls “analytic dependency,” the analysis of “constraints on national units that may occur under any kind of social system (not just capitalism) and which may or may not have [adverse] developmental consequences” (p. 29). The relationship between power asymmetry and exploitation, in Packenham’s view, is an empirical rather than a definitional issue, and he vigorously defends analytic dependency from the challenge of “holistic dependency” in which “empirical data may be used . . . to support the dependency perspective, but never to question its fundamental claims, which are postulated to be true by definition” (p. 32). In particular, Packenham is wary of dependency theorists’ “utopianism” (p. 34) and their vague appeal to world socialism as an alternative to the present world order: “even if one achieves socialism in one country—no easy task, especially given the demanding criteria that can always be invoked for identifying *true* socialism—it is always possible to argue that the fundamental problems remain as long as that one country operates in a capitalist world economy. . . . Without entering into the question of the desirability . . . of [the achievement of world socialism], one can surely say that it is not very feasible in any foreseeable future” (p. 34).

Also valuable is a long essay by Andre Tiano offering a wide range of practical suggestions to third-world policymakers entering into contracts with foreign firms. Tiano tries, usually successfully, to be both reasonable and realistic in offering advice that he expects will offend committed socialists and committed capitalists alike, but which he hopes will be of value in helping third-world countries cope with the imperfect and inevitably compromise-laden world in which they find themselves.

In line with an important theme in the dependency/world systems literature, a number of the essays consider the global system as a whole and speak in terms of centuries rather than years or decades. One such is that of George Modelski, who applies his familiar “long cycle perspective” to the study of dependency reversal. Modelski’s chapter is interesting, although not everyone will be comfortable with a perspective that focuses on “mega-learning processes” that sound suspiciously like the modernization theorists’ notion of diffusion. Another chapter in this genre is Christopher Chase-Dunn’s analysis of “structural

mobility in the capitalist world economy” in which he charts the movement of a number of countries from periphery to semiperiphery and from semiperiphery to core.

The collection also includes several case studies that are useful in that they make an effort to explore relationships—particularly those involving the Soviet Union—that are not typically examined by dependency theorists. Especially valuable is James Ray’s examination of Cuba, which addresses such questions as whether the Cubans have exchanged their traditional dependence on the United States for an even more pervasive dependence on the Soviet Union and whether the Cuban commitment to equality has come at the expense of economic growth. Also interesting is Cal Clark and Donna Bahry’s assessment of ties between the Soviet Union and Eastern Europe, although their conclusion that Eastern European dependence has declined significantly might have been tempered by the more natural comparison of the present situation with the interwar years rather than with the immediate postwar period. Finally, Ladd Hollist offers a useful, if perhaps overly faithful, application of traditional Marxist notion of imperialism to the evolution of Brazilian agriculture, concluding that although the nature of Brazilian agriculture has changed substantially several times over its history, the “sociocultural, sociopolitical, and economic dynamics of the dependency syndrome” have remained much the same over the centuries (p. 181).

In sum, this collection offers a useful, if hardly seminal, contribution toward conceptualizing the position of third-world countries in an international system dominated by countries that are much more powerful and wealthy. As such, it can be recommended to all who are interested in keeping abreast of current debates concerning the pervasive hierarchy that is perhaps the central characteristic of contemporary international politics.

VINCENT A. MAHLER

Loyola University of Chicago

The Caribbean Challenge: U.S. Policy in a Volatile Region. Edited by H. Michael Erisman. (Boulder, Colo.: Westview Press, 1984. Pp. xiii + 208. \$22.50, cloth; \$10.95, paper.)

Central America and the Caribbean have become a growth industry for publishers; *The Caribbean Challenge* is a topical addition to the literature. The nine authors in this collection examine U.S. foreign policy toward that troubled region, particularly during the first years of the Reagan administration. The book is divided into

three sections: the first contains "macroanalyses" or general overviews of U.S. ideological, economic, and military interests in the region. Part 2 presents case studies of tensions in U.S. relations with four Caribbean Basin nations: Cuba, El Salvador, Mexico, and Jamaica. The work concludes with a reprinted essay by Abraham Lowenthal, an influential analyst of U.S. policy in the region.

These essays serve as a comprehensive introduction to recent U.S. policy moves in the Caribbean. Although some essays are more balanced than others, the authors are uniformly critical of the Reagan administration's policies. Of particular concern to these analysts is the reemergence during the last years of the Carter administration and under Reagan of a foreign policy establishment that views the turmoil in the Caribbean region in stark East/West terms rather than as internally generated by poverty and repression. As a result of this return to cold war assumptions, U.S. foreign policy in the area has become increasingly security conscious and stridently anti-Soviet.

Washington's perception of events in the Caribbean Basin primarily in East/West terms has added stress to already taut relations in the case of several countries. All of the authors agree that Cuba is seen by the Reagan administration as "the Soviet empire's westernmost strategic outpost" (p. 99); but from the Caribbean perspective, Cuba is gaining acceptability as a legitimate actor with its own foreign policy agenda. The contributors are somewhat less in accord on how the United States should proceed with Havana. Josefina Cintron Tiryakian calls for establishing close economic ties, whereas Juan del Aguila cautions that ending the U.S. trade embargo may be a step in the right direction, but "to suppose . . . [it] will make Havana behave is inconsistent with practical realities" (p. 107). In John Booth's analysis of El Salvador, Bruce Bagley's discussion of Mexico, and Jacqueline Braveboy-Wagner's chapter on Jamaica, Washington is also seen as misguided in assuming that all regional problems are inextricably tied to superpower rivalry. Furthermore, because of Washington's heightened security concerns, the Reagan response to regional economic problems is inappropriate. The chapters by Kenneth Boodhoo, Braveboy-Wagner, and Lowenthal give detailed critiques of the Caribbean Basin Initiative unveiled in February, 1982. They argue convincingly that the Initiative reflects primarily the present administration's "interest in military security, political loyalty, and advantages for U.S. firms rather than the region's long-term development" (p. 193).

While *The Caribbean Challenge* will provide a useful introduction to U.S. policy in the Caribbean, this recommendation comes with some

caveats. First, the volume will have a brief shelf life, because it focuses narrowly on the early years of the Reagan administration. Second, the volume has no concluding essay to summarize and expand on contributors' insights; the Lowenthal essay is excellent but does not serve this function. A final concern is for the use of the construct, "Caribbean Basin." The concept has been created by U.S. military planners to meet American strategic needs. This notion, though, seems artificial to the bulk of regional inhabitants—the people of the Caribbean archipelago and Central America who have dissimilar historical and cultural traditions. The artificiality of the "Caribbean Basin" was evident during the Falklands/Malvinas crisis, for example, when the English-speaking Caribbean supported British claims to the islands, while the Hispanic nations of the region backed Argentina. Some analysts, including *The Caribbean Challenge's* editor, believe the Basin construct is useful since the insular Caribbean is becoming more "Central Americanized" or open to U.S. domination and intervention. Evolving relations between the U.S. and the nations of the region will resolve the debate.

SHERRIE L. BAVER

The City College of New York

The Middle East since Camp David. Edited by Robert O. Freedman. (Boulder, Colo.: Westview Press, 1984. Pp. xv + 263. \$28.50, cloth; \$11.95, paper.)

It is in the nature of annual surveys of recent trends and events in any region of the world to do little more than rehash old headlines and perhaps provide a chronological listing useful for future reference. The reader has little right to expect fresh viewpoints and profound analysis. By and large, this collection of eight papers originally presented at a May, 1982 conference sponsored by Baltimore Hebrew College (and later updated to include the Israeli invasion of Lebanon) conforms to these expectations. The contributions, heralded in the Introduction as providing "an encompassing analysis of the major developments in Middle Eastern politics from the Camp David agreements of 1978 until the Israeli invasion of Lebanon of 1982" (p. 5), are not badly done, but neither are they replete with new and exciting insights.

The volume is comprehensive in organization, even if the essays vary considerably in quality. The first two (of three) parts concentrate on superpower and regional perspectives. Freedman's lengthy survey of Soviet policy leads off the book and is followed by Barry Rubin's

summary of the U.S. role during this period. In the second section, Shireen T. Hunter and Robert E. Hunter look at trends in the Arab world, and Robert G. Darius examines recent developments in Iran and their impact on that country's foreign policy. The third part (taking up one-half of the book) consists of analyses of individual actors: John F. Devlin on Syria, Don Peretz on Israel, Louis J. Cantori on Egypt, and Aaron David Miller on the PLO. A brief epilogue by Freedman summarizes developments through October, 1983.

A lot happened in these four years, but the Middle East seems to have survived, despite its much-mentioned "instability" and "volatility." Neither the Camp David approach nor the Iranian Revolution has transformed the Middle East as it once appeared they might. In Lebanon, the fifth Arab-Israeli war left Israel with more occupied territory and deeper internal divisions, underscored the Arab world's inability to act in unison, and added another layer to Lebanon's burden. Despite the initial fears, the Iran-Iraq War did not result in catastrophe but remains locked in a weary stalemate. Far from closing in on the Gulf's oil, the Soviet Union has seen its influence waver and decline. And the United States, despite the setbacks and disappointments in its policies toward the region, is no worse off there than before.

This survey may prove useful as a means of quick review. It may also be usable as a supplementary textbook for courses on the Middle East or international relations, although its advantage in breadth of coverage is offset by the short time-period it considers and the lack of sufficient background to adequately explain events and place them in the proper context.

J. E. PETERSON

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Risks of Unintentional Nuclear War. By Daniel Frei and Christian Catrina. (Totowa, N.J.: Allanheld, Osmun, 1983. Pp. xxxi + 255. \$10.95, paper.)

Nuclear War and Nuclear Peace. By Gerald Segal et al. (New York: St. Martin's Press, 1983. Pp. xii + 162. \$25.00.)

In *Risks of Unintentional Nuclear War*, a study commissioned by UNITAR's Institute for Disarmament Research, Daniel Frei evaluates the chances that nuclear war will occur unintentionally, not from rationally premeditated calculations of strategic or political benefit by adversaries in conflict. He argues that accidental nuclear war is unlikely given the redundancy of modern nuclear command, control and com-

munications systems, and leaders' caution. But unintentional war would be the product of decisions "based on false assumptions, i.e. on misjudgment or miscalculation" by legitimate authorities (p. ix). Human misunderstanding connected with short decision times leads to crisis instability. War could begin at a low level of armed conflict, and miscalculation, paranoia, the imperatives of vulnerable weapons systems, decision-maker stress, or organizational foul-ups could lead unintentionally to nuclear escalation.

The book does not contain new arguments or research. It instructively organizes the observations and conclusions of the relevant literatures of strategic doctrine, game theory, organizational process, cognition, and social psychology that bear on crisis stability. At the end of each chapter, Frei draws conclusions regarding the contributions to the risk of unintentional nuclear war of the factors discussed, and the factors that mitigate such factors. His final conclusions emphasize the most worrisome risks: weapons systems and doctrines that lead to short decision times; doctrines that create the belief by one nuclear power that the other is seeking nuclear superiority, or lacks commitment to its own and its allies' defense; decision processes that exacerbate stress and reduce information availability; and unwise patterns of security commitments by the superpowers. Frei sets out 32 recommendations to mitigate the risks, including some novel ideas. He suggests that the superpowers negotiate a "code of conduct" for their relations in third-world environments and for crisis management. He proposes that negotiations should be undertaken to establish new means of arms control verification, perhaps based on the experience of the International Atomic Energy Agency's nuclear safeguards system.

The Frei book, *Risks of Unintentional Nuclear War*, is a comprehensive discussion, based on familiar but sound materials, of crisis instability in the nuclear strategic context. Its methodical combination of technological and human factors is its innovative contribution to the nuclear literature. Its synthesis of important literatures is excellent. The final recommendations are creative, important, and sensible. It would be useful to have a subject index, but the text is copiously referenced and the sources are listed in an extensive bibliography.

The authors of *Nuclear War and Nuclear Peace* have struck an elegant blow for the "radical centre" against attacks by both right and left on Western nuclear strategic thinking, NATO, and arms control efforts. The primary target of this well-integrated set of essays is the Campaign for Nuclear Disarmament and its European and U.S. analogues that variously support denucleariza-

tion, nuclear freezes, nuclear free zones, decoupling the United States from NATO, and unilateral disarmament. The authors argue that because nuclear weapons technology cannot be "unlearned" and that it is employed in support of political objectives, realism requires involvement in the messy business of making the nuclear world a comparatively stable one. Nuclear weapons are not the source of East-West discord, but are central to the military confrontation that emerges from it.

The authors establish the morality of a sensible "centre" policy by discrediting the moral arguments of the extremes. The Right claims that new technologies such as "star wars" could enable the West to fight a war against weapons, not civilians. The Left argues that nuclear weapons are immoral because the threat of their use is morally tantamount to their use. The authors argue that it is more moral to pursue strategies that effectively deter the use of nuclear weapons than it is to follow either the Right or the Left's potentially destabilizing prescriptions.

Segal points out the basic asymmetry of U.S. and Soviet nuclear doctrines—the United States basing deterrence on assured destruction while the Soviets stake their survival on the deterrent effect of a credible war-fighting capability. But he argues that both can be made more secure through arms control and the deployment of appropriate, less vulnerable, weapons. Edwina Moreton argues that arms control is useful regardless of the state of U.S.-Soviet relations, and for arms control agreements that lead to greater strategic stability. She calls for reexamining the sanctity of the United States strategic triad and considering elimination of land-based ICBMs. Lawrence Freedman reminds us that emplacement of Pershings and GLCMs in Europe was a response to European fears of the United States decoupling from NATO, and maintains that their deployment is preferable to decoupling or to maintaining existing levels of battlefield tactical nuclear weapons as symbols of U.S. commitment. John Baylis argues, in a more narrowly focused essay, that a British nuclear deterrent is preferable to unilateral disarmament or precipitous moves in the direction of either a consolidated European nuclear force or total reliance on conventional weapons.

Nuclear War and Nuclear Peace is a stimulating and thoughtful work written in an engaging and succinct style. The book's "radical centre" position could serve admirably as a basis for late 1980s strategic and arms control policy.

BENJAMIN N. SCHIFF

Oberlin College

The Voice of the Poor. By John Kenneth Galbraith. (Cambridge, Mass.: Harvard University Press, 1983. Pp. v + 88. \$8.95.)

If one keeps in mind that this book is based on a set of lectures and is not meant to be a complete argument, it can be read with great profit. Because Galbraith makes a number of major implicit assumptions, the book must be read as a starting point, not as a definitive statement.

Galbraith's wholly admirable purpose "is to suggest some of the things which . . . rich countries should be hearing from the poor" (p. 3). Galbraith's first major assumption is that all poor countries would want to say the same things, at least approximately, to the rich countries. Although this is clearly not true, it is benign for the purposes for which it is used. However, a second assumption is less benign: Any given poor country speaks with one voice, and any internal conflicts are, for this purpose, less important than are the commonalities within the poor country. Current events in Lebanon, El Salvador, and elsewhere certainly seem to belie this. For example, one can certainly imagine that the poor members of a poor country, but not the rich members, might like to say to any rich country: Why should we take your advice when there are numerous poor people in your country? Galbraith appears to have fallen into a common oversimplification: In poor areas almost all are poor, and in non-poor areas virtually none are poor.

This book is composed of four main chapters. In the first, Galbraith wants to remind everyone that a country's historical situation places constraints on what it can do. Galbraith claims, correctly, that both the United States and the USSR ignore this, and also ignore their own histories. Thus, these two countries give advice and push programs and practices that are bound to have unintended consequences as well as failing to achieve their explicitly intended consequences. Although both correct and stated very well, Galbraith ignores two points and makes an implicit assumption about a third: Today's poor countries are in a different situation than today's rich ever were; there are some rich countries today, and it may be possible to learn from them. Do the United States and the USSR actually intend beneficent consequences for the poor countries when they give aid and advice? Certainly this can at least sometimes be doubted.

Galbraith generally appears to mean economic growth by "development," but it may be more appropriate to distinguish development from growth according to who benefits, with development implying a more equitable distribution of income.

Chapter 3, the second major chapter, deals with the modern form of imperialism: influence. Galbraith argues that this is bound to fail because "the will to national independence is the most powerful force in our time" (p. 36) or, "the unbounded and universal determination of people everywhere to govern themselves" (p. 42). This and the preceding chapter would have benefited from less independence. The obvious question is, What is a nation? A historical look at the results of the old version of imperialism convinces one that many nations exist at least partly because of the old imperialism. For example, in Lebanon, political offices are guaranteed to certain religious groups, while others are frozen out. This, of course, does not excuse the United States and the USSR for their acts or intentions, but it does make answers more complicated.

Chapter 4 concerns the military relations between the rich and poor countries. Galbraith is correct that these relations hurt the poor countries in several ways.

In his final chapter, Galbraith switches perspectives to discuss the domestic policies of the rich countries. Again, he is correct that reliance on monetary policies in the United States results in high interest rates that draw money to the United States from other countries. The results of this migration are not going to be helpful to most residents of poor countries, although those who are already rich may become even richer.

Throughout, Galbraith writes with grace and wit and in a manner likely to provoke thought rather than stifle it. The result is a book well worth reading even by those mainly concerned with domestic politics and policies.

RICHARD GOLDSTEIN

Brighton, Massachusetts

The Pharmaceutical Industry and Dependency in the Third World. By Gary Gereffi. (Princeton, N.J.: Princeton University Press, 1983. Pp. 291. \$25.00, cloth; \$9.95, paper.)

Gereffi has labored long and intensely to gather data on the operations of pharmaceutical companies in Mexico, South America, and even Sri Lanka. He has been able to transpose into readable English the technical jargon of medicine manufacturing so that the details about tranquilizers, sedatives, and analgesics and their significance to the world market become readily understandable.

Because the developing world's population is often not within walking distance of health clinics nor has adequate purchasing power to supplement welfare medicine with their own purchases, an

analytical look at third-world pharmaceutical manufacturing is indeed useful and helpful to those trying to gauge that facet of national development.

Gereffi takes several legitimate cases and somewhat documents his hoped for political indictment, however from that he draws generalizations that do not hold globally. He strives to fit his field data and analyses into dependency theory; technology transfer benefits are not cited.

Modernization theory became established in the social sciences after World War II as third-world nationalist movements emerged. In the 1950s and 1960s, such studies compared the validated norms of the market place among Western democracies and underdeveloped nations. In the 1960s and 1970s, a new generation of social scientists promoted a rival theory of dependency, an amalgam of Marxist and other-variety socialist perceptions. With a framework traced back to Lenin's works, by the 1970s a new network of academicians had out-published the modernization theorists, not in quality but in quantity.

Dependency theory contends there is always a drain of economic surpluses from the less-developed nations to their foreign investors. Such a position ignores postwar expropriations. More important, dependency theorists continue to ignore the return of foreign profits in elaborate infrastructures that nationalistic regimes usually then nationalize, and the huge foreign debts to Western democracies that will never be repaid, whether they are owed to private banks, governments, or combinations of capitalist creditors.

In examining the Mexican industry that manufactures steroid hormone medicines, Gereffi focuses on Syntex, beginning in the 1950s when there was a corporate debate about whether Syntex should fundamentally produce primarily intermediate steroids or whether it should enter the finished products markets. Corporate leaders decided to market some finished products made in Mexico. Gereffi asserts that the decision had something to do with U.S. Senate antitrust hearings, but the company's operations were located entirely in Mexico at the time (p. 109). It was not until 1959 that Syntex moved to Palo Alto, California.

On page 110, Gereffi charges that, to avoid taxes, Syntex produced finished drugs in its plant in Puerto Rico. However, Puerto Rico is part of the United States. The Commonwealth gives tax advantages to encourage mainland industries to open branches on the island, but semifinished drugs from Mexico that received final processing were and are subject to taxation on the added value.

Since 1973, Mexico has succeeded in reducing its annual population growth from the disastrous

3.6% in 1976 to 2.9% in 1983. More than 7,000 Centers for Family Planning distribute free contraceptives to Mexican women. In his eagerness to indict multinational corporations, Gereffi does not mention the expense these foreign firms incurred in Mexico in training Mexican chemists and the expenses involved in pioneering the manufacture of the pill in Mexico by Syntex, Ortho, and Parke-Davis, and in phasing out sales to Mexican concerns. That doesn't fit the dependency theory, but it certainly affirms the older developmental theory.

Nevertheless, a careful reading of this volume will yield numerous useful capsule histories of the political advantages and disadvantages that beset pharmaceutical concerns in Latin America. The basic scholarship on government-corporation relationships is excellent. Some of the generalizations drawn to fit theory, however, deserve a caveat.

MARVIN ALISKY

Arizona State University

The United States and Mexico: Patterns of Influence. By George W. Grayson. (New York: Praeger Publishers, 1984. Pp. xvii + 215. \$27.95, cloth; \$13.95, paper.)

This book is part of the series "Studies of Influence in International Relations" edited by Alvin Z. Rubenstein. Influence is "demonstrated when either country affects through nonmilitary means, directly or indirectly, the behavior of the other in a manner designed to achieve a policy advantage" (p. 2). Influence is a derivative of power, but influence is situational and short-lived, whereas power is latent, continuing, and relative. Grayson never makes the difference clear, and it is critical to an understanding of U.S.-Mexico relations because power determines the nature of influence. Grayson contributes to this analytical confusion in the introductory chapter when he discusses seven problems in analyzing influence patterns in U.S.-Mexico relations that make such analysis very difficult. And in the Conclusion he adds, "Smoke from dozens of sources obscures the view of influence patterns in U.S.-Mexican relations" (p. 193). In truth, there is little attempt to be systematic in applying the analytical framework. A list of six questions (p. 6) to be given special attention to determine why a given policy outcome occurred is hardly referred to in the text. The text itself treats the historical relationship between the two countries, including chapters on the Echeverria and Lopez Portillo years, with the chief focus on oil and gas, then discusses the role of Mexico in the Central American crisis, the failure

of Mexico to join GATT, the continuing problem of immigration, and the economic crisis of 1982. There seems to be a conscious effort to stay away from two concepts used by many analysts in approaching bilateral relations, dependence, and interdependence. Grayson mentions dependence, but there seems to be an aversion to employing the concept of interdependence, which would be most useful in the case of the debt crisis of 1982. His view is that the debt crisis increased Mexican dependence, but a more realistic view would be that it highlighted the interdependence between the two countries. It should be added that several bilateral problems, such as maritime, border, and foreign investment issues are not discussed. Although membership in GATT is mentioned, very little is said about other trade issues that could well be the central bilateral issues of the 1980s. Grayson begins the book with the statement, "The United States relationship to Mexico is more critical to its vital interests than any other bilateral ties, with the possible exception of those with the Soviet Union" (p. xi). What seems to justify this incredible assumption is a sense of impending doom for Mexico and its relations with the United States; the book is replete with this sense of catastrophe about to happen, for example, Grayson concludes that a peace accord that permitted thousands of anti-Sandinistas to resettle in the United States could alter the outlook for effective immigration reform (p. 166). He is no sympathizer with illegal immigration; in discussing the Simpson-Mazzoli bill, he adds, "Chicano leaders, encouraged by Mexican politicians, have even excoriated this modest congressional initiative on the grounds that it will accentuate civil rights abuses and produce a laterday Third Reich" (p. 166)—hardly a fair assessment of genuine concern for civil rights on the part of Hispanics. In conclusion, primarily because of its fuzzy analytical framework, this book is not a major contribution to the literature on U.S.-Mexico relations.

C. RICHARD BATH

University of Texas at El Paso

The Reluctant Supplier: U.S. Decisionmaking for Arms Sales. By Paul Y. Hammond et al. (Cambridge, Mass.: Oelgeschlager, Gunn & Hain, 1983. Pp. xv + 310. \$27.50.)

The Reluctant Supplier furnishes a very useful descriptive account of selected aspects of post-World War II arms transfers. The authors provide an excellent summary of the role of arms transfers in U.S. foreign policy and the changes successive administrations have introduced. They give a detailed account of the institutional decision-

making process for U.S. arms transfers, including the reforms implemented under President Carter and largely continued by the Reagan administration.

Hammond et al. use extensive interviews with executive branch officials along with primary material to develop a case study of President Carter's failed Conventional Arms Transfer Talks. Interviews with relevant personnel were also used in a study of the 1970s' reduction in the roles and numbers of U.S. military advisors. Finally, the authors present a valuable summary of regional arms transfer data, reporting both agreements and deliveries in constant dollar measures whenever possible.

The authors carefully demolish the claims that arms transfers are accelerating in volume, with the United States leading the charge. Hammon et al. point to factors that promote the demand and supply of arms transfers. However, they emphasize that neither demand nor supply are limitless. Budgetary and foreign exchange problems among others constrain demand, especially among the poorer third-world nations. Supplier states are constrained by the requirements of their own militaries. Additionally, the United States has a history of legislative resistance to arms transfers.

Hammond et al. employ constant dollar measures to show that worldwide arms demand has risen, whereas U.S. transfers, counting grants and government and commercial sales, have remained stable. This conclusion may overstate stability, because the government sources the authors use probably understate commercial sales. Nevertheless, these findings continue the work of other scholars in refuting the "merchants of death" perspective so often found in popular and professional literature. Hammond et al. challenge assertions that world arms transfers are escalating without limit while the United States is massively increasing its own transfers.

There is some imprecision contained in the authors' use of aggregate measures to indicate a rise in worldwide demand for arms. The weapons being bought since the 1960s are more sophisticated and expensive than before. Major arms suppliers are transferring much state of the art technology. An increase in procurement outlays by a government may reflect efforts to replace older arms and upgrade aging inventories, rather than an increase in demand. Such increases may also reflect rising GNPs, resulting in larger state budgets and thus more funds for procurement. In general, this seems to be true of most developing countries as their military budgets increased only slightly as a percentage of GNP between 1967 and 1976, a period that corresponds to the arms demand analysis in *The Reluctant Supplier*.

In general, Hammond et al. support using arms transfers to pursue American policy goals. They are critical of the mid-1970s' reductions in the numbers of U.S. military advisors and the limits put on the roles of military missions. They favor using military advisory groups as advisors and "in country" spokesmen for U.S. policy. They also find fault with the Carter administration's arms control efforts. They seem more comfortable with the Reagan administration's approach to arms transfers, although they are mindful of the uncertain gains such efforts can bring. Indeed, arms transfers can have many contradictory impacts on relations between states. However, the authors contend that great powers cannot long abstain from using such a widely desired commodity to gain influence and pursue their own goals. They provide an eloquent statement of a mainstream perspective on arms transfers. They take most established U.S. foreign policy goals as givens and focus on modes of realizing them.

This mainstream orientation leads to little emphasis being given either to the Third World or the implications that new, highly lethal weapons have for levels of destruction and regional power asymmetries. New weapons also require even greater infrastructure investments, skilled labor, and supporting industries simply for maintenance, to say nothing of coproduction. These requirements hardly match the economic development strategies most appropriate to third-world resources and social needs. Instead, a "dependent militarism" evolves with developing nations ever more skilled labor and technology. The authors give little attention to the effects arms transfers have on third-world populations and economies.

Finally, Hammond et al. single out Andrew Pierre for an unfair and hyperbolic attack. The authors imply that in his *The Global Politics of Arms Sales* (Princeton University Press, 1981) Pierre uses only current dollar figures. This is not supported by the pages they cite. The attack is aimed at Dr. Pierre's analysis, but comes closer to being an *ad hominem*.

M. STEPHEN PENDLETON

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The Bar Kokhba Syndrome: Risk and Realism in International Politics. By Yehoshafat Harkabi. Edited by David Altshuler. Translated by Max D. Ticktin. (Chappaqua, N.Y.: Rossel Books, 1983. Pp. xvii + 206. \$15.95.)

Between 132 and 135 A.D., the Jews of Judea led by Bar Kokhba revolted against the occupying Roman authorities. Although it was brutally crushed by the militarily superior Romans, Bar

Kokhba's rebellion has been mythologized in contemporary Israel as a symbol of heroism worthy of emulation. In his provocative and enlightening work, Yehoshafat Harkabi debunks this myth and analyzes the implications for international politics in general, and for Israeli foreign policy in particular, of what he terms the "Bar Kokhba Syndrome"—the blind glorification of rebelliousness and heroism detached of responsibility for, and oblivious to, their outcomes.

Challenging the popular view that the uprising was successful, Harkabi argues convincingly that the rebellion, judged by its ultimate consequences, was an unprecedented disaster because it caused the death of 580,000 Jewish soldiers, led to the exile of the Judean inhabitants, and transformed the Jewish people from active subjects to passive objects of history. The revolt ended in catastrophe, according to Harkabi, because its leaders failed to anticipate the inevitable outcome, did not appreciate the limited nature of their resources, and refused to consider more realistic alternatives that might have averted national suicide.

For Harkabi, the rebellion epitomizes the height of political irresponsibility because "it is never in the national interest to behave suicidally" (p. 64). At the same time, this tragic episode enables him to distill important lessons about the requirements of rational choice and the perils of risk-taking. First and foremost among these is the need to pursue realism in political action. Realism consists of several elements: envisaging the consequences of political decisions, recognizing that decisions often entail a choice between bad and worse options rather than between good and evil outcomes, realizing that dreadful consequences may result from well-intentioned policies, refraining from provocation for which one's adversary may have only one devastating response, and avoiding actions whose success depends entirely on optimal conditions.

Harkabi laments the failure of Israel's political leadership, particularly since 1967, to draw these lessons, decries the tendency to exalt the Bar Kokhba uprising as a heroic episode, and calls for a national self-reckoning based on a more accurate reading of history. He contends that recent Israeli governments have been intoxicated by the Bar Kokhba Syndrome by manifesting a proclivity to excessive reliance on force, downplaying the severe constraints of national resources, and obfuscating distinctions between ephemeral tactical victories and enduring success at the strategic level. Unrealism is also rampant among Israeli doves, who assume erroneously that the PLO and the Arab states have already reconciled themselves to Israel's existence, and it is prevalent among hawks, who refuse to make the necessary ter-

ritorial concessions to moderate the Arab-Israeli conflict. Harkabi concludes that a realistic policy requires Israel to eschew annexation, withdraw from the occupied areas, and acknowledge Palestinian rights to national self-determination, preferably in a confederal arrangement with Jordan.

Although Harkabi deserves praise for an incisive and trenchant analysis of risk-taking and its implications for contemporary international politics, his work is not without shortcomings. In recommending the "Jordanian option" as the optimal solution to the Palestinian question, Harkabi appears to have forgotten his own caveat that lessons drawn from history are more helpful in identifying actions that must be avoided than they are as prescriptions for specific policy outcomes. In addition, the prose is unduly repetitive, and the final chapters are somewhat disjointed. The book is also inundated with typographical errors.

Nevertheless, Harkabi's brilliant exposition of the Bar Kokhba Syndrome is an intellectual *tour de force* and provides original insight on responsible statesmanship and rational choice in both national and international political realms.

MICHAEL RUBNER

Michigan State University

Armed Forces and the Welfare Societies: Challenges in the 1980s: Britain, the Netherlands, Germany, Sweden and the United States. Edited by Gwyn Harries-Jenkins. (New York: St. Martin's Press, 1983. Pp. xiv + 218. \$29.00.)

This is a defense scholars' salvo in the guns-versus-butter debate now annotating the transition from the postwar era to the emerging prewar world. The book's contributors are a half-military, half-civilian, almost wholly European research team sponsored by Britain's International Institute for Strategic Studies. Their message is that the West has reached a critical threshold in civil-military affairs. This crisis results from the strain on peace-lulled materialistic societies because of escalating military materiel and personnel costs imposed by technology and the welfare state. They warn that our thoroughly demobilized welfare states face radical changes in their national defense establishments unless threat perceptions soon rise.

An editor's introduction to pose the issues is followed by competent case studies of British scrimping, Dutch experimentation with military democracy, German conscription problems, the Swedish model of universal military training but

no standing army, and American efforts to maintain a professional military establishment. A postscript spells out lessons from the case studies. The main problems are: (1) the technology-sophistication factor is forcing smaller states to stop weapons system production; (2) the welfare state drive toward equality combines with low threat perception levels to make conscription decreasingly viable; (3) high government and industry pay levels are thwarting military efforts to attract highly qualified manpower; (4) antimilitary attitudes and/or sheer indifference to defense requirements are creating tremendous pressures for change of traditional military structures; and so, (5) projecting expected costs of traditional establishments yields a head-on political collision between national concerns for security and for social justice within a decade.

For these analysts demographic projections are also ominous: The trend of steady and drastic decline throughout the West in the traditional male 18 to 24 age cohort from which to draw essential manpower will continue until the mid-1990s. This trend melds with the technological and other cost factors to reduce each state's ability to maintain strong forces in being. But because technical progress rapidly increases real weapons hardware costs, the quandary comes full circle. The automated battlefield is forcing each West European state to specialize; none can long maintain independent industrial capabilities for land, sea, and air weapons development. The United States may soon consolidate its monopoly of aerospace and high technology weapons. As there arises a new generation unacquainted with war, increasingly Europeans face the prospect of becoming military client states of America and of drifting toward neutralism at the same time. These tendencies combine with welfare state values leading to military unionism, increased soldier pay, regularized working hours and conditions, and lower social status for officers, and so threaten to turn the military profession into just another occupation or—much worse—into a potential scapegoat for hostile public opinion.

Until recently the military establishments enjoyed public indifference and a "splendid isolation" from societal influences, allowing each to operate as a "total institution" (pp. 74, 77). Because this favored status no longer applies, and because in Europe's stagnant economies only hard political decisions can prevent the welfare states' crumbling, challenges to the armed forces' political legitimacy are at hand. Many Europeans simply believe their armed forces cannot provide national security. The authors conclude that it may be impossible to maintain European armed forces in their traditional form and size, and that morale problems may become chronically debili-

tating—however the "men versus technology" debate is resolved. Predictions here are sketchy.

The U.S. chapter shows a marked contrast. America's recently renewed sense of both danger and purpose may reinforce a "fundamental cyclical 'swing' in the public mood back toward another period of extroversion and interventionism abroad" (p. 184). Thus Alan Ned Sabrosky correctly finds America at a turning point of the Klingberg Cycle. This is a 50-year alternation of mood between introversion and extroversion discovered by Frank L. Klingberg and described in his *Cyclical Trends in American Foreign Policy Moods* (University Press of America, 1983). Sabrosky asserts: "American commitments abroad are virtually certain to be maintained as part of a more fundamental reassertion of American activism in foreign affairs" (p. 192). The European contributors do not yet understand this cycle, but it should help solve the problem of low threat perception.

JON ALEXANDER

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Strategic Military Surprise: Incentives and Opportunities. By Klaus Knorr and Patrick Morgan. (New Brunswick, N.J.: Transaction Books, 1983. Pp. vi + 265. \$14.95, paper.)

This book represents an introductory study of a subject that has not received extensive scholarly attention. Using a case study approach, the authors and their contributors, Michael Doyle, Michael Handel, and Richard Betts, have identified some of the key factors that characterize strategic military surprise. The effort is avowedly heuristic, but it also has great substantive value. Although the conclusions are necessarily tentative and qualified, they are reasonable and thought-provoking. This book is a significant contribution in a relatively new field of strategic studies.

The 20 case studies are presented in four chapters. The time frame covers 107 years, from the Austro-Prussian War of 1866 through the Yom Kippur War of 1973. The cases represent a wide range of contexts, including geographical variation (Europe, the Far East, Latin America, and the Middle East) and relative capabilities of attackers and victims (equal and unequal). In all cases, the attacker's "surprise" was the result of at least one of three factors: unexpected initiation of military hostilities (e.g., by Germany in 1914 and Japan in 1941); unexpected extension of military hostilities in the course of an existing conflict (e.g., in the cases of Inchon and Tet); and/or use of an unexpected or new mode of warfare (e.g., the German Blitzkrieg and the 1945 Soviet inva-

sion of Manchuria). A "strategic" attack is defined as one which attempts to "inflict a striking defeat that sharply alters the military situation and possibly determines the outcome of the conflict" (p. 1).

Given these somewhat ambiguous definitions, the case studies, which constitute much of the book, do fit the authors' criteria of strategic surprise. The intriguing question is why any given attack was a surprise, and the answers in this book range from the secrecy of the attacker's plans (as in the case of the use of the atomic bomb against Japan) to the more general and recurrent theme of misestimates, which is stressed in cases as diverse as the Franco-Prussian War and the battle of Dienbienphu.

Despite the value of this book, it has several weaknesses. The authors and contributors do not adopt a common framework, even though one is presented in the introductory chapter. This results in an analysis that at times lacks focus and direction. An appendix cataloging the various hypotheses would have been extremely helpful. Another problem is the lack of any rationale for the selection of cases. For instance, why was the 1940 German attack on Norway included while more obvious cases like Midway, D-Day, and the 1975 North Vietnamese Army offensive were excluded? A third problem is an occasional lack of conceptual clarity. For example, the distinction between an unanticipated attack and an attack that is anticipated as being unlikely is not regarded as significant in terms of "surprise," whereas such a distinction is critically important in decision-making analysis. Finally, there are enough typographical errors to make reading the book somewhat disconcerting.

Despite these problems, this is a useful and provocative book. The last three chapters are particularly important. They summarize the case studies by focusing, respectively, on the attacker's incentive structure, the elements that constitute the opportunity for strategic surprise, and some general prescriptions for policymakers. The pessimistic conclusion is that the incentives and opportunities for strategic surprise will continue to make it a viable and generally successful policy option, despite technological advances in various warning systems and conscious decision-making efforts to assess an adversary's capabilities and intentions.

STAFFORD T. THOMAS

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Superpower at Sea: U.S. Ocean Policy. By Finn Laursen. (New York: Praeger Publishers, 1983. Pp. xiii + 210. \$24.95.)

On Saturday, March 3, 1981, a two-sentence notice was tacked onto the bulletin board in the State Department newsroom. This modest note fell like a bomb on the worlds of diplomacy, international law, and international relations; it announced that the United States would seek to ensure that the Tenth Session of the Third United Nations Conference on the Law of the Sea—due to open only two days thence—would not, as scheduled, be its last by preventing conclusion and adoption of a comprehensive convention on the Law of the Sea. Furthermore, virtually the entire U.S. delegation to the Conference, capable and experienced people, were summarily dismissed, and a far smaller United States delegation (headed by an individual of no visible qualifications) was instructed not to engage in any negotiations whatever pending completion of a thorough review of the draft treaty then under consideration. Thus ended, abruptly and unceremoniously, more than a decade of constructive American efforts to achieve through multilateral negotiations a legal regime designed to insure peaceful and orderly use of more than 70% of the earth's surface.

Finn Laursen, a Danish political scientist, was present at several sessions of UNCLOS III and has written previous analyses of U.S. policy and policymaking in connection with the Law of the Sea. This book covers the evolution of this policy from the late 1960s through the signing of the United Nations Convention on the Law of the Sea at Montego Bay, Jamaica, on December 10, 1982. More than a chronological description of the evolution of American policy on the numerous complex issues of the Law of the Sea, the account is decidedly analytical.

In an effort to try to explain the policies described, the study applies four analytical models, namely, a modernized version of classical realism (statist goals), international independence, bureaucratic politics, and domestic politics. All four models can help to describe and explain some aspects of U.S. ocean policy in this period, but domestic politics is found to have the greatest explanatory power. It explained the passage of the Fishery Conservation and Management Act of 1976, and the Reagan shift away from multilateralism in 1981 and 1982. (p. ix)

The book is organized into nine chapters, each followed by numerous notes, primarily citations of sources. The text is followed by a "selected bibliography" that includes a list of publications of the Center of International Studies of Prince-

ton University, under whose auspices Laursen wrote this book. The chapter notes and the selected bibliography together form a comprehensive and useful collection of published materials for further reference. The index is also useful.

Laursen begins with an overview of the problem in which he summarizes developments in both the Law of the Sea and U.S. policy in relation to it. Then he explains the analytical perspectives and hypotheses to be applied to the problem. The next five chapters are straightforward descriptions of the major components of U.S. ocean policy through the Carter administration—the politics of security, offshore petroleum rights, fishing, and deep seabed mining—and of the Draft Convention and the Reagan review of it. Chapter 8 applies the analytical tools described earlier to these various conflicting interests within the United States government and society, and the final, very brief, chapter contains concluding remarks.

Laursen did not have the kind of access to, and experience with, the inner workings of the United States government as did, for example, Ann Hollick, Robert E. Osgood, and John Norton Moore, who all have written extensively about U.S. ocean policy, but Laursen has used a wide range of published materials including, wisely, some of the materials produced by nongovernmental organizations (NGOs) represented at UNCLOS III. I am impressed with the accuracy of Laursen's descriptions of events and of American decision making, which undoubtedly results from his attendance at several sessions of the Conference and his association with NGOs, as well as his ability and diligent research.

This book is a superb case study of foreign policy decision making and as such would be a useful supplement to textbooks in a variety of foreign policy and international relations courses, but because of its specialized nature has limited value as a text itself. It is balanced, both ideologically and between fact and theory, but it is quite clear in its conclusion that the Reagan policy regarding the Law of the Sea is misguided and harmful to the long-term best interests of the United States. It ends with the hope that "maybe a future administration in Washington will give up confrontation and return to law and diplomacy" (p. 181).

MARTIN IRA GLASSNER

Southern Connecticut State University

War in the Modern Great Power System, 1495-1975. By Jack S. Levy. (Lexington: University Press of Kentucky. Pp. xiv + 215. \$24.00.)

This volume is the integration of Levy's work on war, beginning with his award winning dissertation and carried admirably forward in several important, widely cited journal articles (some material from which is reprinted in the volume). The first chapter posits that "great power" wars are different than wars in which lesser powers are the principal actors; as a consequence, he claims, they must be analyzed separately. Moreover, Levy argues the modern power system began neither in 1815 as many political scientists assume, nor following the Treaty of Westphalia in 1648 as many historians assert, but rather emerged at the end of the fifteenth century following the signing of the Treaty of Venice in 1495. A "Great Power" is defined to have a high level of relative military capabilities and a conception of their interests as global rather than local; to pursue their perceived interests more aggressively, often using military threats and force; to be recognized by other great powers; and to often possess certain perquisites in international organizations (veto power, for one). Levy's actual list of great powers is catholic. What is especially important here is that Levy is not simply extending a set of analyses back in time. He is making a theoretical argument that great powers are different, and that analyses that fail to recognize that fact face potential validity problems. This assertion itself is not empirically examined in the book.

Levy has done yeoman work in generating a data set from historical materials, building consciously and conscientiously upon the work of others such as Pitirim Sorokin, Quincy Wright, David Singer, Melvin Small, and Frederick Woods and Alexander Baltzly. One hundred and nineteen wars involving great powers are identified in chapter 4. Chapter 5 details major variables: duration, extent, magnitude, severity, intensity, and concentration. It presents a clinical description of great power war as preparation for more analytical investigations presumably to follow. Chapter 6 is a summary of so-called "trends" in war. It presents scattergrams of some of the variables over time in order to provide some idea of the trends. Neither the graphics nor the description of them are particularly surprising. Using the date as a dependent variable, simple bivariate statistics are employed to examine a linear hypothesis of constant change in duration, extent, magnitude, severity, intensity, and concentration.

Although the inclusion of a considerable amount of quantitative material is commendable, most of it is what Edward Tufte would call "chart

junk." There are no really striking, non-obvious patterns. Scattergrams, bar charts, and bivariate correlations are unlikely to be satisfying to either the quantitatively or qualitatively oriented student of war. Cyclical analysis is suggested but dismissed as being "very complex" and not a justifiable expense in light of the visual absence of such trends in the bivariate displays. Chapter 6 ends with the examination of the major variables in five mutually exclusive, exhaustive time periods, yielding insights relating to which periods were the least war-prone (the eighteenth century).

The only hypothesis to be examined in this volume relates to the contagion of war, which is explored by calculating first-order autocorrelation coefficients (all of which are weak) and comparing the actual data to a Poisson distribution (the *chi-square* value suggests randomness, not contagion). Five-year aggregations and simple lagged correlations also indicate the absence of any regular, simple pattern.

Levy explicitly acknowledges both the contributions and the liabilities of the volume: It is a descriptive effort at collating some information on great power wars over the past five centuries. On the other hand, all of the information is presented at the systemic level and relates to the attributes of 119 great power wars. Thus, the effort is a macro-systemic one that ultimately must be oriented around a theoretical question. No such question emerges in this volume, and, as suggested in the Conclusion, it awaits further work. Several important and interesting findings are presented. For example, great power wars are more likely to emerge while another war is in existence, but great powers are not, apparently, especially mindful of the costs of the last war when deciding upon whether to enter the next one. What remains is for Levy and others to come up with plausible theoretical statements that can be empirically tested to explain these facts. It is that very difficult task of providing answers to the important "why" and "so what" questions that is missing in this important, descriptive volume.

Ultimately, we need to know if great power wars are a distinct social phenomenon, whether the underlying behavior is itself unique, what the characteristic dynamics are, and how all of this has been evolving in time. Levy's work will hopefully encourage additional inquiries of this sort.

MICHAEL D. WARD

University of Colorado

The Emergence of the NIEO Ideology. By Craig Murphy. (Boulder, Colo.: Westview Press, 1984. Pp. x + 190. \$22.00.)

To the states composing the Third World, nationalism and cultural autonomy has meant the right to develop their economies according to their own mix of political, social, and economic traditions and not to remain vassals to those countries who were first off the mark as far as industrialization was concerned. Why, as they would put it, must the American political experience and subsequent privileged economic position be enshrined as the perfect and transcendent model any more than the twisted neo-Marxist Soviet model? Central state planning, by the end of World War II, was a feature utilized by all of the belligerents, democracies and dictatorships alike. Further, why should not this tradition of using the power of the state to redress domestic, economic, and social inequalities be transformed into the international arena, so that the economic inequalities of the South in relation to the North have a chance of redress through the intervention of international institutions whose purpose would be not to ensure a free global marketplace preserving a historic division of labor based on traditional notions of supply and demand, or creditor and debtor, but to provide measures of discrimination (or non-reciprocity) in trade matters to favor the many rather than the few national actors?

The lesson that the Third World had already learned by the end of the War—and has relearned repeatedly ever since—is that the rich get richer and the poor get poorer if the rich are left to their own devices. Who, in other words, is to look after the poor besides the powerless poor themselves?

The New International Economic Order (NIEO) has been termed by some American political scientists who admire the Roosevelt New Deal and the Johnson Great Society as "global welfarism." If by this is meant linking economic and social pluralism (i.e., nationalism) with international institutions that would intervene in the "natural" international marketplace to redress the otherwise economic imbalance between rich and poor, then indeed the agenda of the NIEO was spelled out early, with the OPEC price rises of the 1970s merely adding political clout to a doctrine that was already well formed and already the object of dispute. As Murphy puts it in discussing whether the NIEO can be seen as a "new internationalism":

Critics who call the NIEO ideology 'mercantilism' can deal with the rights of states, at least as they are reflected in the western tradition of international law. But the NIEO ideology defines duties of states that go beyond respecting others' rights. And the

central concern of the new order ideology remains the activities of international institutions designed to help manage the world economy, something traditional mercantilists never thought of. (p. 165)

The emphasis on the positive duties of states—which is an argument that the weak always try to make against the strong—is well entrenched in the concepts of the NIEO and is resisted accordingly by those states that feel they might have something to lose. Why should the United States, as the Reagan administration put it, give away any theoretical or remotely advantageous self-benefit by creating new competition for itself in the Law of the Sea exploitation of deep seabed minerals through the creation of new international institutions that it might not be able to dominate? Why not keep the rules of the game in your favor as long as possible?

International institutions that work to favor the weak should themselves be kept weak or, even better, prevented from coming into existence. Alternatively, those that protect the strong should continue their useful protective function. Again, national bankruptcy must be avoided at all costs, even the endless rescheduling of interest payments (no mention of principal repayments yet) because of its threat to the lender. Even though debtor strikes, internationally speaking, have not yet taken place, they are not far from the surface in the negotiations concerning Latin American debt.

So far, although there have been some incremental adaptations of international economic institutions to third-world development needs, there has not yet been any fundamental restructuring of the international economy to provide the Third World with the essential NIEO ingredients of justice, equity, and autonomy. Dependency, in a world of interdependencies, still seems the order of the day. This book makes good, if sometimes difficult, reading.

ROBERT S. JORDAN

Naval War College; University of New Orleans

Law, Morality, and the Relations of States. By Terry Nardin. (Princeton, N.J.: Princeton University Press, 1983. Pp. xii + 350. \$35.00, cloth; \$14.50, paper.)

This is an important philosophical analysis of international law—and, to a lesser extent, international politics and organization—in terms of what Nardin identifies as the “practical” and “purposive” conceptions of international society. Obviously well-versed in the literature of his subject, Nardin calls upon a wealth of earlier analyses to buttress his own: “that morality and law are

best understood as authoritative practices constraining the pursuit of different purposes rather than as instruments for the joint pursuit of shared purposes” (p. ix). Nardin singles out Michael Oakeshott’s *On Human Conduct* (Oxford University Press, 1975) as the catalyst for his ensuing analysis.

In a rather difficult introductory chapter that demands a close reading, Nardin defines his terms and outlines his approach. Once his eclectic conceptualizations have been mastered, however, the rest of Nardin’s analysis, which is divided into three parts, falls into place rather easily.

If I understand Nardin’s theoretical position correctly, the main function of international law is practical, that is to regulate the relations of states possessing different and even incompatible goals, not purposive in the sense of achieving shared purposes. International society, argues Nardin, is “an association of independent and diverse political communities, each devoted to its own ends and its own conception of the good . . . which can only be enjoyed through participation in a common body of authoritative practices” (p. 19). This practical conception is “essentially constraint-oriented” (p. 14) and “is entitled to stand as the most adequate and illuminating of any of the conceptions of international society, morality, and law in terms of which the attempt to understand world affairs has been made” (p. 24). Although this final claim is certainly hyperbolic overkill, what follows is impressive.

Part 1 examines the gradual emergence of the modern state system “as a kind of practical association” (p. 49). It demonstrates how international law as a “body of rules specific to the relations of states” (p. 59) developed from an earlier understanding of the law of nations as a part of natural law and explores various experiments in international government.

Part 2 analyzes how international law can exist without such centralized rule-making institutions as a legislature, enforcement agency, or common judge. “It is possible to imagine law without legislation and law without sanctions, but impossible to imagine law without rules and obligations” (p. 133). Myres McDougal is taken to task for his purposive usage of international law to achieve “world public order.” “As an account of the character of international law,” McDougal’s conception “is unsatisfactory because it dispenses with the distinctions between rules and interests, making and applying law, and authority and efficacy upon which law, as a mode of association distinct from one governed wholly by considerations of interests and power, rests” (p. 206).

Part 3 examines international law in its moral aspect. In so doing it discusses some of the impli-

cations of the practical/purposive distinction for the concepts of international justice in the redistribution of social and economic goods, implementation of human rights, and waging of war. Here Nardin defends a practical conception of international justice against various purposive alternatives and objections. He writes, for example, that "to insist on respect for human rights is to demand that the policies and laws of a community reflect the principles of impartiality with respect to persons and their ends inherent in the idea of practical association" (p. 276).

In what I found to be maybe the most interesting discussion of all, Nardin discusses in his final chapter the relevancy of international morality and law for a culturally and ideologically diverse world. Although he acknowledges the existence of this diversity, which Adda Bozeman has so eloquently analyzed elsewhere, Nardin maintains that "the evidence accumulated through historical and anthropological study suggests the existence of certain striking moral similarities among otherwise dissimilar societies" (p. 320). He argues that "many of the conflicts between Western and non-Western states concerning international law can be explained by the different situations and interests of each, without resorting to the idea of a cultural gap" (p. 322). He concludes, therefore, that "not only are international law and morality possible in the face of ideological diversity, but they constitute practices peculiarly well adapted to helping states deal with the difficulties created by that diversity" (pp. 322-323).

Nardin has produced a stimulating analysis of international law within a heuristic framework that will amply reward the careful and thorough reader. His book is documented with useful footnotes throughout and contains a bibliography and index at the end. It should prove a major interpretation of, as well as addition to, the existing literature.

MICHAEL M. GUNTER

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The Making of America's Soviet Policy. Edited by Joseph S. Nye, Jr. (New Haven, Conn.: Yale University Press, 1984. Pp. x + 369. \$27.50.)

Why has it been so difficult for the United States and the Soviet Union to break out of the downward spiral of the cold war? Joseph Nye and his colleagues make two central arguments. First, although "American policy [is affected by] Soviet behavior . . . the fit is so imperfect that further explanation is necessary" (p. 6). The reactive component is not to be discounted totally, but

American foreign policy in general, and America's Soviet policy in particular, also are shaped in fundamental ways by their domestic roots—more specifically, their domestic political roots in the unique institutional structures, electoral practices, and ideological milieu of the American political system.

The book's second key argument is that these have not been especially nourishing roots. They have fed tendencies for "inconsistency and incoherence" in American policy (p. 334), for extreme pendulum swings between overemphasizing and overreacting to the Soviet threat, and at other times tendencies for self-deceptive underemphasis and unilateral restraint. The overswings to the Right have caused important opportunities for improving American-Soviet relations to be missed. The overswings to the Left have encouraged Soviet intransigence and provocations.

One of the most commendable features of this volume is how all of the chapters address these two themes. The lack of integration that weakens many other edited collections is avoided. These authors build upon each other's distinct expertises so that there is not only substantive comprehensiveness, but also an impressive overarching theoretical structure. The three chapters in Part 1, "The Actors," are exemplary. William Schneider elucidates the changes in American public opinion that have contributed to the oscillations of the past 20 years from cold war to detente to neo-cold war. He documents both the intralite split between conservative and liberal internationalists, and the tension within the noninternationalist (but no longer inattentive) mass public, over the values of peace and strength. I.M. Destler finds the only consistency in Congress' role since 1972 to have been "to disrupt such purposive policies as the executive branch was pursuing" (p. 38). His analysis is sensitive both to issue area specific factors and to more general patterns of congressional behavior. Robert Bowie's wealth of policy world experience comes through in his critical discussion of how the executive branch has organized itself from Truman to Reagan for the formulation and conduct of Soviet policy. His advocacy of a strong Secretary of State system is more convincing than most.

Part 2 applies the inward focus to key policy subareas. Richard Betts draws the lesson that instead of repeating past counterproductive efforts to transcend domestic constraints on both nuclear arms control and competition, doves and hawks respectively should acknowledge that "treaties are not salable unless they appear to preserve military equality, and that unilateral efforts sufficient to substitute for treaty restraints on Soviet forces are not long sustainable" (p. 124). Alexander George's article on political crises adds a

historical context and a conceptual framework that inform the discussion with a sense of the inevitability but not immutability of American-Soviet international rivalry. Marshall Goldman and Raymond Vernon provide a well-reasoned indictment of anti-Soviet economic coercion, although the depoliticization they propose is hard to square with the evidence of domestic constraints provided elsewhere. Deserving of special mention is Strobe Talbott's powerful chapter on human rights, in which he shows the self-defeating consequences of overt American efforts to prod Soviet internal democratization. His identification of the tension between doing good and doing well (p. 200) gets to the very essence of the powerful causes and problematic consequences of American foreign policy's domestic roots.

In Part 3, Ernest May ("The Cold War"), Stanley Hoffmann ("Détente"), and Samuel Huntington ("Renewed Hostility") make the domestic causes and consequences themes the focuses for their historical reviews. These chapters whet the researcher's appetite for delving more deeply into the domestic politics of some of the specific incidents and policies they highlight. Dimitri Simes's chapter on the roots of the Soviet Union's "American policy" does not as neatly fit the volume's thematic structure, but does serve as a necessary complement.

Nye concludes with a synthesizing and prescriptive chapter. Nothing less than "a flexible strategy that combines a sensible vision of the future and a definition of our interests with appropriate means in a manner that can sustain domestic support" is called for (p. 341). This is quite the tall order. At certain points the reform proposals (both Nye's in this last chapter and the other authors' in their respective chapters) are left too vague; others are open to debate over their feasibility or viability. More important, though, is the success of the overall effort to develop what in another context Alexander George has called "policy-relevant" theory. In this and other ways this volume should be a model for others.

BRUCE W. JENTLESON

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The Arab-Israeli Conflict: Perspectives. Edited by Alvin Z. Rubinstein. (New York: Praeger Publishers, 1984. Pp. ix + 221. \$27.95.)

The pendulum of world attention rarely swings away from the Middle East for long. This is a realistic reaction because, in spite of being the subject of many clichés, the Middle East is in fact potentially the world's most continually explosive tinderbox. It is therefore not at all surprising that

scholars, journalists, and politicians continue to analyze and unravel the volatile Middle Eastern tangle.

The edited volume assembled by Alvin Z. Rubinstein attempts to further our understanding of what has been to date the most publicized aspect of the Middle East drama. The Arab-Israeli conflict is dissected in this volume as follows: In chapter 1, "Genesis," Adam M. Garfinkle sketches the pre-1948 background of the present hostilities. In chapter 2, "Seven Wars and One Peace Treaty," Itmar Rabinovich itemizes the bare details concerning the several major collisions between Israel and its Arab neighbors and inhabitants. In chapter 3, "Transformation: External Determinants," Rubinstein views the Arab-Israeli conflict within a broader set of circumstances ranging from the many technological advances in weaponry systems, through the U.S.-Soviet rivalry, to international terrorism. In "Israel: From Ideology to Reality," Alan Dowty examines the complexities and dilemmas of present-day Israeli political life. Aaron David Miller discusses what he refers to as "The Palestinian Dimension" in chapter 5. And in the concluding essay, Haim Shaked presents an overview designed to tie past, present, and future policies together.

The Dowty, Miller, and Shaked essays are insightful and provocative examinations of extremely complicated subjects. I found in them not only intelligent presentations of descriptive material, but also a keen sense for what the evidence may well mean.

Despite the noteworthy contributions of the three essays mentioned previously, in several instances I found the volume to be noticeably uneven in terms of the depth of treatment, and, overall, to be guilty of certain important sins of omission. There is a pronounced tendency to present a sketchy picture of events, to be more concerned with chronology and touching upon numerous points rather than thoroughly investigating any of them. In many ways the volume should be subtitled "An Introduction" rather than "Perspectives."

The lack of attention to political economy, either domestic or international, is bothersome. In addition, too little attention is directed toward how domestic and international events in the Middle East—specifically, the Arab-Israeli conflict—are parts of a puzzle in which the pieces taken individually acquire different texture and meaning than that which occurs when they are viewed holistically.

It is also not clear from this volume how we should read the Arab-Israeli conflict. Is it fundamentally a political matter with the frequent military overtones and exchanges being critical but

subsumed into larger nonmilitary, possibly even a non-Arab-Israeli, framework? Or, perhaps we should interpret the Arab-Israeli conflict as being a number of discrete, equally critical ingredients, with the military aspects being somehow separate and periodically ascendant? Several essays treat the conflict as a seemingly continual succession of military episodes while on the other hand, Shaked suggests that it is "a multi-issue, or multidimensional conflict" (p. 183). Although I do not want—and indeed would be most suspicious of—forced parsimony and simplified resolutions of complex matters, it would have been appropriate to have a more thorough, integrating treatment of these matters than was generally presented to the readers.

On balance, this volume contains more rewards than difficulties for the reader. In those instances where I found problems, there were important compensating qualities; and the essays by Dowty, Miller, and Shaked, make valuable contributions.

CARL F. PINKELE

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International Dynamics of Technology. By Ralph Sanders. (Westport, Conn.: Greenwood Press, 1983. Pp. xiii + 332. \$35.00.)

The impact of technology and technological change on both international relations and national development has been pervasive. It is not so much that technology is out of control as the fact that the emergence of new technologies has created new global issues and made the management of political relationships more complex. Ralph Sanders attempts to capture how the interaction between politics and technology has created fundamentally new challenges for decision makers who are trying to maximize national interests and capabilities and minimize their vulnerability to technological changes outside of their immediate political direction.

Sanders suggests that there are three main reasons why the interactions between technology and international relations are important. First, an increasing number of the problems faced by humankind either are caused by technology or call for technical solutions. Second, the technological dimension of international relations is the least understood and appreciated by scholars and policymakers. And third, technological changes will continue to give rise to new and unique challenges that will demand additional knowledge and new skills on the part of national leaders.

For Sanders, the important point to get across is that the proliferation of technology-related issues is not a temporary phenomenon. Sanders

believes, correctly in my view, that these types of issues will increasingly occupy the attention of political leaders—who in many cases will lack the tools and capacity to deal with them effectively. Sanders's book represents an effort to alter that potential situation from occurring by highlighting the continued salience of technology in the economic, military, and political realms. In this regard, Part 4, which deals with institutions, strategies, and processes for managing technology, is particularly relevant.

Sanders's analysis points out that we may be witnessing a significant change in the conduct of diplomacy, which has occurred in response to technological imperatives. Nonetheless, he believes that "rather than being swept along by events, mankind can help shape the future by exploiting technological strategies among other devices" (p. 6). This may require, however, major improvements in the world's institutions and decision-making processes devoted to dealing with technological matters. In fact, Sanders suggests that "in the distant future the growing diffusion of technology worldwide could provide a potent counterforce to nationalism and thus contribute to restructuring the political organization of the planet" (p. 6). Although he does not push this argument too far, he might have acknowledged in somewhat greater depth that there are impediments to the widescale diffusion of technology within the presently constructed international system and that the constraints and opportunities associated with technology look much different from the perspective of the Third World than from the industrialized nations.

In the final analysis, Sanders acknowledges that "managing technology to gain maximum social, including international, benefit is an integral part of political wisdom" (p. 305). Unfortunately, at times he assumes that the political foresight and understanding will be there because of the imperatives created by the presence of technology. One could easily argue in the other direction, that is, we are seeing the emergence of a more neomercantilist world in which technologies are increasingly viewed as a national resource to be coveted, nurtured, and protected—but not shared. The rise in global competition particularly in the industrialized world, the growing concern about the increasing sophistication of exports from the newly industrialized nations, and the major differences within NATO and among many scholars and policymakers about whether technology can be used as a bargaining chip in East-West relations, leads one not to discount Sanders's argument about the salience of technology but to see it as a source of division and conflict.

Overall, one has to applaud Sanders for pro-

ducing an excellent piece of work, one that will be useful to scholars and practitioners involved in the field of foreign policy and international relations. Given the fact that new technologies such as computers and materials science have enhanced our problem-solving capabilities, it is easy to see how one might become optimistic about the readiness of political leaders to forge closer bilateral or transnational links in order to deal with global problems. Yet, the history of international relations over the last several decades indicates that the "wisdom" that Sanders hopes for may be severely lacking. Let us hope he is not as wide of the mark as he might be.

DENIS FRED SIMON

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The International Law of Pollution: Protecting the Global Environment in a World of Sovereign States. By Allen L. Springer. (Westport, Conn.: Quorum Books, 1983. Pp. xiv + 218. \$37.50.)

The purpose of Springer's book is to offer an analytical framework through which to study international environmental law and to suggest its prospects for the protection of environmental interests. He begins with a discussion of the range of environmental problems identified as appropriate for international concern and examines some of the scientific, economic, and political factors that make these problems particularly difficult to resolve on an international level. This is followed by a discussion of the role of international law as a framework for environmental protection and four major "schools of international environmental law, e.g., the traditional, transnational, modernist, and ecological imperative" (pp. 34-52).

Springer's basic argument is that the "international legal order has the capacity to resolve a number of significant environmental problems . . . through the implementation of treaty obligations and evolving norms of customary international law . . ." (p. xiv). As he admits (p. 54), his bias is toward the "traditional" approach as reflected in Hans Kelsen's *Principles of International Law* (Rinehart, 1952). Thus, it is the discovery and application of specific legal norms created by state practice and treaties that are the heart of his subsequent attention.

After first describing the general threshold at which environmentally damaging actions become legally significant, Springer discusses problems associated with making the threshold a viable legal concept. Specifically, he examines the ways in which concrete working limits for various kinds of detrimental change are established and given

legal expression before turning to current attempts by publicists to move away from traditional liability-based regimes of state responsibility in favor of approaches designed to create responsibility before pollution occurs. The final chapter is a case study of a proposal to build an oil refinery at Eastport, Maine, which illustrates the counter-thesis that nations encounter problems when they attempt to rely upon ad hoc negotiations (as opposed to external legal standards) to settle transboundary environmental disputes.

The book's major contribution is to advocate the relevance and the importance of the present legal system in preventing and/or settling environmental problems of international concern. The analytical "framework" that Springer utilizes is consistent with his adopted role of the international legal scholar as an "advocate" as opposed to a "social scientist" (p. 52). That is, he makes no attempt to analyze why scholars have questioned *whether* the present international legal system is sufficiently developed to resolve the disputes that arise over environmental issues. Rather, he assumes that rules of state responsibility, once in place, provide a major force for international environmental cooperation.

Unfortunately, the book's contributions beyond this advocacy position are rather meager. Springer disavows any attempt to analyze the "international legal order's capacity to respond to the threat posed by pollution," or "a comprehensive assessment of the steps that must be taken to enhance this capability" (p. 196). As such, his approach probably exaggerates the potential influence of international law upon global environmental management and adds very little to our ability to explain or predict international behavior. Other students, like Springer, take international environmental management as the policy outcome to be explained, and they offer generalizations that may be examined by themselves or by others. For example, they have suggested that levels of national economic development, domestic political structures (e.g., the state-society relationship), and/or international organizations are crucial influences on states' efforts to manage global environmental pollution. However, in this instance, international environmental scholars interested in further theory development will have to search elsewhere for testable generalizations. Some international legal scholars, on the other hand, may find the answers they are seeking in this book.

JAMES P. LESTER

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Alliance Security: NATO and the No-First-Use Question. Edited by John D. Steinbruner and Leon V. Sigal. (Washington, D.C.: Brookings Institution, 1983. Pp. viii + 222. \$28.95, cloth; \$9.95, paper.)

The Atlantic Alliance and Its Critics. Edited by Robert W. Tucker and Linda Wrigley. (New York: Praeger Publishers, 1983. Pp. vii + 192. \$24.95.)

Both of these anthologies address, in different ways, one of the central and long-standing dilemmas of the Atlantic Alliance: Over the decades, the United States has become compelled to deal with the Soviet Union on the basis of nuclear *parity* (as was reflected in the arrangements of the Strategic Arms Limitation Talks), whereas Washington can convincingly guarantee the security of Western Europe only on the basis of an implied American nuclear superiority. This is a dilemma for which neither the United States nor the Europeans can be blamed, nor is it likely that they can escape from it. The advent of nuclear parity between the superpowers has led to conflicts between the United States, which needs to take into account the potential nuclear devastation of the United States and therefore must qualify the use of nuclear weapons, and NATO partners at the forward line of defense who cannot accept a strategy that implies sustained conventional warfare at the expense of their territory and population. But aside from the security dimensions of that dilemma, both the United States and its European NATO partners (as well as the Soviet Union) perceive military-strategic developments as carrying a fundamental political import for the shape of the present and future European or political order. All parties express political purposes in the language of nuclear and conventional strategy and perceive the meaning of strategic doctrinal announcements primarily in political rather than purely military-strategic terms.

One of these doctrinal questions is that of the "no-first-use" of nuclear weapons. This is the focal point of *Alliance Security*, edited by Steinbruner and Sigal, in which a number of prominent American and European experts consider the political as well as military-strategic implications of a NATO doctrine that would deny the Western alliance the initiation of nuclear warfare. When McGeorge Bundy, George F. Kennan, Robert S. McNamara, and Gerard Smith published an article in the spring, 1982 issue of *Foreign Affairs* in which they concluded that no American president could responsibly initiate the use of nuclear weapons in defense of Western Europe, they challenged a doctrinal orthodoxy that had governed NATO's strategic thinking for decades. Because the no-first-use idea was, at bottom, directed

more toward the structural and political deficiencies of NATO than toward affecting the East-West military balance, it inevitably addressed long-standing issues that had concerned NATO over the decades and thus occasioned an intense debate among American and European public figures and strategic experts. NATO planners had always assumed Soviet superiority on the level of conventional forces and therefore the necessity for NATO to deter a Soviet attack with the threat to resort to nuclear weapons—the no-first-use idea, on the other hand, challenges both the premise and the conclusion of that assumption and calls for relying on conventional defense of NATO territory and a corresponding adjustment of NATO's deterrence doctrine. As a corollary, the no-first-use principle calls for a sharp distinction between conventional and nuclear military operations and a withdrawal of nuclear weapons from forward positions—reminiscent of the disengagement proposals of the late 1950s, one of which was presented by George F. Kennan in the fall of 1957 in the BBC lectures.

The arguments for and against a revision of NATO doctrine that are presented in *Alliance Security* are marshalled with sophistication, clarity, technical expertise, and circumspect political judgment. David N. Schwartz provides a historical perspective of NATO's doctrines and the corresponding force posture adjustments; and William W. Kaufmann deals, in two chapters, with the conditions and limits of strategic and tactical nuclear deterrence and with the possibilities for a nonnuclear deterrence posture that emerge from the NATO-Warsaw Pact military balance. Jonathan Alford (in a few pages) and Leon V. Sigal (in two chapters) examine the relationship between stable deterrence and the no-first-use principle, with Sigal arriving at the conclusion that the no-first-use strategy, inasmuch as it calls into question America's willingness to use nuclear weapons in Europe's behalf, would give "undue advantage to those who would oppose any change whatever" and thus "only delay the implementation of essential changes in NATO's posture and plans" (p. 145). Gert Krell, Thomas Risse-Kappen, and Hans-Joachim Schmidt examine West German attitudes on the no-first-use question, a subject of central importance because in many respects NATO has been reduced to a bilateral U.S.-FRG security connection, and because the German position on the issue is governed by the particularities of the Federal Republic's geography, history, and its special relationship with the German Democratic Republic. In the concluding chapters, Johan Jorgen Holst assesses the practical obstacles standing in the way of implementing the no-first-use principle, and John D. Steinbruner offers

some general reflections on Alliance security and on matters of disagreement that are sharpened by the no-first-use debate.

The essays that comprise the volume edited by Tucker and Wrigley, *The Atlantic Alliance and Its Critics*, were presented initially at a seminar series sponsored by the Lehrman Institute during 1981 and 1982. Their focal point is the political rather than the military-strategic dimension of transatlantic issues. Theodore Draper and A.W. DePorte provide a long-term perspective on the Atlantic Alliance, with Draper arriving at rather pessimistic conclusions about the past performance and future prospects of the Alliance, whereas DePorte believes that it has over the decades served some of the most important interests of its members reasonably well. Pierre Hassner sharply disagrees with DePorte's central thesis that the postwar division of Europe has solved the German problem, and he examines also

the domestic social and political conditions that affect Alliance cohesion. Simon Serfaty and Pierre Lellouche deal with the future of the Alliance, both seeing the need for structural changes and a fundamental political redirection that would reflect a more sustained and self-conscious consideration of European regional political interests—a view that is not shared by Robert W. Tucker, who suggests in his concluding remarks that the corrosive effects on the Alliance will continue so long as Europe remains as committed to detente as it is to the Alliance.

Both volumes are required reading for anyone interested in the future of the Atlantic Alliance in a decade in which the foundations laid for the transatlantic compact in the postwar era are visibly eroding.

WOLFRAM F. HANRIEDER

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Normative Theory

After Marx. Edited by Terence Ball and James Farr. (New York: Cambridge University Press, 1984. Pp. x + 287. \$37.50, cloth; \$11.95, paper.)

Commemorating the centenary of Marx's death, this collection of 12 essays on Marxian theory is based on the view that Marxism is a uniquely fruitful, but often inadequate, ambiguous, and unfinished body of social and political theory. The essays tend to be sympathetically critical of Marx and Marxism, and most offer reformulations or new accounts of troublesome areas. A unique offering of this collection is that most of the authors are conversant with non-Marxian schools of social theory and philosophy, often allowing them to experiment with eclectic and sometimes innovative approaches.

The essays are divided into three thematic sections. The first, "History and Revolution," deals with issues surrounding Marx's explanations of history. A strong essay by William Shaw, "Marxism, Revolution, and Rationality," argues that recent attempts to show the untenability of Marx's views of revolution in terms of prisoner's dilemma game theory rely on a narrow instrumentalist conception of rationality as well as on unsupportable psychological assumptions. An essay by Jon Elster, "Historical Materialism and Economic Backwardness," proposes a general theory of

economic backwardness to show why—contrary to Marx's expectations—socialist revolutions have occurred in backward countries while advanced countries have developed resilient forms of capitalism. The result is a rather disappointing two-dimensional typology combining economic and motivational variables rather than anything like an explanatory theory. Two well-argued articles in this section deal with Gerald Cohen's recent and influential attempt to construe Marx's historical explanations in functional terms. Philippe Van Parijs's "Marxism's Central Puzzle" assesses Cohen's approach in light of an evolutionary theory that would show how reciprocal causality (the "central puzzle") is possible in Marx's base-superstructure model. And in "Marxian Functionalism," James Nobel suggests that Marx's conception of history not only relies on functional explanations (as Cohen argues), but also provides the concrete accounts of change that justify such explanations (which Cohen believes Marx lacks). Finally, Richard Miller's "Producing Change: Work, Technology, and Power in Marx's Theory of History" argues that Marx's explanations do not invoke technological determinism, but instead rely on the broad political and economic causalities expressed in the concept of mode of production. Miller's essay is perhaps the weakest of the collection: His version of technological determinism is a straw man, and

he seems unaware of structuralist interpretations that parallel his own with more force and clarity.

The essays in the "Morals and Politics" section develop the contours of Marx's political theory. A solid essay by Paul Thomas, "Alien Politics: A Marxian Perspective on Citizenship and Democracy," locates Marx within the tradition of Aristotle, Rousseau, and Hegel that emphasizes—*contra* liberalism—the positive aspects of politics. C.B. Macpherson's competent and lucid "Democracy: Utopian and Scientific" repeats his earlier criticisms of pluralism (it is neither very scientific nor democratic) to suggest a return to pre-liberal democratic ideals in the context of a science like Marx's. In "Marx's Moral Realism: Eudaimonism and Moral Progress," Alan Gilbert argues that Marx is a moral realist comparable to Aristotle. Although Gilbert is surely right that Marx possessed a systematic ethics embodying Aristotelean features, the "realist" label (borrowed from contemporary analytic philosophy) suggests a metaphysics quite foreign to the critical and reflexive elements of Marx's philosophy. Finally, John Roemer's "Exploitation, Class, and Property Relations" makes a remarkable contribution toward specifying economic exploitation in feudal, capitalist, and socialist societies in terms of a general model that focuses on property relations.

The "Methodology and Criticism" section is tightly organized around distinguishing Marx's reflexive materialism from the positivistic materialism of Engels. James Farr's well-argued "Marx and Positivism" emphasizes the incompatibility of Marx's philosophy with positivism. Terence Ball's "Marxian Science and Positivist Politics" underscores the connection between Soviet social engineering and Engels's positivistic interpretation of Marx and includes an excellent section on the Hegelian roots of Marx's philosophy of science. In "Marxism and Method" Terrell Carver seeks to dispel the association between the "dialectical method" that Engels borrowed from a cursory reading of Hegel's *Logic* and Marx's own "methodological pluralism."

If the failings of several of these essays can be traced of any one source, it is in the apparent non-conversance of their authors with parallel problems in critical and structuralist Marxism. Their collective success lies in a vigorous broadening of the terms of discourse surrounding Marx and Marxism, and this is no small achievement.

MARK WARREN

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Happiness, Justice and Freedom: The Moral and Political Philosophy of John Stuart Mill. By Fred R. Berger. (Berkeley: University of California Press, 1984. Pp. x + 364. \$24.95.)

This book represents an attempt to rescue the thought of John Stuart Mill from the serious criticism that has been levelled against it for more than a century. As Berger explains, much of the confusion surrounding Mill stems from the manner in which he rejected the Utilitarianism of Jeremy Bentham. Bentham claimed that the sole scientific means for differentiating between activities is the quantity of pleasure they produce. He also maintained that he was not interested in the intrinsic worth of activities, only their final results. What Mill recognized is that to scientifically compare competing courses of action, they must be of the same order. Evaluating qualitatively different acts, on the other hand, involves bringing standards other than pleasure into the equation and taking intrinsic values into account.

Having disposed of the specific creed of Utilitarianism so effectively, Mill proceeded to insist that utilitarianism in general need not fall prey to such illogic. In 1863 he wrote: "It is quite compatible with the principle of utility to recognize the fact, that some *kinds* of pleasure are more desirable and more valuable than others." Critics have been pondering this assertion ever since. How can a theory which by definition denies the validity of intrinsic worth and considers phenomena by their pleasurable effects alone, they have inquired, distinguish consistently between different orders of pleasure?

Berger believes that Mill offered a reasonable utilitarian alternative to Benthamite hedonism and classical metaphysics. He develops that alternative with reference to the Millian view of human nature. According to Berger, Mill held that the most virtuous choices afford men the most pleasure. By his account, not only did Mill attribute pleasure to the consequences of virtuous acts but also to their contemplation before action. In this fashion, Berger concludes, Mill was able to disengage himself consistently from Bentham, deny that phenomena possess innate worth, and still take qualitative differences seriously.

Berger also brings his considerable analytical skills to bear on Mill's ambiguous "General Happiness Principle," ambivalent theory of justice, and famous defense of individual freedom. These, he argues, are compatible with the Millian conception of man as well. Berger's tone is balanced and nonpolemical throughout. He stands by Mill when he feels his thought is not treated fairly, yet is willing to give the critics their due when they appear to be correct. Thus, although Berger is confident that he has identified a significant link be-

tween Mill's view of human nature and his other controversial ideas, he does not refrain from reproving that view for being overly optimistic.

ETHAN M. FISHMAN

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Wittgenstein: A Social Theory of Knowledge. By David Bloor. (New York: Columbia University Press, 1983. Pp. xi + 213. \$25.00, cloth; \$12.00, paper.)

Ernest Gellner once remarked that anthropologists invariably become Tories for the cultures they study. A similar insight motivates this thoughtful book on the foundations of Wittgenstein's mature philosophy and its relation to the human sciences. Bloor's central contention is that Wittgenstein's remarks on language games and the foundations of knowledge presuppose a naturalist account of social needs in terms of which the nature and evolution of language games can be explained, and his main constructive task in this book is to provide the outlines of such an account. Drawing on the work of anthropologist Mary Douglas he develops a comparative typology of language games based on how they respond to anomalies—whether by excluding anomalous information or practices, or adjusting to them; and if the latter, by what internal mechanisms. Some such account is required, in Bloor's view, because Wittgenstein's notion of forms of life is at bottom sociological, yet Wittgenstein and his anti-naturalist followers have shied away from grappling with sociological—and indeed all causal—questions about language. In order to demonstrate this claim Bloor undertakes a wide-ranging discussion of Wittgenstein's views on the mind/body problem, private languages, universals and family resemblances, the foundations of logic and mathematics, and the philosophy of action. He argues first that Wittgenstein's few remarks on creativity do not begin to explain why language games adapt and evolve in the ways that they do, and, second, that those (such as A.I. Melden) who try to avoid this issue by arguing that causal explanation is irrelevant to the philosopher's task of elucidating *meaning* beg the question. Indeed, for Bloor, the whole debate between those who advocate causal explanations of linguistic actions and those who do not is misleading as well as contrary to the antireductionist spirit of the Wittgensteinian enterprise. Bloor contends that we play many different types of language games (often simultaneously), and that these are governed by different rules and are susceptible to different types of explanation.

Bloor is aware that there is a tension, at least,

between this claim and his own attempt to come up with a comprehensive comparative sociological theory of language games. Wittgenstein eschewed such ontological system-building precisely because he practiced what J.L. Austin preached: He collected linguistic distinctions like one collects beetles, and he thought anything else ultimately misguided. But Bloor is not content with this. Turning the antireductionist case against Wittgenstein, he argues that the kinds of causal explanations of linguistic actions that interest social scientists are themselves parts of our language games and forms of life which we can only ignore (at our peril) by illicitly reducing everything to the cognitive plane. Despite their distaste for all things empirical, ordinary language philosophers are not being true to their own *descriptive* enterprises when they fail to take account of the fact that language games function on a variety of interrelated planes which are "super-positioned" on one another, and that the relations between, for example, cognitive and social planes may be critical to understanding a language game which ranges over both. For example, on the cognitive plane, the statement "I do" in a marriage ceremony might be nothing more than an authentic promise; on the sociological plane, it might be an act of reproducing the nuclear family. If so, an elucidation of the meaning of the action will be incomplete and misleading if it ignores this latter. It is for this sort of reason that Bloor goes on to argue, as opposed to Hanna Pitkin and others, that there is an inherent political conservatism to the Wittgensteinian project: Its point of reference is always the prevailing conventions in a culture. By appealing to these as the ultimate means for understanding and evaluating what we do, Wittgenstein is necessarily making a conservative appeal. These are powerful, if not entirely novel, arguments, and it is worth noting that they stand independently of the particular comparative theory Bloor wants to advance.

With the exception of the penultimate chapter (on Wittgenstein's political conservatism) which is schematic and difficult to follow, the book is lucidly and accessibly written. It could profitably be used in advanced undergraduate and graduate courses on the philosophy of social explanation and the study of ideology.

IAN SHAPIRO

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Coherent Variety: The Idea of Diversity in British and American Conservative Thought. By Michael D. Clark. (Westport, Conn.: Greenwood Press, 1983. Pp. viii + 228. \$35.00.)

With the political resurgence of the Right in this decade, conservative political thought is certainly a timely subject. The conservative tradition, (like the liberal or radical ones) perhaps as a product of engagement with political controversy, is laced with seemingly inconsistent themes or ideas. This has led many persons, even some of its friends, to conclude with Clinton L. Rossiter that "Conservatism is self-contradictory. How can the Conservative preach both the inviolability of the person and the primacy of society, the ascendancy of moral values and concept of property as a natural right, the aristocratic spirit and the brotherhood of men as children of God?" (*Conservatism in America*, 2nd ed. rev., Vintage Books, 1962, p. 60). On closer examination, however, many such apparent inconsistencies reflect reliance on some underlying notion like hierarchy or community. Michael Clark examines a theme not often associated with the conservative tradition, that of diversity, and illustrates the way it ties together somewhat inconsistent ideas and thinkers within this tradition.

The concept of diversity seems more closely linked with such liberal celebrations of individual autonomy as Mill's *On Liberty* than with conservatism. Yet Clark carefully demonstrates the way diversity is presupposed by several core values of the tradition. In the first part of the work, for example, he details the implicit appeal to diversity involved in the historic grounding of conservatism in respect for religion and aversion to intellectual and social disorder. The prominence of ideas like "The Great Chain of Being" and "social harmony" in the works of early European conservatives, he argues, bespeaks a subtle affirmation of the notion of diversity captured in the notion of "unity in variety." It is respect for diversity, Clark further suggests, and a resulting distaste for any emphasis on uniformity and reductionist tendencies that largely account for conservative hostility to rationalism, rather than anti-intellectualism per se as some have charged.

In the second part of the book, Clark traces the notion of diversity first held by British, then American nineteenth-century conservative writers. He concludes this part of the book with a discussion of the role of diversity in the thought of Southern conservatives, where he confronts the way in which the concept has been used in patently reactionary formulations. In the third and final part of the book, Clark brings the discussion up to the present and contrasts conservatives with other critics of uniformity and the conformist

tendencies of the modern era.

As an intellectual historian, Clark makes his argument through an exhaustive survey of political writers over the last 200 years. This is at once a weakness and a strength of the book. As a result of this approach, his treatment of each author is highly compressed, at times verging on the superficial. His provocative placement of William James in conservative ranks (pp. 152-157) fairly begs for a more careful analysis, for example. Yet his purpose is to establish diversity as a basic conservative concept, and by means of this survey, he achieves this end. Using the idea of diversity, Clark not only relates a wide variety of thinkers in a plausible way, but also develops some interesting conceptual insights. One characteristic of a rich and significant work is the extent to which it suggests many avenues for further exploration. If only on this basis, political theorists and others interested in understanding one of the major intellectual and political movements of our time will find this work valuable.

JOHN D. HARMAN

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Political Leadership and Nihilism: A Study of Weber and Nietzsche. By Robert Eden. (Gainesville: University Presses of Florida, 1984. Pp. xx + 348. \$25.00.)

This intriguing critique of attempts at "building a systematic politics upon the foundation of a concept of leadership" (p. xix) contains several original contributions to philosophical and political discourse. First, Eden provides an intelligent examination of Nietzsche's philosophical politics and a cogent argument for reckoning with this "vulgate" Nietzsche. Second, his intricate dialogue between Nietzsche and Weber regarding the place of political leadership in a nihilistic world is the most comprehensive consideration of their intellectual interaction to date. Third, Eden uses this comparison to suggest that in its reliance on leadership American reform liberalism contains not only the seeds of its own destruction (now sprouting in neoconservatism), but also those of liberalism per se.

Eden's argument is tightly, even densely, woven. He begins analyzing Woodrow Wilson's successful substitution of "reform" for "leaderless" liberalism and emphasizing the absence of theoretical limits upon individuality in a Wilsonian world. In this context, Eden introduces Weber's attempt to impose practical limits upon individuality and thereby to preserve liberal politics once liberal principles have become obsolete (chap. 1). Although Weber's emphasis

upon self-imposed moral values seems attractive (especially, Eden fears, to American liberals recently aware of Wilson's dangers), Eden argues that ultimately it too is flawed. He develops this argument through a dialogue between Weber and Nietzsche in which the former tries to defend morality in the latter's nihilistic world. The dialogue begins in chapter 2 with Nietzsche's account of individuality as the core of liberalism and science. What follows is an examination of conflicts between liberalism and science that flow from this common origin: Nietzsche's scientific conquest of morality overcomes even liberal morality (chap. 3) and leads to an aristocratic politics subordinate to new philosophers (chap. 4). Weber responds to Nietzsche's attack upon liberalism by trying to turn science (chap. 5) and politics (chap. 6) toward "moral forces," but he can only marshal psychological resistance. He cannot call in philosophical reinforcements because he concedes to Nietzsche that political philosophy is no longer possible. Eden only briefly discusses Weber's additional reinforcements, that is, "empirical" or "given" limits to leadership. Yet he maintains that Weber was ambivalent about their efficacy and based his argument for moral restraint not upon "given," but upon "constructed," facts (pp. 218-219).

Eden concludes that Weber's attempt to arrest Nietzsche's attack upon liberalism in *medias res* fails because the moral leadership on which Weber depends destroys the moral limits he would defend. To his initial question, "What becomes of liberal politics if we abandon the plane of theoretical understanding as Weber and Nietzsche do, to reconstitute the liberal cause on the plane of practical historicism?" (p. 35) Eden responds, "One must either rely directly upon political philosophy, and defend liberal democratic institutions by recovering the legislative science of modern liberal politics, or give way before Nietzsche's alternative" (p. 219).

Eden clearly chooses the former path, but his philosophic defense of liberal democracy misses the thrust of Nietzsche's challenge and the power of Weber's response. Eden calls for "constitutional suspicion" to be directed toward a critique of political leadership (p. 237). Yet he does not explain why Nietzsche's science spares his (our) constitution, or how that constitution differs from Weber's "empirical" and "constructed" facts. (Not to mention whether it escapes Wilson's critique of private government—the impetus for reform liberalism—with which Eden began.) In short, Eden (like Weber) fails to establish philosophical criteria by which the committed liberal can defend his truth against opinion. Perhaps he (again, like Weber) finds empirical "givens" bolstered by "constructed" facts

the best possible defense for morality. This in itself is not cause for criticism. However, Eden's claim to ascend from opinion to truth, and not to hover somewhere in between, mars an otherwise fascinating work. One is left with the impression that he blames the messenger and refuses to hear the news.

NANCY S. LOVE

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The Political Theory of *The Federalist*. By David F. Epstein. (Chicago: University of Chicago Press, 1984. Pp. ix + 234. \$22.00.)

The Authority of Publius: A Reading of *The Federalist Papers*. By Albert Furtwangler. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 151. \$14.95.)

Jefferson called *The Federalist* "the best commentary on the principles of government, which ever was written," and Americans ever since have remained convinced of its merits. But the work has perhaps suffered from an excess of veneration and of familiarity. Perhaps a fresh start needs to be made, and the essentials of *The Federalist* recovered. Both of these books have made the attempt. The authors have, however, approached Publius from very different vantages, and thus have produced quite different results.

Furtwangler's book contains some promising insights, but its argument fails to develop or think them through. It disappoints because in attempting to be comprehensive, it leaves much half-argued or asserted without evidence. Furtwangler's basic premise is that any interpretation of *The Federalist* must take into account its origins in the popular press, where it was one among many pseudonymous series. Whence, then, came its unique authority? He concludes that the real achievement of *The Federalist* was to set a tone of "literate civility" for future deliberations on constitutional and public questions down to the time of Lincoln and Douglas (p. 147).

This is a thesis worthy of investigation. It calls for a history of Publius' antecedents in the press, a comparison of Publius with his opponents and his rivals, and a demonstration that Publius was taken as a model by others in discussing political questions before the American public. Such a book might well bear out Furtwangler's contentions.

But this is not the book he has written. Instead he spends much energy early in showing that conventional views of the authority of Publius are untenable. He does not establish an interpretation of Publius' teaching, and thus the rest of the book reflects an uncertainty about it that limits the case.

for its authority. For example, Furtwangler argues that in *The Federalist* Madison and Hamilton attempted as much to explain the Constitution to themselves as to the people. It "reveals not only the voters but the framers in the act of deliberating. This, I believe, is the most enduring source of its authority" (p. 87). But given Furtwangler's ideas about the limits of reason (and therefore deliberation) in politics, we must wonder how authoritative that is (compare pp. 111, 148). The final chapter on *The Federalist* No. 10 is gratuitous and breaks no new ground, with the exception of a highly problematic section showing similarities between the essay and psychologist Erik Erikson's findings about the American family.

Central to Furtwangler's analysis is his claim that *The Federalist* lacks a systematic understanding of politics, that is, a "theory" (p. 43). This claim is denied in the very title of Epstein's book, and Epstein gives a most detailed and scholarly proof that Publius had a political philosophy worthy of our most serious attention.

Epstein challenges those who assume that *The Federalist* is merely a reflection of modern political philosophy: *post Locke, ergo propter Locke*. But he also argues against those who find in Publius the legacy of a "classical republican" tradition, pointing to the book's novelty as the first to recommend a wholly popular government. To sustain these challenges Epstein subjects the argument of *The Federalist* to an exhaustive analysis. Publius, he finds, does not rely solely on mechanical devices or on republican public-spiritedness to solve the problems of popular government. Both interest and virtue are deficient, when taken alone.

Epstein understands *The Federalist* to be divided into two main sections—sections that coincide with the division of the first collected edition into two volumes. This division is quite natural, and Epstein makes good use of it in his analysis. The first volume (1-36) considers "the fundamental issues of liberalism," that is a government that will meet the necessities posed by "accident and force" (p. 8). Volume 2 (37-85) explains the requirements of good government and how the new Constitution meets those requirements.

The treatment of the first volume culminates in a detailed (51-page) analysis of *The Federalist* No. 10. Epstein shows that the improvements Madison proposes in the modern science of politics protect man's capacity of forming political opinions as well as his acquisitiveness. It is commonly supposed that Madison's improvements in the science of politics were a refinement of the mechanism of government depending entirely on the acquisitive faculty for its motive force. But Epstein notes that scholars have paid insufficient attention to the first of the two

kinds of faction named by Madison, that moved by an "impulse of passion." Passion is capable of overturning even the most finely tuned mechanical system. It is Madison's improvement on modern political science that man's "faculty of passionately opining" retains a place in the regime. The operation of this faculty is expanded upon in the second volume, where ambition and honor figure more prominently. The particular structure of the government and its branches allows man's political nature to manifest itself without compromising good government (p. 125). Epstein's demonstration that Publius did not simply articulate interest-group pluralism, but a regime in which prudential judgments would always have to be made, is the book's most important contribution.

The detailed analysis of the text of *The Federalist* is likewise a valuable contribution. Epstein does not make the mistake of trying to evaluate all of the previous major commentaries on *The Federalist*. Yet he does take their arguments into account, and his research is quite thorough. But a more systematic presentation of Epstein's fundamental thesis would be very helpful. Also, some connections that might have been made, for example, between justice, the public good, and the "fundamental principles of the revolution," are not fully made (p. 163). Missing, too, is a discussion of founding—which forms an important, if not critical, theoretical section of the book—and particularly founding as an activity calling forth the highest kind of statesmanship. A treatment of essay 40, where Publius discusses the work of the Convention, might have added much power to Epstein's analysis. But not everything about Publius can be said by one author in one book; it is an accomplishment to have started us thinking about Publius. Epstein's fine contribution is indispensable reading for anyone seeking to understand *The Federalist*.

J. JACKSON BARLOW

The New Federalist Papers

A Commentary on Plato's Protagoras. By Larry Goldberg. (New York: Peter Lang, 1983. Pp. 352. \$33.70, paper.)

Goldberg proposes a study of Plato's *Protagoras* that pays due attention to the dialogue's dramatic context. His procedure appears thematic, with separate chapters on the dialogue's characters—Protagoras and Socrates, but also Simonides—with a chapter on Measure and Community and with a conclusion intended to identify the dialogue's structure. But after the first chapter, this approach falls generally into

abeyance as Goldberg pegs his observations to the course of the dialogue.

Two telling features of Protagoras are his secretiveness and his use of flattery, both of which are necessary because Protagorean sophistry is no true friend of democracy. Despite an egalitarian argument affirming the political capacity of all citizens, what Protagoras in fact promises is rule by the wise. Also noted is the fact that Protagoras is circumstantial and discrete; he is quick to isolate and divide, and thus dispositionally not inclined to see virtue as one.

Socrates sets about to expose Protagoras, not merely the deficiency of his wisdom, but the essential contradictoriness of his character, especially his disdain for—and his likeness to—the many. Socrates' subsequent investigation of the unity of virtue, which seems to be a new beginning, is instead an oblique attack on Protagoras' long oration. The effort by Socrates to establish the oneness of virtue comes close to exposing the injustice of sophistry and thus forces Protagoras to employ rhetorical devices to save himself. The problem facing Socrates both here and elsewhere is that in attacking Protagoras, he looks to be a sophist himself.

In the Simonidean section of the dialogue, which Goldberg gives greater attention to than previous scholars, Socrates is seen to improve upon Simonides' ode with a deliberately errant interpretation, to raise the problem of agreement in discourse, and finally to return the discussion on a higher plane to the question of virtue's teachability. One of Goldberg's concerns here is to determine Simonides' relationship to Protagoras (Simonides is called a sophist by Protagoras) and to Socrates (Socrates undertakes to defend Simonides).

In the closing section of the dialogue, Goldberg examines all of Socrates' arguments for why courage cannot be divorced from knowledge. He maintains that the last of these arguments which introduces hedonism is the most complete, but that the hedonistic assumption is not to be associated with Socrates. It more fittingly belongs to Protagoras, who is further encouraged to make knowledge a more prominent part of his sophistry. The ultimate objective is some sort of political community comprising both the wise few and the demos.

By way of criticism, I would say that Goldberg's commentary suffers from some, although not many, reflections that are at least unnecessary if not unwarranted. His observations on the physician—good, bad, and middle—strike me this way. More seriously though, his commentary lacks a unifying thesis. Various themes, such as community, improvement on the sophist, mode of discourse, and nobility, do surface, but they are

not pursued with the regularity needed to disclose the dialogue's structure, and the concluding chapter that is assigned this task is mainly concerned to show parallels (albeit instructive ones) between the *Protagoras* and the *Clouds* of Aristophanes. Hence, Goldberg cannot spare the reader from asking of the end, "Exactly what is the *Protagoras* all about?" This is a question, however, that might legitimately be put to many of the dialogues, and so the criticism is not meant to be overly harsh. Goldberg's commentary is in fact a worthy piece of analysis and a useful contribution, especially because Platonic scholarship today is dominated by a narrow regard for logical consistency.

PATRICK COBY

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Mill on Liberty: A Defense. By John Gray. (Boston: Routledge & Kegan Paul, 1983. Pp. xiii + 143. \$18.95.)

John Stuart Mill and the Pursuit of Virtue. By Bernard Semmel. (New Haven, Conn.: Yale University Press, 1984. Pp. xi + 212. \$17.50.)

Few of the upheavals in political theory in recent years seem to have affected the reputation of J.S. Mill. Although the general focus in the field has shifted backwards somewhat (especially under the impact of Pocock and Skinner), Mill, with Marx, has remained symptomatic for many of the central positions of both nineteenth- and twentieth-century political debate. Both of these books remind us, however, that there are by now nearly as many Mills as there are Marxes, and that the process of interpretive proliferation is by no means yet ended. For Bernard Semmel, Mill was a more conservative political thinker and actor than is generally assumed, as well as more of a classical democrat in his emphasis on virtue than an enthusiast for the political nonparticipation fostered by commercial society. Early on, Semmel's Mill adopted an essentially activist, Stoic-Christian notion of virtue rather than seeking to transform a more passive Epicurean (and Benthamite) concept of pleasure into an ideal of virtue. Wishing to preserve a sphere for individual effort amidst the then-intense debate over "free will" versus "necessity," Mill in this account (and its emphasis is a very useful one) attacked the fatalism of the Owenites in his youth, and continued this line of criticism later against the Saint-Simonians, Comte, Buckle, Hamilton, and others. Without succumbing to something like Carlyle's concept of the "great man," Mill was concerned to furnish a philosophical ground and offer political protec-

tion for an elite able to ensure moral and social progress through their own excellence.

This concern for virtue and the virtuous led Mill not only to his well-known endorsement of the Hare system of proportional representation to ensure political power to an educated elite, but also to positions dissenting from the Radical party in parliament on such matters as capital punishment (Mill remained in favor) and the Crimean War (which he supported). Semmel does not directly connect Mill's adoption of such views to his "pursuit of virtue," but it is apparently the basis of the relative conservatism imputed to Mill in the Introduction and elsewhere in the text. In taking up the conception of self-development from German idealism, rather, Mill's politics are in Semmel's eyes far more those of the modern social democrat than of the conservative or noninterfering liberal.

For Semmel, this is nowhere more clear than in *On Liberty*, which is interpreted not as a paen to "negative liberty" (against Stephen, Arnold, and many others), but as a plea for the "positive liberty" of self-development, public participation, and self-realization (p. 166), an ideal that does not contradict the demands of Mill's principle of utility even if it does enjoin a preference for higher over lower pleasures. The possibility of a potential conflict between the principles of liberty and utility is also the starting point for John Gray's closely argued attempt to exonerate *On Liberty* from many of the deficiencies usually attributed to it. Arguing in particular against James Stephen, Gray allows that such a conflict cannot arise if we presume Mill to have intended his principle of utility to be essentially a principle of evaluation rather than one directly commanding right action. Claiming that men could never be happy so long as they directly pursued happiness as an object, Mill was hence an "indirect" and not an "act" or "rule" utilitarian, whose principle of utility is therefore not a moral principle of the same kind as liberty, and thus cannot conflict with it in most traditionally assumed ways.

Like Semmel, Gray sees a Stoic idea of freedom as self-determination as lying at the basis of Mill's notion of "autonomy" (the term is Gray's), the idea of individuality defended at length in *On Liberty*. The doctrine of liberty for Gray does not imply any particular theory about the proper limits of state activity, or a special commitment of *laissez-faire*, and as such is compatible with some variants of socialism and social democracy. What is important is that there are no "coercive" limits placed on liberty, not that the principle of negative liberty be expressed in absolute terms. Following Rees, Gray presumes that when Mill limits liberty to not harming others, he means harming their interests, the two most essential of which are security and autonomy. This is the basis

for Mill's theory of moral rights, with the domain of freedom of action being equal to the sum of these rights, and the core of Mill's doctrine of liberty is thus that utilitarian grounds can be given for defending the vital interests of autonomy and security in this manner.

Both of these books therefore defend Mill's value and achievements against his critics. As a more general account, Semmel is particularly strong in his focus on *how* Mill sought virtue, but offers no close analysis of any particular text or new material on Mill's biography. Gray can be read best in conjunction with C.L. Ten's *Mill on Liberty* (Oxford University Press, 1980), which tells us more about what Mill's critics have found in the text, but is itself a less tightly woven argument. Both works clearly demonstrate that a social democratic Mill may yet have much to offer the late twentieth century, and that Mill's reputation remains assured.

GREGORY CLAEYS

University of Hanover (West Germany)

Hannah Arendt: Politics, Conscience, and Evil.

By George Kateb. (Totowa, N.J.: Rowman & Allanheld, 1983. Pp. 204. \$24.95.)

George Kateb has written a provocative, very personal, and at times intellectually agitated study of Hannah Arendt's political thought. Kateb acknowledges Arendt's unique capacity to stimulate thought on a range of discomfiting questions from the nature of politics to the evil of totalitarianism set against the crisis of modernity. He captures the spirit of Arendt's politics by demonstrating that her "life's work is in defense of freedom" (p. 196). He recognizes, although doesn't affirm, Arendt's enormous ambition to recover the lost "treasure" of political action (p. 7). Kateb's explication of Arendt's understanding of political action stands among the best we possess. That power of explication, however, reveals the core passion of the book and the foundations of Kateb's agitation. He defends political philosophy as moral speculation, and representative democracy as a unique form of moral life, against Arendt's subversion of them. Accordingly, Kateb finds the practical import of Arendt's political thinking painful, shocking, blasphemous, and "foreign whenever it is seriously attended to, certainly in America" (p. 1).

Kateb contests Arendt's defense of the "existential supremacy of political action" (p. 7) and her divorce of the pursuit of worldly power and glory from moral constraints. He would exorcise Arendt's heroic and agonistic conception of politics "in order to save our experience" (p. 41).

Kateb decries her linkage of citizenship and participatory politics with the Founders' experience of "public happiness," because that challenges his own celebration of representative democracy.

Kateb fears that any great politics may become vicious, even totalitarian. He affirms representative democracy because it humbles politics, treats all government as "morally dubious, morally inferior" (p. 129). He accuses Arendt of missing the moral authority of representative democracy, of failing to see that limited politics, joined to capitalistic competition, fashions the moral environment in which democratic individuality flourishes. No politicians, but rather Emerson, Thoreau, and Whitman are Kateb's heroes. Last, the consensual foundations of representative democracy legitimize civil disobedience, which, Kateb feels to be, if "not the most heroic [then] as heroic and beautiful as nonmurderous politics can be" (p. 111).

Yet because he substitutes moral discourse for political thinking, Kateb misses much about Arendt and modern democracy. She exposed the political nature of civil disobedience to reveal the power of citizen association that was needed if they were to act effectively in a polity dominated by bureaucratic powers. Kateb admits these vast inequalities, but to save representative democracy bids us act as if it "were completely legitimate, as if its politics were purely the politics of consent" (p. 145). However, even Kateb's democratic heroes, who expressed more contempt for representative democracy than does Arendt, would have rejected such a pretense.

Arendt allowed us access to an American politics greater and more worldly than Kateb's. She reclaimed the republican tradition, with its passion for an active and virtuous citizenship. That tradition, Arendt argued, activated the revolutionary spirit that fashioned the very institutions that Kateb advocates. Arendt felt that for the Constitution to exclude that strong citizenship constituted a real loss. Perhaps, in a representative democracy, civil disobedience is the best politics we can anticipate. But the sometimes politics of an Emerson or a Thoreau pales besides the worldliness of that great American republican, John Quincy Adams. In his diary, he defended a long life devoted to politics:

More than sixty years of incessant, active intercourse with the world has made political movement to me as much a necessary of life as atmospheric air . . . and thus, while a remnant of physical power is left me to write and speak, the world will retire from me before I shall retire from the world. (*Memoirs of John Quincy Adams*, Charles Francis Adams [ed.], J.B. Lipencott, 1874, vol. 10, p. 415)

Kateb attempts to exorcise this sensibility. Arendt recalls the forgotten worldliness of American politics and helps us to recover our public lives and political souls.

MATTHEW F. STOLZ

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Liberalism Reconsidered. Edited by Douglas MacLean and Claudia Mills. (Totowa, N.J.: Rowman & Allanheld, 1983. Pp. xvi + 144. \$32.50, cloth; \$16.95, paper.)

Although there may be spirited debate over precisely what liberalism is and should be, it is relatively well accepted that liberalism is a doctrine under attack and hence on the defensive. The siege mentality of the 1960s may have passed, but the fundamental questions about liberalism's mission and future remain. Indeed, there was a time when liberalism felt it would inherit the earth, but its global status is now one of retreat. And it is by no means obvious that that retreat will be reversed in the near future. However, the necessary, if not sufficient, prerequisite for that reversal is self-clarity on the part of those committed to liberal values, and the present volume represents a serious and provocative exchange that will help foster that clarity.

Some time ago, Alexis de Tocqueville recognized the inherent tension that exists between the liberal commitments to both liberty and equality. This volume is a lively testament to the fact that that tension remains. All of the nine essays confront, and in varying ways try to resolve or overcome, that tension. As it evolves in these essays, the debate is one between proponents of a neutral government that protects the private rights of individuals on the one hand, and proponents of a government that actively intervenes to create at least minimum levels of economic and social equality through redistributive economic programs on the other.

Several of the authors argue that the tension between these competing strands of liberalism can neither be resolved nor indefinitely maintained. Walter Berns in "Taking Rights Frivolously" argues that liberalism should stress liberty understood as the pursuit of happiness privately defined. The end of government is merely to promote the environment within which diverse private ends can be pursued. Theda Skocpol in "The Legacies of New Deal Liberalism" and Christopher Lasch in "Liberalism in Retreat" argue in favor of jettisoning the liberal commitment to individualism in favor of promoting the grounds for a more cohesive community, a large component of which would be to foster greater

social equality that would make a more democratic community possible. Of course, this latter approach raises the spectre of a significant encroachment on individual liberty, and indeed potentially of majority tyranny, an issue Amy Gutman addresses in "How Liberal Is Democracy?" by reflecting on the relation between liberalism and democratic government.

In varying ways the remaining authors try to show that the commitment to both liberty and equality need not cause an irreconcilable tension and thereby the necessary choice between one side of the tension or the other. Ronald Dworkin in "Neutrality, Equality and Liberalism" argues that commitments to both governmental neutrality as regards private individual behavior and to the perception of a responsibility to reduce economic inequality can both be traced to a more fundamental liberal principle. Hence, the seeming tension dissolves. Through a reading of Locke, Nathan Tarcov in "A 'Non-Lockean' Locke and the Character of Liberalism" tries to show that both the commitment to liberty and property rights and recognition of the need to foster community can be found in Locke, the most frequently cited of all liberal authors. And in different ways Marshall Cohen in "Toward a Liberal Foreign Policy," Mark Sagoff in "Liberalism and the Law," and Michael Williams in "Liberalism and Two Conceptions of the State" also try to take off the sharpest edges of the conflict between commitment to neutral government and reformist aspirations.

Throughout, the essays are cogent and relevant, and in a way that is uncharacteristic of volumes of this kind, the authors address each other directly and at times specifically. Hence, this volume will be valuable not only for the much needed discussion of the nature and meaning of contemporary liberalism it will foster, but also as a useful classroom and teaching tool.

GREGORY B. SMITH

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The Sexual Politics of Jean-Jacques Rousseau.

By Joel Schwartz. (Chicago: University of Chicago Press, 1984. Pp. xi + 196. \$17.00.)

Of the many and varied aspects of Rousseau's thought, few are as controversial and seemingly repugnant as his views on human sexuality, women, and the family. In recent years, Rousseau has been almost universally assailed by feminists who see in most of his works, particularly in *Emile*, the musings of a pathological misogynist. In this outstanding book, Schwartz takes issue with this preemptory rejection of Rousseau and

offers a probing and important reinterpretation of Rousseau's sexual politics.

Although Schwartz is clearly a fan, it is not his purpose in this work to launder or make palatable Rousseau's views on women. Indeed, at times Schwartz is almost at a loss to explain the divergent and paradoxical positions Rousseau takes on women and sexuality at various points in his writings. Rousseau, Schwartz argues, views human sexuality as the "bridge to politics" (p. 28), but understanding how that bridge is crossed is complicated by the fact that Rousseau holds two distinct and contradictory teachings about sexuality: "one teaching celebrates sexuality, the other condemns it" (p. 1).

Schwartz argues convincingly that Rousseau views sexuality as more fundamental a foundation of civil society than even agriculture or economics, notwithstanding Rousseau's statement in the *Social Contract* that the "true founder of civil society was the first person who fenced off a plot of ground." Thus, Rousseau holds in his first view of sexuality, as identified by Schwartz, that the mutual dependence bred by sexual relations is beneficial in providing the social impetus. However, because Rousseau always views dependence on other people as the bane of human freedom, he must also resent the development of the family as a consequence of female sexual manipulation because it takes away the possibility of true individual liberty. This is the second view of sexuality in Rousseau that Schwartz identifies, and it is the basis of criticism of Rousseau by feminists from Mary Wollstonecraft to Susan Okin.

Most of Schwartz's book is an attempt to reconcile these two opposed views of human sexuality and women's use of it that Rousseau sees as basic to the political history of the species. This reconciliation is perhaps impossible to achieve other than to argue, as Schwartz does, that for Rousseau both the family and the state are artificial, as opposed to natural, human constructs. Such a conclusion is no less satisfying than Rousseau's fundamental political idea that politics is unnatural but nevertheless capable of bringing much virtue and excellence to human life. The same is true of sexual relations, although like politics generally, such relations are also capable of becoming the chains of modernity that strip modern man—and woman—of their natural freedom.

Perhaps the most important lesson of Schwartz's book is that Rousseau's ideas concerning sex and women are a maze that must be entered and traversed successfully if we are ever to understand his larger political theory. In his chapters on the natural and political history of sexuality, and Rousseau's deep-seated ambiva-

lence to this facet of human beings, Schwartz points the way to a fuller realization of Rousseau's political thought. Although in his conclusion he mirrors his subject's ambivalence on the score of sexual differentiation versus androgyny, Schwartz nevertheless contributes greatly to our understanding of Rousseau's (and our own) search for individual freedom and the barriers raised in that search by the inequities of sexual politics.

RICHARD P. HISKES

University of Connecticut

The Politics of Locke's Philosophy: A Social Study of "An Essay Concerning Human Understanding." By Neal Wood. (Berkeley: University of California Press, 1983. Pp. xiv + 241. \$26.50.)

There are two books wrestling for control between the covers of this little volume. The first is an unexceptionable and sometimes insightful account of the social content of Locke's philosophy. The second is a problematic argument that Locke's views are illuminated by the imposition of Marxist categories. Wood is of two minds throughout; I suspect he never finally decided what he wanted to say, and the resulting text is contradictory, not dialectical.

Wood wants to rescue the *Essay* from today's analytic philosophers. He is willing to concede that it contains technical discussions of rarefied epistemological problems, but he wants to focus on the social ramifications of Locke's theories. The empiricist project for pursuing knowledge summons up a view of an individual critically evaluating the evidence, refusing to knuckle under to Aristotelian schoolmen or noble lords. Locke is fully aware, more I think than Wood acknowledges, that the accumulation of knowledge is a social process; for all the brilliance of "the incomparable Mr. Newton," we need a society where people can weigh competing evidence freely.

That means we need to dispense with the non-sensical maxims of the schools, with heated devotion to religious sects, and with mindless repetition of past verities. Wood suggests, I think quite rightly, that Locke is following up Bacon's attack on the idols of the mind. Again rightly, Wood

sees that Locke's chief concern is religious faction. Puritan and Catholic fanatics not only make for religious civil war; they also cram their followers' heads full of nonsense, preventing them from pursuing the truth.

So far, so good. But sometimes Wood wants to cram the richly varied dimensions of Locke's social concerns into one category and unmask Locke as the teacher of the bourgeoisie. After discussing science, religion, politics, the changing reading public, and more, Wood concludes, "The *Essay*, therefore, both in England and America, had a definite appeal for the emergent bourgeoisie" (p. 64). After correctly arguing that the parties that so exercise Locke are chiefly religious sects, Wood suddenly announces that Locke perhaps opposed the Puritan Revolution because of "its momentary threat to the propertied and promise of fundamental change to the common people" (p. 110). A cognitive ego out to accumulate true beliefs is, we are told, just like a conative ego out to accumulate pleasure for itself, and that's just like capital accumulation (p. 160). Locke's attack on scholastic jargon and pettifoggery is "by implication" a rejection of natural inequality (p. 128).

This simply will not do. Here Wood deliberately and perversely collapses his thesis; Locke's concerns were surely much broader. Only a flat misreading of the *Education* will make Locke a believer in the unalterability of self-interest. And although Wood uses it freely, *bourgeois* has become all too flexible a concept, more a term of abuse than a precise category.

Sometimes (pp. 118, 126, 178, 180) Wood distinguishes between finding Locke's intentions and charting the likely effects of the *Essay*. Here, if he likes, Wood is surely free to speculate on economic class, but his view is still murky because his Marxism is muddled. Wood wants to "eschew a dogmatic and mechanical approach," but he insists that "class is a fundamental lever of social change" (p. 13). He thinks J.H. Hexter and Lawrence Stone dismiss class because "they mistakenly conceive of class as an entity or structure instead of a relationship" (p. 12), but he himself characterizes class as an entity (p. 13). Marxism here, unhappily, is an idol of the theater that interferes with an otherwise fine study.

DON HERZOG

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Empirical Theory and Methodology

Strategic Planning and Forecasting: Political Risk and Economic Opportunity. By William Ascher and William H. Overholt. (New York: John Wiley & Sons, 1983. Pp. xii + 311. \$29.95.)

According to the authors, political forecasting attempts to anticipate three classes of trends or events. First, there is the distribution of power among institutions and individuals. A second focus is the climate of politics that encompasses how specific political events are played out. Finally, political forecasting examines the continuities and changes in authoritative policy and in actor strategies. Ascher and Overholt seek to provide both an overview of the field of political forecasting and to reorient our thinking about the subject, moving the discussion away from the relative merits of competing mathematical models to a study of political forecasting in the context of the policy process. The authors contend that it is only in this way that forecasting can regain its credibility and become useful to policymakers.

Ascher and Overholt succeed admirably on the first point. They provide a highly readable and thorough overview of forecasting methodology, the logic of political forecasting, and the relationship of forecasting to planning. Central to the authors' review of political forecasting is the assertion that the principal task of the forecaster is to describe the future environment in terms that are useful to the policymaker. This is best done by viewing forecasting as a heuristic device that has its greatest impact on the policymaker through the presentation of scenarios. The construction of scenarios is urged over the employment of complex models because of the need for comprehensiveness in many forecasting efforts and for the ability of scenarios to project the dynamics of politics rather than just project a trend or predict an event. The most effective analytical device for structuring scenarios is held to be viewing politics as organizational activity.

Ascher and Overholt are less successful in meeting their second objective, that of anchoring political forecasting in the context of the policy process. Their treatment of this subject tends to be disappointing. It does not take us beyond our present understanding of the difficulties of integrating forecasting, planning, and policy. Moreover, issues discussed at length in the literature on intelligence estimating and surprise (areas certainly encompassed by political forecasting) are

left largely untouched. For example, the authors note the difference between long-range and short-range planning, but no parallel discussion exists regarding the distinction between short- and long-range forecasting. This omission is significant because of the differing problems encountered in integrating the two into the policy process. Long-range forecasting faces neglect, while short-term forecasting faces politicalization.

To some extent the problems encountered here stem from the structure of the book. In its method of presentation, *Strategic Planning and Forecasting* is typical of many current business-oriented texts. Frequent section headings and subdivisions are employed to cover the subject matter. The result is a tendency to highlight the central features of a problem rather than to explore them in an in-depth and integrated fashion. This point can be illustrated through the use of case studies. Ascher and Overholt employ three well-written case studies on political development to argue in support of a heuristic and scenario centered forecasting methodology. They also begin their book by introducing a thoughtful model linking forecasting and strategic planning into a coherent whole, yet, none of the case studies constitutes an effort to demonstrate the practical utility of this approach. They are instead all written as self-contained essays.

In sum, Ascher and Overholt have provided us with a valuable overview of political forecasting and pointed us in the direction of more thoroughly linking forecasting to policymaking. The nature of those linkages, however, still eludes us.

GLENN P. HASTEDT

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The Evolution of Cooperation. By Robert Axelrod. (New York: Basic Books, 1984. Pp. x + 241. \$17.95.)

Robert Axelrod's *The Evolution of Cooperation* is a unique effort to employ the prisoner's dilemma in exploring the possibilities of extending and improving the limited cooperative proclivities of humans. Axelrod is fascinated and possibly mesmerized by the analytical utility of game theory in general, and the prisoner's dilemma in particular. Fascination with games of mixed motives as the primary means of gaining leverage

on promoting cooperation is deserving of study in and of itself. For scholars appreciative of harmony, cooperation, and peace, the Hobbesian state of nature or zero-sum model possesses an irresistible appeal and immediacy; the more sanguine and realistic non-zero-sum model of Locke holds less interest. Perhaps, the intellectual challenge of meeting the Hobbesian authoritarian solution is a more demanding task. Axelrod seems to vacillate between the two models, using the zero-sum game to dramatize the imperativeness of cooperation and non-zero games as the means by which we can resolve our differences. The problem is twofold: Everyone chooses the more profitable defect strategy, and/or most people fail to see the gains of cooperation.

Axelrod's inquiry proceeds with an interesting combination of historical cases including trench warfare in World War II, a study of biological evolution, game theory proper, and a fascinating computer tournament involving many well-known students of game theory. From these sources, Axelrod discovers that the best strategy for participants in prisoner's dilemmas is pursuit of a "tit for tat" strategy, that is, the player cooperates on his first move and then does whatever the other player did on the previous move. Axelrod then offers advice to participants and reformers in Part 4 and Part 5 on how to promote cooperation in mixed motive situations. This is fascinating material, especially for social psychologists and game theorists. Axelrod claims that cooperation can be initiated and encouraged by enlarging the shadow of the future, changing the payoffs, teaching people to care, teaching reciprocity, and improving our recognition abilities of mutual gains—advice proffered by most authors of books on how to win friends and influence people.

I find it odd that Axelrod and his fellow game theorists seldom inquire into the market economy as a means of cooperation; indeed, one can make a powerful case that the market economy is the most robust institution for cooperation ever to have evolved. If this is the case, then a history of market economies done in terms of the prisoner's dilemma might shed not only a great deal of light on the evolution of markets, but it also might add to our capacity to promote cooperation. How did markets emerge from the prisoner's dilemma of ancient times? How do markets enable participants to overcome the defect option?

Although Axelrod does not list markets in the Index, the text does refer to a handful of market examples including cartels; interestingly, all deal with market failure, that is, imperfect markets. (Almost all applications of game theory in economics are to oligopoly!) And, Axelrod correctly notes, these are instances of "bad" cooperation, because they reduce the supply of

goods and increase consumer prices for non-members of the cooperative group. Trade wars are also briefly mentioned, but they are really examples of inefficient public policies adopted as much for the advancement of the interests of politicians as for inefficient home industries.

But if normal cooperation is viewed as a good thing, why do game theorists not buttress their case with citations from the history of free-market competition? That markets promote cooperation has been a powerful theme in the writings of all its advocates from Adam Smith to von Hayek. Markets, it would seem, go unappreciated because of the transparency of self-interest and the nonobvious workings of the hidden hand of exchange. Accordingly, markets appear as divisive, exploitative, manipulative, and uncertain—the opposite of conscious cooperation. Still, Axelrod's advice on promoting cooperation would seem best exemplified in everyday exchange activities of the marketplace. Surely the market is the best example we have of a non-zero-sum game. Politics, on the other hand, converts the latter game into zero-sum situations.

Despite my stricture, Axelrod's thoughtful and lucid book is a worthy addition to the literature of game theory and, indeed, a worthy addition to political theory. It deserves the widespread favorable attention it has already received.

WILLIAM C. MITCHELL

University of Oregon

The Spatial Theory of Voting: An Introduction.

By James N. Enelow and Melvin J. Hinich.
(New York: Cambridge University Press, 1984.
Pp. xv + 238. \$39.50.)

The title of this book indicates it will introduce the uninitiated to the structure of spatial theories of voting. This notion is reinforced by the authors' claim to bring the reader "step by step" to "a full appreciation of the contributions spatial theory has made to our understanding of voting" (p. 1). The incremental approach breaks down, however, after the third chapter of this 10-chapter book. Neither does this work, like many others in positive or formal political theory, live up to its promise that "the level of mathematics needed to express the important results in this field is surprisingly modest" (p. xiv). Too often claims to this effect are made when they are unwarranted, usually accompanied by the assertion that the book is written for the general reader. Such promises are misleading when the authors consider, as in the case of the book under review, a "modest" mathematical background to include geometry, quadratic equations, probability

theory, and calculus. The reason I dwell on this point is that positive or formal theory has penetrated virtually every subdiscipline of political science. Thus, every student of politics can benefit from the non-obvious conclusions that characterize this rapidly burgeoning literature. But many are intimidated by its mathematical nature. Therefore, is it extremely important that a clear and accurate statement describe for potential readers when a particular work is an introductory text, a nontechnical discussion that uses the spinoff from the literature, or a technical discussion for specialists in the field. Enelow and Hinich's book falls into the latter category.

The major objective of this book is to develop a new spatial theory of voting, one that can explain the dynamics underlying both committee voting and voting in mass elections. Thus, the core of the book is chapter 4, where the authors develop their new model. Here Enelow and Hinich successfully "rebuild the multidimensional theory of candidate competition in mass elections" (p. 79). This is accomplished through the skillful unfolding of the relationship that exists between the classic right-left ideological predictive dimension voters use to label candidates with the perceptual views of individual voters. Then the link between policy issues and the ideological predictive dimension is demonstrated. In this fashion, Enelow and Hinich manage, with methodological agility, to lay the foundation for a broad theory of party competition through the use of a spatial model of candidate competition that explains how "voters estimate the issue positions of each candidate by translating the candidate's position on an underlying predictive dimension into a set of positions on the policy issues of the campaign" (p. 54). Chapter 5 incorporates nonpolicy issues or the fixed issues of a campaign such as gender, religion, peace, and prosperity into the model. In chapter 6, the model is generalized to encompass committee voting. Chapter 7 demonstrates how the model can be applied under conditions of candidate-induced uncertainty, the perceptual uncertainty of voters about the point along the ideological predictive dimension where candidates ought to be located, and predictive uncertainty about what candidates will do when elected. Chapter 8 provides an interesting discussion about the impact institutions can have on voting outcomes. And chapter 9 provides empirical verification of the model through an analysis of the 1976 and 1980 presidential elections.

Although I cannot recommend this book as an introduction to spatial theory, it does have something of value to specialists in the field.

THOMAS J. MORTILLARO

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Social Impact Assessment Methods. Edited by Kurt Finsterbusch, Lynn G. Llewellyn, and C.P. Wolf. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 318. \$29.95.)

This book offers an inventory of social impact assessment (SIA) methods divided into four parts: a paradigm of SIA, primary data collection methods, secondary data collection techniques, and special problems and methods. In Part 1, Wolf presents a rational problem-solving scheme of "anticipatory research" for assessing potential winners and losers and providing "use knowledge" to mitigate and manage "people impacts." Michael J. Carley discusses a number of numerically oriented methods and argues for more extensive preprogram assessments. Cynthia Flynn et al. present a group ecology method for assessing social and economic change linking the direct effects of a project to the probable social and economic responses of the people affected.

Part 2 begins with a thorough cataloging by Finsterbusch of the multiple SIA uses of survey research. Roy Roper discusses the role of ethnography, focusing on the effects on both individuals and families of potential sociocultural disruptions. Annabelle Bender Motz explores in Part 3 the virtues and drawbacks of historical documents, the use of which is often limited by their questionable validity and reliability. Thomas Dietz and C. Mark Dunning examine demographic impact assessment methods, with emphasis on comparing a baseline "without project" scenario against "with project" scenarios for assessing the impact of policy-induced demographic changes on community infrastructure and local service demands. Demographic simulation models are also presented. Carley discusses the surprisingly limited overlap between SIA and social indicators research.

Part 4 addresses six special topics. Steve H. Murdock and F. Larry Leistritz evaluate the informational characteristics, methodologies, and applicability of 13 widely used socioeconomic impact assessment models. Rabel J. Burdge elucidates the role of direct public input from key informants, advisory groups, community forums, nominal group exercises, Delphi techniques, questionnaires, and jury panels, and the use of secondary information and agency records. John W. Lounsbury, Kent D. Van Liere, and Gregory J. Meissen provide a psychosocial "attitude theory" framework for SIA. Jeryl Mumpower and Barry F. Anderson discuss the twin problems of random judgmental inconsistency and systematic judgmental bias. Tentative solutions to both problems are explored, notably the triangulation of methods for converging toward correct solutions. James F. Palmer highlights the visual aspects of

impact assessment. Finsterbusch concludes by emphasizing the ethical dimensions of SIA and discussing techniques for multiobjective decision making in a somewhat abstracted presentation of utilitarian democratic theory and principles of ethical pluralism. He argues for three assessment cycles in SIA work: eliminating unacceptable alternatives, eliminating those that are unpromising, and selecting the best remaining option. As with much else in the SIA literature, this is much easier to put into print than it is to implement.

This is not a statistically oriented book, unlike others of the evaluation genre, and the discussion is often overly general. This is not to say that the content is unrealistic, only that it sometimes lacks specificity. One particular difficulty is the diverse nature of the methods that are presented. Eclecticism is a two-edged sword, producing a lack of focus as well as a wealth of alternative approaches. SIA is therefore difficult to define, except in specific operational contexts. One source of this lack of clarity is the often deliberately ambiguous wording and consequent latitude for interpretation in national legislation. What should be criticized for lack of definitiveness perhaps is not the methods of impact assessment, but rather the political process that creates the problems that must be addressed by those methods.

MACK C. SHELLEY, II

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Contemporary Public Policy Analysis. By Stuart S. Nagel. (University: University of Alabama Press, 1984. Pp. xiv + 174. \$17.75, cloth; \$7.50, paper.)

Stuart Nagel's latest book on public policy analysis is taken from a series of lectures given in 1982 at the University of Alabama. The lectures and the book try to do two things: provide an introduction to the field of public policy analysis, and discuss benefit cost analysis as a specific technique in policy analysis. The book succeeds in the introductory aspects, but, unfortunately, fails in the analysis portion.

Taking the successful portion of the book first, Nagel presents a comprehensive list of institutions, periodicals, and approaches to the study of policy analysis. He notes, for example, the Kennedy School's emphasis on economics, Berkeley's emphasis on political science, and Northwestern's social-psychological approach to policy analysis. He presents good descriptive material on journals and publishers in policy analysis in his chapter,

"Trends in Policy Analysis." There also is a brief but interesting description of the differences between business analysis and analysis for the public sector. The concluding chapter on ethics is informative, and throughout the book interesting examples are provided from the area of Nagel's expertise, criminal justice. But even within these relatively good sections of the book, there is excessive "list-making," which is annoying to the reader and distracting to the flow of the writing.

The bulk of this book is a rather tedious and drawn out application of benefit cost techniques to various problems in policy analysis. In fact, the book might be more accurately retitled "Benefit Cost Analysis in Contemporary Policy Studies." The analysis would be useful for readers who are interested in reviewing basic algebraic principles and applying them to situations with known and unknown variables, monetary and nonmonetary benefits, mutually exclusive and combinations of projects, and all permutations thereof. But most readers are not interested in such a review and could work out the applications themselves with some basic knowledge of algebra.

Furthermore, there is nothing new or exciting about the discussion of scholarship in the area of benefit cost analysis. Nagel points out that using the formula benefits minus costs ($B - C$) is superior to dividing benefits by costs (B/C) in formal analysis. But this is nothing new; economists have been using this formula for the past 20 years when comparing multiple projects. The only readers who might find this information interesting are government agency analysts who have not kept up with the literature on benefit cost analysis.

In sum, Nagel's latest book is a disappointment.

JAMES S. LARSON

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The Parable of the Tribes: The Problem of Power in Social Evolution. By Andrew Bard Schmookler. (Berkeley: University of California Press, 1984. Pp. xi + 400. \$19.95.)

This wide-ranging, cross-disciplinary book is frustrating to review. On the one hand, it deals with significant issues—the evolution of civilization and the state, the causes of war, the roots of alienation. On the other hand, it is often a collection of commonplaces from various disciplines surveyed and an oversimplification or inaccuracy in exposition.

The basic organizing concept of the book is, as the title goes, "the parable of the tribes." This

parable is based upon the following situation (p. 21):

Imagine a group of tribes living within reach of one another. If all choose the way of peace, then all may live in peace. But what if all but one choose peace, and that one is ambitious for expansion and conquest?

The end result is that the tribes compete for power—"A selection for power among civilized societies is inevitable" (p. 22). In one sense, the basic argument underlying the book is little more than a modest variation on the security dilemma, or, more classically, Rousseau's stag hunt metaphor (see Kenneth Waltz, *Man, the State and War*, Columbia University Press, 1959).

Schmookler argues that although human biology and psychology shape human behavior considerably, social and cultural norms may override these, and consequently produce the circumstances in which humans come to live in a civilization that is not consistent with their nature. The result? Alienation. However, Schmookler's discussion is partially undermined by some rather questionable assumptions. For example, at one point he embraces the concept of "group selection" to explain the evolution of human altruism, an explanation not acceptable to most contemporary evolutionary theorists. His "conquest theory" of the evolution of civilization and the state is in the mainstream of anthropology as far as I can tell, but the arguments go little beyond what Marvin Harris and John Pfeiffer, among others, say.

An important concept for Schmookler is "synergy." He defines a synergistic interaction as "one in which each part functions in a way that enhances the welfare of the other parts as well as its own" (p. 216). This concept is part of a plea by Schmookler for humans to realize the necessity of understanding their place in nature. His actual use of synergy, though, does not seem to depart too much from a standard systems viewpoint. His analysis does not examine the issue in nearly as detailed a fashion as Peter Corning's *The Synergism Hypothesis* (McGraw-Hill, 1983).

Later in the book, Schmookler offers critiques of both capitalism/liberalism and Marxism. One difficulty is his lack of awareness of refinements in both ideological camps. His attack on Marx is a full-scale assault on a straw man. Twentieth-century Marxists such as Gramsci and Habermas simply have to be considered given Schmookler's battle plan against Marxism.

Schmookler's lack of expertise may be most evident in his concluding chapter—a mere six pages, which one would think to be the culmination of the argument. He concludes that humans must better integrate their nature (rooted in their

hunting-gathering past) with the demands of advanced civilization. This is surely anticlimactic; worse, there are no hints as to how this is to take place given the dynamic of "the parable."

All of these problems notwithstanding, this is an interesting book. Schmookler dares to attempt a grand synthesis; his failure to achieve his goals is thereby rendered less significant. His ambitious effort may make this book pleasant reading.

STEVEN A. PETERSON

Alfred University

Genius, Creativity, and Leadership: Historiometric Inquiries. By Dean Keith Simonton. (Cambridge, Mass.: Harvard University Press, 1985. Pp. ix + 231. \$20.00.)

This book sets out to bring together, evaluate, and to suggest the significance of published material—principally but not exclusively quantitative—dealing with the factors that appear to be associated with artistic and scientific creativity, and with remarkable political leadership. Simonton has himself in the past decade or so published some of the best work in this area.

Factors such as the effects of heredity, intelligence, goodness or badness of character, personality type, level and type of education, productivity, age, *zeitgeist*, and political turmoil are all evaluated. The problems that inevitably arise concerning historical accuracy, historical comparability, and conceptual clarity are presented carefully.

Simonton's discussion of family influences—birth order, orphanhood, and family heritage—is exemplary. Francis Galton's, Havelock Ellis's, and other studies indicate, for example, that first-born children are more apt to be major scientists or artists, whereas political leaders are much more likely to be found among middle children. The question of only-child status, and of revolutionary as opposed to integrative leadership, are also considered, as is the effect of preferential educational treatment for eldest children. Simonton suggests that revolutionary leadership is more associated with living in a large family (which of course increases the chance of being other than an eldest child) than of being a middle child per se.

One melancholy conclusion that parents ambitious for their children's success should note is that the very best thing they can do to help their children, regardless of birth order, is to die, the sooner the better: "the proportion of orphanhood among . . . geniuses far exceeds those found in comparison groups" (p. 29). It is not strictly necessary to die; the loss can also be by events such as divorce or alcoholism. As Simonton

notes, however, it is the death of a same sex parent that has the major effect, and, most of the instances he discusses being male, this event may lead to an even more important influence: "Poets, dictators, and military heroes have one biographical datum in common: they exhibit a high percentage of dominating, possessive or smothering mothers" (p. 29).

Parenthetically, in his section on famous families, Simonton argues that when intelligence and education (both family-linked) are controlled, family itself has virtually no independent explanatory power.

In such ways this book seeks to use the data of history and the methods of the social sciences to move toward generalizations about the social determinants of creativity. More important perhaps than the tentative (although very useful) generalizations that Simonton offers, he shows that systematic inquiry can produce firm and testable generalizations of good explanatory and predictive utility.

ROBERT S. ROBINS

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Raymond Wolfinger, University of California, Berkeley
Charles E. Young, Chancellor, UCLA

The American Political Science Review

Vol. 79

September 1985

No. 3

CONTENTS

<i>Marxism and Secular Faith</i>	Richard J. Arneson	627
<i>Althusser's Marxism without a Knowing Subject</i>	Steven B. Smith	641
<i>Alexis de Tocqueville on Political Science, Political Culture, and the Role of the Intellectual</i>	James Ceaser	656
<i>Assessment of Political Power in the Israeli Knesset</i>	Amnon Repoport & Esther Golan	673
<i>Multiparty Equilibria under Proportional Representation</i>	Joseph Greenberg & Shlomo Weber	693
<i>Risk and Uncertainty as Factors in the Durability of Political Coalitions</i>	John R. Wright & Arthur S. Goldberg	704
<i>The Decline of Class Revisited: Class and Party in England, 1964-1979</i>	Jonathan Kelley, Ian McAllister, & Anthony Mughan	719
<i>Party Goals and Government Performance in Parliamentary Democracies</i>	Kaare Strom	738
<i>An Adaptive Model of Bureaucratic Politics</i>	Jonathan Bendor & Terry Moe	755
<i>Vulnerabilities and Responsibilities: An Ethical Defense of the Welfare State</i>	Robert E. Goodin	775
<i>The Responsive Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974</i>	Eric M. Uslaner & M. Margaret Conway	788
<i>Expectations and Preferences in Presidential Nominating Campaigns</i>	Larry M. Bartels	804
Communications		
<i>Comment on The Problem of Strategic Voting under Approval Voting</i>	Steven J. Brams & Peter C. Fishburn	816
<i>Reply to Brams and Fishburn</i>	Richard G. Niemi	818
<i>Rejoinder to Niemi</i>	Steven J. Brams & Peter C. Fishburn	819
<i>Comment on Wilson</i>	John T. S. Keeler	819
<i>Reply to Keeler</i>	Frank L. Wilson	822
<i>Reply to Calvert and Wilson</i>	Alphons J. C. van de Kragt, John Orbell, & Robyn M. Dawes	823
<i>Comment on Carson and Oppenheimer</i>	Robert A. Bernstein	825
<i>Comment on Carson and Oppenheimer</i>	David R. Morgan	826
<i>Comment on Carson and Oppenheimer</i>	Arthur Sanders	827
<i>A Reply to Our Critics</i>	Richard T. Carson & Joe Oppenheimer	827
Book Reviews		831
American Government and Politics		831
Comparative and Other Area Studies		859
International Relations		889
Normative Theory		907
Empirical Theory and Methodology		918

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BOOK REVIEWS

American Government and Politics

- Abramowitz, Alan I., & Walter J. Stone. *Nomination Politics: Party Activists and Presidential Choice*. Emmett H. Buell, Jr. 831
- Barber, Sotirios A. *On What the Constitution Means*. Ronald C. Kahn 832
- Berry, Jeffrey M. *Feeding Hungry People: Rulemaking in the Food Stamp Program*. Robert K. Whelan 832
- Bridges, Amy. *A City in the Republic: Antebellum New York and the Origins of Machine Politics*. Gerald Benjamin 833
- Buffa, Dudley W. *Union Power and American Democracy: The UAW and the Democratic Party, 1935-1972*. Alan Draper 834
- Coser, Lewis A. *Refugee Scholars in America: Their Impact and Their Experiences*. Ian Shapiro 835
- Crotty, William, & John S. Jackson III. *Presidential Primaries and Nominations*. David G. Lawrence 836
- Dery, David. *Problem Definition in Policy Analysis*. Lawrence M. Mead 837
- Flammang, Janet A., ed. *Political Women: Current Roles in State and Local Government*. Eileen L. McDonagh 838
- Golembiewski, Robert T., & Aaron Wildavsky, eds. *The Costs of Federalism: Essays in Honor of James W. Fesler*. William E. Hudson 839
- Greenwood, Ted. *Knowledge and Discretion in Government Regulations*. Duane Windsor 840
- Hargrove, Erwin C., & Michael Nelson. *Presidents, Politics, and Policy*. John P. Burke 840
- Hess, Stephen. *The Government/Press Connection: Press Officers and Their Offices*. Robert E. Gilbert 841
- Johnson, Chalmers, ed. *The Industrial Policy Debate*. David Menninger 842
- Johnson, Charles A., & Bradley C. Canon. *Judicial Policies: Implementation and Impact*. William Haltom 843
- Joslyn, Richard. *Mass Media and Elections*. Charles M. Tidmarch 844
- Kellerman, Barbara. *The Political Presidency: Practice of Leadership from Kennedy through Reagan*. Robert Locander 845
- Kessel, John H. *Presidential Parties*. Alan Shank 846
- Kiewiet, D. Roderick. *Macroeconomics and Micropolitics*. David H. Everson 846
- Krukones, Michael G. *Promises and Performance: Presidential Campaigns as Policy Predictors*. Larry Boyle 847
- Lamis, Alexander P. *The Two-Party South*. Charles D. Hadley 848
- Light, Paul C. *Vice-Presidential Power: Advice and Influence in the White House*. Ryan J. Barilleaux 849
- McClosky, Herbert & John Zaller. *The American Ethos: Public Attitudes toward Capitalism*. Robert Y. Shapiro 850
- McFarland, Andrew S. *Common Cause: Lobbying in the Public Interest*. Darrell M. West 851
- Melanson, Philip H. *The Politics of Protection: The U.S. Secret Service in the Terrorist Age*. James L. Waite 851
- Olson, Susan M. *Clients and Lawyers: Securing the Rights of Disabled Persons*. Edward V. Heck 852
- O'Toole, Laurence J., & Robert S. Montjoy. *Regulatory Decision Making: The Virginia State Corporation*. Fred Thompson 853
- Preston, Michael B. *The Politics of Bureaucratic Reform: The Case of the California State Employment Service*. Luther F. Carter 854
- Rosenstone, Steven J. *Forecasting Presidential Elections*. Gary King 855
- Spitzer, Robert J. *The Presidency and Public Policy: The Four Arenas of Presidential Power*. Michael A. Genovese 856
- Sullivan, Terry. *Procedural Structure: Success and Influence in Congress*. Burdett Loomis 856

Taylor, Serge. <i>Making Bureaucracies Think: The Environmental Impact Statement Strategy of Administrative Reform</i>	A. B. Villanueva	857
Zigler, Edward F., Sharon Lynn Kagan, & Edgar Klugman, eds. <i>Children, Families, and Government: Perspectives on American Social Policy</i>	Mary Anne Cavicchi	858

Comparative and Other Area Studies

Aronoff, Myron J., ed. <i>Political Anthropology Yearbook</i> , Vol. 2, <i>Culture and Political Change</i> ; Vol. 3, <i>Religion and Politics</i>	Richard Handler	859
Ascher, William. <i>Scheming for the Poor: The Politics of Redistribution in Latin America</i>	Robert H. Dix	
Benjamin, Roger, & Robert T. Kudrle, eds. <i>The Industrial Future of the Pacific Basin</i>	Glenn R. Fong	861
Berrington, Hugh, ed. <i>Change in British Politics</i>	Jeff Archer	862
Bogdanor, Vernon, ed. <i>Coalition Government in Western Europe</i>	James A. Dunn, Jr.	863
Boulle, L. J. <i>Constitutional Reform and Apartheid: Legitimacy, Consociationalism and Control in South Africa</i>	Allan D. Cooper	864
Burklin, Wilhelm P. <i>Grüne Politik: Ideologische Zyklen, Wähler, und Parteiensystem</i>	Russell J. Dalton	865
Butler, David, & Denis Kavanagh. <i>The British General Election of 1983</i>	Jeffrey B. Freyman	866
Clarke, Harold D. et al. <i>Absent Mandate: The Politics of Discontent in Canada</i>	Barry Cooper	866
Friedman, Douglas. <i>The State and Underdevelopment in Spanish America: The Political Roots of Dependency in Peru and Argentina</i>	Paul G. Buchanan	867
Gelman, Harry. <i>The Brezhnev Politburo and the Decline of Detente</i>	John M. Carfora	868
Gertz, Cherry, Carolyn Baylies, & Morris Szeftel, ed. by Cherry Gertz. <i>The Dynamics of the One-Party State in Zambia</i>	James R. Scarritt	869
Griffin, Anne. <i>Quebec, the Challenge of Independence</i>	Keith Hendersen	870
Ham, Christopher, & Michael Hill. <i>The Policy Process in the Modern Capitalist State</i>	Joyce Marie Mushaben	870
Kahler, Miles. <i>Decolonization in Britain and France: The Domestic Consequences of International Relations</i>	William Safran	871
Kemezis, Paul, & Ernest J. Wilson, III. <i>The Decade of Energy Policy: Policy Analysis in Oil-Importing Countries</i>	John Heilman	872
Large, David C., & William Weber, eds. <i>Wagnerism in European Culture and Politics</i>	John Bokina	873
Lewin, Leif. <i>Ideologi och Strategi: Svensk politik under 100 år</i>	Bertil L. Hanson	874
Liebman, Charles S., & Eliezer Don-Yehiya. <i>Civil Religion in Israel: Traditional Judaism and Political Culture in the Jewish State</i>	Itzhak Galnoor	875
Lijphart, Arend. <i>Democracies: Patterns of Majoritarian and Consensus Government in Twenty-One Countries</i>	David R. Herron	876
Milbrath, Lester W. <i>Environmentalists: Vanguard for a New Society</i>	David D. Dabelko	877
Moses, Joel C. <i>The Politics of Women and Work in the Soviet Union and the United States: Alternative Work Schedules and Sex Discrimination</i>	Jeanne-Marie Col	877
Mueller, Hans-Eberhard. <i>Bureaucracy, Education, and Monopoly: Civil Service Reforms in Prussia and England</i>		
Vann, James Allen. <i>The Making of a State: Württemberg, 1593-1793</i>	M. W. Jackson	878

Munck, Ronaldo. <i>Politics and Dependency in the Third World: The Case of Latin America</i>	Elisha Greifer	879
Nelson, Daniel N., ed. <i>Communism and the Politics of Inequalities</i>	Bohdan Harasymiw	880
Nelson, Sarah. <i>Ulster's Uncertain Defenders: Protestant Political, Paramilitary, and Community Groups and the Northern Ireland Conflict</i>	John Kurt Jacobsen	881
Ogene, F. Chidozie. <i>Interest Groups and the Shaping of Foreign Policy: Four Case Studies of United States African Policy</i>	Richard Dale	882
Organski, A. F. K. et al. <i>Births, Deaths, and Taxes: The Demographic and Political Transitions</i>	Mark Lichbach	883
Scharf, C. Bradley. <i>Politics and Change in East Germany: An Evaluation of Socialist Democracy</i>	Thomas A. Baylis	884
Sigler, Jay A. <i>Minority Rights: A Comparative Analysis</i>	Daniel C. Kramer	885
Stohl, Michael, & George A. Lopez, eds. <i>The State as Terrorist: The Dynamics of Governmental Violence and Repression</i>	William L. Waugh, Jr.	886
Volkan, Vamik D., & Norman Itzkowitz. <i>The Immortal Atatürk: A Psychobiography</i>	James Chowning Davies	887
Zimmermann, Ekkart. <i>Political Violence, Crises, and Revolutions: Theories and Research</i>	Wayne Gabardi	888

International Relations

Banks, Michael, ed. <i>Conflict in World Society: A New Perspective on International Relations</i>	John M. Rothgeb, Jr.	889
Becker, William H., & Samuel F. Wells, eds. <i>Economics and World Power: An Assessment of American Diplomacy since 1789</i>	David A. Deese	890
Beres, Louis René. <i>Reason and Realpolitik: U.S. Foreign Policy and World Order</i>	Arthur Stein	891
Choucrist, Nazli, ed. <i>Multidisciplinary Perspectives on Population and Conflict</i>	Demetrios G. Papademetriou	892
Cowhey, Peter. <i>The Problems of Plenty: Energy Policy and International Politics</i>	Ernest J. Wilson III	892
Dore, Isaak I. <i>International Law and the Superpowers: Normative Order in a Divided World</i>	Christopher C. Joyner	893
Frei, Daniel. <i>Assumptions and Perceptions in Disarmament</i>	Patrick Callahan	894
Goren, Roberta; ed. by Jillian Becker. <i>The Soviet Union and Terrorism</i>	Stephen Sloan	895
Harding, Harry, ed. <i>China's Foreign Relations in the 1980s</i>		
Kim, Samuel S., ed. <i>China and the World: Chinese Foreign Policy in the Post-Mao Era</i>	Robert S. Ross	896
Jervis, Robert. <i>The Illogic of American Nuclear Strategy</i>	Philip W. Dyer	897
Lincoln, Jennie K., & Elizabeth G. Ferris, eds. <i>The Dynamics of Latin American Foreign Policies: Challenges for the 1980s</i>	Steve C. Ropp	898
Malone, Peter. <i>The British Nuclear Deterrent</i>	Jeffrey Boutwell	899
McCaughy, Robert A. <i>International Studies and Academic Enterprises: A Chapter in the Enclosure of American Learning</i>	Manfred Grote	900
McGowan, Pat & Charles W. Kegley, Jr., ed. <i>Foreign Policy and the Modern World-System</i>	Ralph G. Carter	901
Munoz, Heraldo & Joseph Tulchin, eds. <i>Latin American Nations in World Politics</i>	David Pion-Berlin	902
Pillar, Paul R. <i>Negotiating Peace: War Termination as a Bargaining Process</i>	Martin C. McGuire	903

Putnam, Robert D.; & Nicholas Bayne. <i>Hanging Together: The Seven-Power Summits</i>	Werner J. Feld	903
Rice, Condoleezza. <i>The Soviet Union and the Czechoslovak Army, 1948-1983: Uncertain Allegiance</i>	Dale R. Herspring	904
Suter, Keith. <i>An International Law of Guerilla Warfare</i>	Richard B. Finnegan	905
Ulam, Adam B. <i>Dangerous Relations: The Soviet Union in World Politics, 1970-1982</i>	James P. Nichol	906

Normative Theory

Benton, Ted. <i>The Rise and Fall of Structural Marxism: Althusser and His Influence</i>	James C. Foster	907
Dietze, Gottfried. <i>Liberalism Proper and Proper Liberalism</i>	Robert B. Thigpen	908
Dunn, John. <i>The Politics of Socialism: An Essay in Political Theory</i>	Gregory Claeys	909
Gillespie, Michael Allen. <i>Hegel, Heidegger, and the Ground of History</i>	Donald J. Maletz	910
Gray, John. <i>Hayek on Liberty</i>	Leonard Williams	911
Hyfler, Robert. <i>Prophets of the Left: American Socialist Thought in the Twentieth Century</i>	Sanford N. Greenberg	911
Khadduri, Majid. <i>The Islamic Conception of Justice</i>	Glenn E. Perry	912
Matthews, Richard K. <i>The Radical Politics of Thomas Jefferson: A Revisionist View</i>	Darryl Baskin	913
Pitkin, Hanna Fenichel. <i>Fortune Is a Woman: Gender and Politics in the Thought of Niccolo Machiavelli</i>	Sonia Kruks	914
Tarcov, Nathan. <i>Locke's Education for Liberty</i>	Sonia Kruks	914
Thompson, W. D. J. Cargill. <i>The Political Thought of Martin Luther</i>	Peter Augustine Lawler	916
Wildavsky, Aaron. <i>The Nursing Father: Moses as a Political Leader</i>	John A. Rohr	916
Williams, Howard. <i>Kant's Political Philosophy</i>	Steven M. DeLue	917

Empirical Theory and Methodology

Maxwell, Nicholas. <i>From Knowledge to Wisdom: A Revolution in the Aims and Methods of Science</i>	Mark Oromaner	918
Noelle-Neumann, Elisabeth. <i>The Spiral of Silence: Public Opinion—Our Social Skin</i>	James W. Hottois	919
Ward, James F. <i>Language, Form, and Inquiry: Arthur F. Bentley's Philosophy of Social Science</i>	Kenneth R. Hoover	920

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Marxism and Secular Faith

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It has been argued by Mancur Olson and others that Karl Marx's theory of revolution is logically defective in that from its premises one cannot draw Marx's conclusion that workers will unite to revolt against capitalism. Workers who might wish for large social changes are confronted with a collective action problem that Marx fails to appreciate—so runs the criticism. The critics are assuming that Marx is reasoning from a Hobbesian premise to the effect that insofar as they are rational, individuals act always to fulfill narrowly self-interested goals. This article denies the assumption. In particular it is urged that to make sense of Marx's optimistic hopes about the likely outcome of successful majoritarian working-class revolution, one must attribute to him a secular faith that most people are disposed to play fair with others. This disposition is relatively weak and only sporadically effective in determining behavior, but in the right revolutionary circumstances, Marx hopes, it might play a considerably greater role in this respect. (A circumstance on which Marx places great weight is material abundance.) Being optimistic about the future, Marx cannot be as cynical about human motivation in the present as commentators often take him to be.

In the middle of the seventeenth century an astute political scientist wrote, "Of all Voluntary Acts, the Object is to every man his own Good" (Hobbes, 1968, p. 209). If we treat this statement not as a tautology but as a broad empirical claim, we may render it so: each person always acts with the aim of satisfying narrowly self-interested goals. One might quibble about how to interpret "narrowly." How narrow is narrow? In the tradition of political analysis that was inaugurated or at least given a hefty push forward by Hobbes, acting from narrowly self-interested motives can include acting to benefit friends and family members whose interests have become incorporated into the self-interest of the agent. The crucial denial is that people ever willingly act to sacrifice "their own" interests (including the interests of those near and dear to them) for the sake of mere strangers or for the sake of a mere perception of moral duty. Call this claim the "assumption of egoism." Obviously for this assumption to do useful explanatory work, it need not be strictly true, just close to true. If self-sacrificing conduct motivated by altruism or devotion to morality sometimes occurs, but too infrequently and on too small a scale to alter political outcomes significantly, the simplifying assumption of egoism will suffice for political theory. The assumption of egoism figures in a

venerable research program in political science that is very much alive and kicking.

As one of the great innovators and eccentrics in social theory, Karl Marx is hard to pigeonhole in terms of the category just described. Critics have seen in Marx both a penchant for generalizing social science invoking the assumption of egoism and an anticipation of hermeneutic and critical theory methods geared toward the normative interpretation of culture (e.g., Habermas, 1971, pp. 25-63; 1973, p. 238; and Gouldner, 1980). In this article I consider an interpretation and criticism of Marx that place him squarely in the Hobbesian camp. To defend Marx from the criticism, I argue, one must deny that Marx accepts the assumption of egoism (but this denial does not call in question Marx's commitment to generalizing social science).

Marx's theory of social change postulates that people are sometimes motivated to action by class interest, and that some of these actions crucially influence historical development. In the words of the *Communist Manifesto*, "The history of all hitherto existing society is the history of class struggles" (Marx & Engels, 1976b, p. 482). The upshot of Marxian social theory is that certain nasty features of capitalism render life unpleasant for capitalist workers. Workers can improve their group situation through collective action; in fact, as a group they would be better off if capitalism were abolished. Hence, the theory concludes, once workers comprehend their situation they will be disposed to undertake collective action to ameliorate their lot under capitalism and to do away with capitalism altogether when prospects are good that revolutionary action will succeed.

The premises of Marx's argument are open to

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challenge at many points. In 1965 Mancur Olson objected to its logic (Olson, 1965/1971; see also Tullock, 1971, and especially Buchanan, 1982). On Marx's own premises, according to Olson, he ought to have concluded that although it is in the interest of the workers as a group to revolt against capitalism, such action is not in the individual interest of any worker, so that workers if rational will not revolt. In this article I rehearse the Olson criticism and show how Marx might have defended himself against it. For this project I accept two ground rules: 1) any view imputed to Marx must square reasonably well with his texts, and 2) to count as a *defense* of Marx, an interpretation of his thought must not only evade the criticism under discussion but also propound on his behalf a view that is at least *prima facie* plausible and worthy of further examination in the light of present social science knowledge.

The Problem

Consider a good that is *collective* with respect to some group in the sense that if any person in the group consumes some of the good, it is not feasible to exclude any other group member from consuming some of it. Assume that the members of a group are *rational self-interested* in the sense that each member of the group acts efficiently to maximize his own expected benefit. Consider a group of N persons for whom a particular good that might be produced is collective. A *feasible supplier* of this good for N , let us say, is any subgroup for whom the gains from supplying the good exceed the total cost of supplying it. If the smallest feasible supplier in N has so many members that any member's contribution toward provision of the good is imperceptible to other members and will not affect any other individual's decision whether or not to contribute, the group N is in Olson's terminology "large." No individual of a "large" group has a rational incentive to contribute toward provision of the collective benefit, according to familiar dominance reasoning.¹

The application of this simple apparatus to Marx's theory is straightforward. According to Olson's Marx, socialist revolution—along with lesser benefits from smaller-scale class struggle—is a collective good for the class of workers,

but the workers constitute a "large" group, so none has a rational incentive to make a contribution toward the collective cause.

To put the point in another terminology, as suggested by Russell Hardin (1982, pp. 25-32), Olson is saying that the logic of proletarian revolution mirrors the logic of a many-person single-play Prisoner's Dilemma (PD). Even though all would benefit from the hoped-for revolutionary action, none has good reason to initiate it, so the revolution will not occur unless revolutionaries behave irrationally. The notoriously observable absence of revolution in the industrially advanced countries, where Marx predicted revolution would occur, appears to be only one among many examples of the suboptimal voluntary provision of collective goods that neoclassic economic theory leads one to expect as a matter of course.

First Solution: IIPD

In single-play PD, cooperation is not rational. But in indefinitely iterated prisoner's dilemma (IIPD), cooperation may be rational.² In this section I explore, and finally reject, the suggestion that the logic of many-person revolutionary class struggle should be modelled as the logic of many-person IIPD.

Iterating a PD opens up possibilities of rational cooperation, because each player must consider the possibility that what he does now may affect the behavior of others on future plays (see Luce & Raiffa, 1957, pp. 97-102). In an iterated PD, dominance reasoning does not support the strategy of "always defect." If a PD is to be iterated an unknown number of times, there can be coordination equilibria—strategy pairs such that no player would benefit from a change of strategy by any one player—provided certain conditions are met, chief among them 1) that each player's strategy dictates a punishing response to defections by others, and 2) that each player holds the probability of further repetitions of the game to be sufficiently high and the number of probable repetitions to be sufficiently large so that the long-run prospect of sustaining cooperation promises greater gain than the short-run temptation of defection.

Differences in the incentives facing agents in many-person as opposed to two-person iterated PD suggest that even if cooperation does occur in some many-person versions, it is likely to limp along at suboptimal levels. Roughly, the dif-

¹Notice that in the extreme case where a collective benefit will not be obtained unless all members of a group contribute toward its provision, the incentive to take a free ride on the contributions of the others disappears, regardless of the size of the group. For a thorough explication of Olson (1965/1971), see Hardin (1982, chaps. 2-3).

²For analysis of PD repeated a fixed number of times, where the number of iterations is common knowledge to all participants, see Schick (1984, pp. 68-76).

ference is that in two-person IIPD, punishing a non-cooperator directly benefits the punisher, whereas in many-person IIPD, cooperators cannot punish the defection of free-riders in subsequent play without harming themselves by denying the benefits of cooperation to all. Nonetheless IIPD analysis might well be the appropriate explanation of instances of ordinary or trade-union class struggle, brought about by labor-management conflicts of interest that are expected to recur into the indefinite future. "Ordinary" class struggle contrasts with "extraordinary" class struggle, the latter being marked by the expectation that a fundamental change in property relations throughout society is possible in the very short run and by a greatly increased willingness on all sides to break the customary rules of the ordinary class struggle game.³ Even if ordinary class struggle should prove to be explicable on the model of self-interested rational behavior in many-person IIPD situations, extraordinary class struggle is Marx's abiding analytical concern and the linchpin of his doctrine.

An extraordinary class struggle is unlikely to form a pattern that is amenable to IIPD analysis. IIPD-style reasoning seems inapplicable to the moment of revolution, when the game being played is unique and nonrepeatable and known to be such by the players. To be sure, one can often analyze a single outbreak of revolt into a connected series of events each of which can be viewed as a single play of a game. But there are disanalogies between this scenario and IIPD. Strategic interactions that can be represented as IIPD often owe their character to the presence of stable institutions that guarantee that the game is likely to be repeated over and over, but such institutions may disappear or become problematic in the chaos of approaching revolution.

Mulling over the dynamics of street protests that occurred in St. Petersburg in February, 1917, Trotsky observed, "A revolutionary uprising that spreads over a number of days can develop victoriously only in case it ascends step by step, and scores one success after another. A pause in its growth is dangerous, a prolonged marking of time, fatal" (Trotsky, 1964, p. 117). Suppose Trotsky's conjecture holds. The revolutionary process then decomposes into a series of games each of which (after the first) occurs only if a sufficient level of cooperation has been attained in the preceding game. It is not known in advance

how many games must be played to determine the success or failure of the revolutionary outbreak, but in the nature of the case the sum of payoffs for the complete series is pretty much fixed independently of how many games intervene between the present one and the final decisive encounter (what matters is, for example, not how many street demonstrations occur but whether or not they topple the regime). I take it to be plausible also to suppose that when the decisive game occurs, in which successful cooperation would ensure a successful revolution, the decisiveness of the encounter is common knowledge to all or at least presumed with high probability. But this last point destroys the IIPD-like appearance of the series.⁴ If rational self-interested cooperation through the series is explicable, this can only be because the structure of payoffs in the last decisive game is *not* PD, rather assurance or some other matrix that renders cooperation individually rational. If this final encounter is PD, in anticipation of this sad ending would-be revolutionaries if rationally prudent will decline to join at the outset. Either way, IIPD analysis turns out to be not pertinent.

To put the same point another way, any positive joint payoff for mutual cooperation, compared to mutual defection, in the early encounters of a revolutionary crisis will just consist in an enhanced likelihood that the movement will not fizzle out before a decisive engagement in which winning the battle would mean winning the civil war. But if the assumption at the outset must be that in the end everything hinges on a single contest, then the dynamic of mobilizing revolutionary forces must be quite unlike the dynamics of building trade union support, where there can be joint positive payoff for cooperators at many points along a path that extends into the indefinite future.

Besides being essentially single-play, extraordinary class struggle differs from ordinary class struggle in the number of participants required to sustain it. Other things being equal, one would expect that the larger the number of players in a many-person IIPD, the smaller the prospect of cooperation, owing to rising costs of explicit or

³A taxonomy of varieties of extraordinary class struggle is beyond the scope of this essay. Extraordinary class struggle can be political (aimed at replacing the government or overthrowing the state) or social (aimed at wresting control of economic enterprises) or both.

⁴The example in the text concerns extraordinary class struggle directed toward a coup. Consider a revolution that follows the syndicalist pattern of a wave of enterprise occupations. Each occupation decomposes into a series of games, but if a single encounter is believed to be decisive, the point in the text holds here also. Or one could consider the players to be collectives of workers in a number of enterprises, and the question for each collective is whether to join in a wave of enterprise takeovers. This issue, if PD at all, is once again single-play PD.

implicit bargaining, bluff, and strategy. When Marx writes of class struggle as a constant daily fact of capitalist society, he is thinking of small-scale conflicts pitting a few workers against a single capitalist. Despite the common label, the class struggle that in Marx's theory is expected to precipitate the demise of capitalism is a very different phenomenon, involving at least transnational common struggle by the working class of several European nations for transcendent aims.⁵ The large number of participants involved in Marxian scenarios of revolution itself militates against the prospect that the possibility of cooperation among rational egoists in IIPD situations could explain how such a revolution came about, should one ever occur.

Marxist usage of the term "class struggle" tends to accord this honorific label to small-scale disputes involving small numbers of workers arrayed against their bosses, even if a successful outcome of the struggle for those few workers would be disadvantageous to other members of the working class. Generosity in classifying fractional conflict as class struggle usually proceeds either from the hope or the prediction that fractional conflict will set in motion a process leading to more conflict on an increasingly wide scale and culminating in class struggle more strictly construed as struggle by the entire class or in the interest of the entire class. But insofar as IIPD-style cooperation is assumed to be the driving force of class struggle, the expectation that parochial or balkanized disputes could merge smoothly into unified revolutionary struggle looks to be misplaced.⁶ The motivations that might ignite the one will not spread to the other. The two processes are apparently discontinuous.⁷ From the fact that workers cooperate with each other in small-scale and geographically concentrated conflicts in which defection from cooperation can readily be

detected and the threat by cooperators to cease cooperating if defectors do not pay their dues can be credible, a rationally self-interested person will not infer that it is reasonable to cooperate in large-scale and geographically dispersed interactions in which such detection is difficult and such threats are bluff.⁸

Second Solution: Selective Incentives

Perhaps the best way to escape a PD trap is never to be caught inside one. Commentators have found several sources of selective incentives in situations that would otherwise be PD. These are holes in the trap. A brief review of the prospects for explaining the emergence of class struggle by these means is in order.

One idea is that cooperators might credibly threaten selectively to punish non-cooperators, thus diminishing the attractiveness of the free ride strategy. In this connection Olson mentions the violence and coercion that appear to be endemic to labor dispute in American history (Olson, 1971, pp. 66-76).⁹ But a full explanation of how

⁵Closely related to IIPD possibilities is the possibility of interdependent strategy choice. A situation that otherwise meets the conditions of single-play PD will still fail to be PD unless players' strategy choices are independent in the sense that no player is able to adopt a strategy that makes his choice of play contingent on what others do on that very same play. This is a strong assumption and one may wonder whether it holds true or even approximately true in real-world conflict and cooperation settings. For example, suppose that after a snowstorm neighbors have preferences regarding the shoveling of snow from their sidewalks that form a PD configuration. The situation will not be PD if each neighbor can remove the snow from his own walk while simultaneously watching to see that every other neighbor is doing the same. Is the formation of class struggle the soluble snow-shoveling problem writ large? Obviously, the greater the number of shovelers and the more geographically dispersed they are, the more difficult it will be to make one's own choice of play on a particular move contingent on the choices of others. And equally obviously, extraordinary class struggle involves large numbers of geographically dispersed agents (although monitoring technology can to some degree reduce the importance of geographic dispersal). Pending detailed analysis of individual cases, it does not seem that the Olson criticism will fail to apply to Marx's analysis owing to absence of independent strategy choice.

⁶Barrington Moore sensibly observes that in a revolutionary setting, "There is liable to be a good deal of bullying by militants to force laggards into line and sustain the appearance of solidarity" (Moore, 1978, p. 321). Buchanan rather broadly claims that Marx does not countenance the use of intimidation to motivate proletarians to join in struggle (Buchanan, 1982, p. 93),

⁵As evidence of the immaturity of the French working class in 1848, Marx cites its failure to perceive the need for a European revolutionary war against England (Marx, 1978b, p. 56).

⁶For another argument supporting this claim, see Roemer (1979, pp. 763-767).

⁷Rosa Luxemburg aptly characterizes this discontinuity: "With the psychology of a trade unionist who will not stay off his work on May Day unless he is assured in advance of a definite amount of support in the event of his being victimised, neither revolution nor mass strike can be made. But in the storm of the revolutionary period even the proletarian is transformed from a provident *pater familias* demanding support, into a revolutionary romanticist for whom even the highest good, life itself, to say nothing of material well-being, possesses but little in comparison with the ideals of the struggle" (cited in Holmstrom, 1983, p. 315).

cooperation might emerge and become stable is unlikely to be forthcoming from this quarter. For so far we have not been informed how cooperation rises to the point at which there is a substantial body of cooperators who find themselves irked at the non-cooperating behavior of a few. Even if we assume an initial state of cooperation, with just a few defectors, the sanctioning of these defectors will be costly, and the gains to be had from sanctioning will be shared among all cooperators regardless of who joined the sanctioning posse. The collective action problem replicates itself at the level of organizing the imposition of sanctions.

Another idea is that initiators of cooperation will anticipate the creation of an ongoing organization that will channel a stream of profits to the original founders. Union organizers will become trade union officials; today's revolutionaries will be tomorrow's commissars. Individuals with entrepreneurial talent alert people to opportunities for lucrative collective action, are paid by those people to organize cooperation, and perhaps attract initial followers by promising them shares of the expected payments (Kavka, 1982, p. 457).

Whatever its merits, this entrepreneurial account of the origins of class struggle appears to be inconsistent with Marx's expectation that class struggle in capitalist society will promote an egalitarian society free of hierarchy—as the *Communist Manifesto* puts it, “an association, in which the free development of each is the condition of the free development of all” (Marx & Engels, 1976b, p. 506).¹⁰ A more ground-level difficulty is that the account suggests motivations for organizers, but supplies no reason whatsoever for members of the target population to pay these organizers, join their incipient organizations, and initiate cooperation under their direction (Laver, 1980, pp. 204-208). Organizers and entrepreneurs of class struggle may promise selective benefits to potential supporters, but on the assumption that all parties to these discussions are individually self-interested, why should the masses believe that the fledgling trade unionists or revolutionaries out

of power will keep their promises once installed in power? Bygones are bygones for a self-interested utility-maximizer.

A third selective incentive that might supply rational motivation for self-interested members of “large” groups to contribute toward provision of collective benefits is negative contribution costs. If the act of contributing is valued for its inherent pleasures, apart from further consequences, these pleasures might offset the gains from free-riding sufficiently to motivate contribution (Hirschman, 1982, p. 86). One cannot enjoy solidarity and sibling camaraderie by sitting on the side lines but only by joining in the fray. Trivially, the solidarity of participants is available only to participants, but even vicarious solidarity would tend to be inhibited by the awareness that one is shirking participation. However, doubts intrude. One may wonder whether in many cases these participation benefits correctly fit into the category of strictly self-interested satisfactions. Why does banter with one's mates on a protest parade seem more zestful than banter with the same mates in a pool hall? If the answer is that one especially values contributing to a good cause in concert with one's friends, then the desire that is being satisfied is mixed in character, partly moralized rather than fully self-interested. Moreover, if participation benefits only flow from collective action that has reached a certain threshold size, the PD problem recurs for individual contributions to the initial stages of cooperation below the threshold level (Buchanan, 1982, p. 95). Another limitation to the participation benefits explanation of collective action is that participation benefits often are ephemeral, but collective action ordinarily must be sustained and steady in order to be successful. Reiterated, the initially fascinating union meeting becomes a crushing bore.¹¹ If the collective goal can be reached in one exciting leap, all may be well, but if many leaps are required the pace slows to a dull trudge, and free-rider conduct beckons.

The fading-joy limitation is particularly pertinent to what I have called “ordinary” class struggle, while the threshold difficulty applies with special force to extraordinary or revolutionary class struggle.

Regarding all selective-incentive explanations of how collective action arises, one issue is whether the stipulated incentives really are sufficiently strong and prevalent to motivate contribu-

But Marx would surely accept that a proletarian movement democratically organized may rightfully use informal analogues of conscription for military defense. I agree with Buchanan, however, that a movement that is sufficiently well organized to enjoy access to means of coercion has already solved its collective action problem, so coercion cannot explain how collective action begins.

¹⁰The inconsistency arises, of course, only if entrepreneurially minded revolutionaries are correct in their belief that they can parlay leadership in the struggle into power and privilege in its aftermath.

¹¹In a June 6, 1983 *Los Angeles Times* interview, long-time radical activist Dorothy Healey offers a bemused perspective on the joys of participation: “I hate meetings . . . My life has been absolutely filled with meetings. That's what the Left is . . . one big meeting” (Horowitz, 1983, p. 7).

tions in the case at hand. Another issue for us is whether Marx's texts suggest that he himself thought that selective incentives explain the emergence of class struggle. Without going into this latter issue I will just mention that the answer seems to me to be that he did not.

Marx on Motives

Participants in class struggle as Marx conceives it are motivated by a desire to promote their common class interest. This entails that the participant is willing, at least to some extent, to sacrifice his personal interest and even the interest of his fractional subgroup within the working class to the general interest of the international working class. Marx is optimistic that this wide cooperation will be forthcoming. In an interview with a journalist regarding the alleged conspiracies of the International Workingmen's Association, he comments, "Formerly, when a strike took place in one country it was defeated by the importation of workers from another. The International has nearly stopped all that. It receives information of the intended strike, it spreads that information among its members, who at once see that for them the seat of the struggle must be forbidden ground" (Marx, 1974, p. 395). The compulsion in Marx's "must" arises from a sentiment of caring for fellow members of one's class, not a perception of where one's individual self-interest lies. In a loftier tone, Marx observes, "It is one of the great purposes of the Association to make the workmen of different countries not only *feel* but *act* as brethren and comrades in the army of emancipation" (1974, p. 86). The members of this army strive to defeat the opposing forces of capital, but their aim is emancipation not for themselves alone but for humanity. To be motivated by class interest in the relevant sense is *not* to be disposed to promote the welfare of one's own class at the expense of the welfare of all other classes. Class interest is not supposed to induce proletarian fighters to wrest an advantage for their class at the expense of the desperately poor, chronically unemployed underclass, for instance. This is so despite Marx's judgment that the lumpenproletariat is more likely to furnish mercenaries of the established order than allies of the revolutionary cause. In short, the "interest" component of class interest becomes thoroughly attenuated under inspection. Fully class-conscious workers aim to promote the interests of the international working class because they subscribe to a theory (namely Marx's) according to which promoting working-class interests is the best available strategy for improving the future for humanity generally.

Marx's accounts of struggles in which the participants lack full class consciousness also refer to

motives beyond egoism. In a well-known passage of *The Eighteenth Brumaire* (1979), Marx makes an invidious contrast between the French working class, capable of bold political action to defend its interest, and the French peasantry, incapable of such initiatives, at least in the historical period under review (1848-1851):

The small peasants form a vast mass, the members of which live in similar conditions, but without entering into manifold relations with one another. Their mode of production isolates them from one another, instead of bringing them into mutual intercourse. The isolation is increased by France's bad means of communication and by the poverty of the peasants. Their field of production, the small holding, admits of no division of labour in its cultivation, no application of science, and, therefore, no multiplicity of development, no diversity of talents, no wealth of social relationships (Marx, 1979, p. 187).

Marx expects the reader to infer that, in contrast with the peasant's situation, large-scale industry does admit of extensive division of labor, the application of science, and so a wealth of social relationships among the industrial workers. Some have seen in this passage a glimmering of the idea that an anomic pattern of social relationships lacking repeated encounters among potential cooperators will tend to prevent the emergence of cooperation in PD situations (Hardin, 1982, p. 184). But the vision of working class politics that Marx contrasts to his sketch of peasant existence is too epic, too grand to be reducible to this sensible, mundane thought.

While mordantly cynical in some respects, Marx's description of clashes and battles in February-June 1848 is entirely uncynical in its presentation of the bold and self-sacrificing motivations of revolutionaries past and present. "But unheroic as bourgeois society is, yet it had need of heroism, of sacrifices, of terror, of civil war and of national battles to bring it into being" (Marx, 1979, p. 104). Marx observes that after the February revolution, "the Paris proletariat . . . revelled in the vision of the wide prospects that had opened before it" (p. 109). The dazzling view here alluded to is not merely one of more efficient means ready to hand for satisfying proletarians' narrow self-interests, but rather a perhaps utopian vision of a better world for all. This vision motivates collective action, even hopeless action, in the story Marx tells. Praising the heroism of the Paris proletariat in the street fighting of the June days, Marx comments, "At least it succumbs with the honors of the great, world-historic struggle" (p. 111). Generalizing about proletarian revolutions in the modern era, Marx observes that they "recoil ever and anon from the indefinite prodigi-

ousness of their own aims, until the situation has been created which makes all turning back impossible" (p. 107). This does not mean: a situation is reached in which every revolutionary calculates that his expected personal payoff from the strategy of retreating or deserting the cause is less than the expected personal payoff from the strategy of going forward. Rather the idea is that a point is reached at which turning back would renege on a commitment to one's most ideal self-image, to be realized in the attainment of the most prodigious aims by heroic means. This is Marx the German Romantic, not the sober Victorian political economist.

Let's return to the peasant side of the worker/peasant comparison Marx is making. Notice that what Marx is specifically attempting to explain is not how workers are able to cooperate in collective action for common goals while peasants fail to act collectively. Rather he is trying to explain why the peasants' main political intervention was to vote for authoritarian rule in December, 1848, while workers had managed to mount sporadic active resistance to constituted authority between February and June. The act of casting a ballot for Louis Bonaparte may express the class viewpoint of the peasantry but is not—for all its lack of grandeur—in the individual self-interest of any given peasant (even on the assumption that Bonaparte's election would be in the collective interest of the peasantry). The problem Marx is trying to solve is simply not the problem Olson accuses Marx of failing to solve.

The same conclusion is suggested by Marx's emphatic statement that voting for Bonaparte was an intellectual error on the part of the peasants. The vote cast expressed their viewpoint, not their rational interests. As Marx puts it: "The Bonaparte dynasty represents not the revolutionary, but the conservative peasant . . . not the enlightenment, but the superstition of the peasant; not his judgment, but his prejudice . . ." (p. 188). The problem is not that the peasant's rational calculation of his individual self-interest inhibits him from acting with others to secure a collectively optimal outcome. The problem in Marx's jaundiced view is that the "rural idiocy" of peasant life precludes his taking an objective and enlightened view of his class interests. Whatever disposition to sacrifice self for class that peasants possess will come to naught as long as ignorance and other intellectual vices vitiate their perception. When Marx characterizes the French peasantry as unable to enforce its class interests, he is not denying the capacity of the peasants to initiate a jacquerie. He is denying that anything will come of such actions. His reason is that the peasantry lacks "manifold relations" and a "wealth of social relationships." I suggest putting a cognitive

gloss on these phrases. The modern world is complex, but the benighted peasant does not encounter its complexity, whereas the industrial worker, whose social experience is more various, is more prone to adopt sophisticated social theories reflecting this modern complexity. So at any rate Marx hopes.

Human Nature: Marx's View

If Marx does not hold that narrow self-interest supplies good reason for individual participation in class struggle, on what motivational assumptions does he then rely? If class struggle is the motor of history, what propels the motor?

The clue I propose to follow is to ask what view of human nature must be assumed in order to make sense of the millenarian aspect of Marx's thought—his assumption that whereas "(i)n all revolutions up to now the mode of activity always remained unscathed and it was only a question of a different distribution of this activity . . . the communist revolution is directed against the preceding *mode* of activity, does away with *labour*, and abolishes the rule of all classes with the classes themselves . . ." (Marx & Engels, 1976a, p. 52). For on the assumption that humans under capitalism are rational egoist utility-maximizers, Marx's optimistic expectation is utterly at odds with his own theory. The Marxian expectation I am querying is not the prediction that the proletariat will wage a successful revolution against capitalism; rather it is the prediction that postcapitalist society will be classless, egalitarian, tolerably free of injustice in basic social institutions. Society abhors a vacuum. If a ruling class is suddenly displaced from power, a remarkable opportunity presents itself for rationally self-interested agents to wrest power for themselves, to consolidate their advantage, and to extract profit and privilege for themselves from their positions of power. Only if *per impossible* each individual's power precisely and continuously balances the power of every other individual will rational egoists caught in the turmoil of revolution fail to move to an equilibrium recognizable as the formation of a new class society.

Any interpretation must reckon with Marx's failure to address this issue directly.¹² Occasion-

¹²A sketchy but pertinent comment appears in *The German Ideology*: "Both for the production on a mass scale of this communist consciousness, and for the success of the cause itself, the alteration of men on a mass scale is necessary, an alteration which can only take place in a practical movement, in a *revolution*; the revolution is necessary, therefore, not only because the *ruling class* cannot be overthrown in any other way, but also because the class *overthrowing it* can only in a

ally Marx seems to hint that the distinguishing factor in the approaching working-class revolution is that for the first time it will not be a segment of society that takes power, but a homogeneous class comprising virtually all members of society. According to Marx a capitalist society ripe for revolution has a simplified class structure consisting of a tiny minority of capitalists and their allies surrounded by an undifferentiated mass of unskilled workers. After the revolution, in this scenario, society takes control of society. Society will have no stake in securing vested interests against itself. In the words of the *Communist Manifesto*, "All the preceding classes that got the upper hand, sought to fortify their already acquired status," but the proletarians "have nothing of their own to secure and to fortify" (Marx & Engels, 1976b, p. 495; see also p. 485). Despite appearances, this passage does not engage the problem just posed. Marx's claim rests on an extreme version of his class-polarization thesis, which has proven to be false, but even assuming its correctness for the sake of argument does not help with the difficulty. Assume a huge homogeneous proletariat united by the need to cohere in struggle to oust the capitalists: once this struggle is successful one should expect to see this proletariat rapidly become heterogeneous as individuals scramble to do as well for themselves as they can by fishing in the troubled waters. Having nothing of his own, a rational egoist will have all the greater motivation to grab something that will become his own to secure and to fortify.

I submit that the problem of rendering intelligible Marx's reasons for optimism about the future of society will prove intractable unless we are willing to jettison the assumption that Marx is assuming that individuals are always motivated solely by narrow self-interest. The explanation of why Marx fails to elucidate the connection between his cynicism about human nature and his utopianism about the future of society is simply that Marx does not embrace cynicism, so there is no connection to be elaborated.

revolution succeed in ridding itself of all the muck of ages and become fitted to found society anew" (Marx & Engels, 1976a, p. 53). Marx's idea here is that participation in revolution is ennobling. This is plausible, but so is Brecht's frequent theme that participation in revolutionary activity hardens and coarsens. Even supposing that Marx rather than Brecht is correct on this point, there will be a wide range of individual degrees of involvement in revolutionary activity, and in any reasonable scenario a majority of the population will not be actively involved to a significant extent: hence they will not fully undergo the revolutionary character transformation that Marx says is crucial.

An alert reader might well object that Marx believes that human nature changes and develops throughout history, so it is not inconsistent for Marx to suppose the individuals formed in capitalist society will be rational egoists while individuals formed in postcapitalist society will have quite other motivations. Indeed it is the process of class struggle itself culminating in revolution that transforms individuals or enables them to transform themselves from selfish individuals into socially minded citizens.

It is certainly true that Marx holds that human nature changes in important respects throughout history. The hunger of a savage differs from the hunger of a civilized gourmet (Marx, 1970, p. 147). *The German Ideology* states emphatically if somewhat vaguely, "The difference between the individual as a person and whatever is extraneous to him is not a conceptual difference but a historical fact" (Marx & Engels, 1976a, p. 81). But from the fact that Marx believes that human nature is changeable in some respects it does not follow that it would be inconsistent for Marx to claim that in some respects human nature is unchanging (Cohen, 1978, p. 151, makes this point).

The claim that Marx believes that human beings are nasty (egoistic) in class society and become nice (altruistic) in the transition to classless society, I will call the "standard view." I have urged that if the standard view is correct, then Marx is after all vulnerable to the Olson criticism, for he provides no coherent account of how purely self-interested persons can unite to overthrow capitalism, much less inaugurate communism. Nor does Marx offer even a rudimentary beginning of an account of how, on the assumption that capitalism is eliminated and communist institutions are installed, these institutions will work to alter human nature away from egoism and toward communalism. To me these considerations suggest that we should reject the standard view if we can elaborate an acceptable alternative, and the alternative I propose is that Marx believes that a concern for others, a moral concern, is a stable component of human motivation throughout history, but one that is all but submerged by adverse circumstances that persist invariant up to the demise of capitalism.

Before adumbrating this "moralized" view of Marx's beliefs about human nature, a preliminary and apparently decisive difficulty must be faced. My view attracts the obvious objection that Marx frequently expresses a tough-minded cynicism about human nature and shows great ingenuity in ferreting out possible selfish motives for what superficially seems to be philanthropic and idealistic action. Any interpretation such as mine that denies that Marx loudly laments the universal bourgeois egoism that, capitalist institutions

engender thereby refutes itself—so goes the objection.

To illustrate how this objection can be defused, I will examine a typically cynical comment by Marx and Engels that happens to be cited by Olson in support of the standard view, of which he is himself an adherent. This famous passage from the *Communist Manifesto* runs as follows: "The bourgeoisie has left remaining no other nexus between man and man than naked self-interest, than callous 'cash-payment.' It has drowned the most heavenly ecstasies of religious fervor, of chivalrous enthusiasm, of philistine sentimentalism, in the icy water of egotistical calculation" (cited in Olson, 1971, p. 104). To read this passage correctly one must distinguish between 1) an ideology that describes and rationalizes ordinary or normal social behavior, 2) people's actual ordinary social behavior, and 3) the full range of people's desires only some of which are expressed in ordinary social behavior. The passage quoted just above deals with 1) and 2), not 3). Marx and Engels are saying that capitalist ideology predicts and prescribes narrowly self-centered behavior, whereas feudal ideology prescribed and predicted unselfish behavior in conformity with the norms of religion, chivalry, and so on. Actual behavior under both feudalism and capitalism was for the most part narrowly self-centered. To this extent capitalist ideology is more accurate and less hypocritical than its feudal counterpart. But to say this is not to pronounce at all on the topic of 3). On this last point the passage quoted is strictly noncommittal. In fact it is fully compatible with the further assertion that most people's strongest preference is to cooperate with others on fair terms, but people inchoately recognize that the going terms of actual social cooperation are fixed beyond their control, so the preference they mainly act upon is to do as well for themselves as they can rather than play the role of sucker in a dog-eat-dog world. This indeed is the gist of the position Marx holds on my interpretation. I don't say the quoted passage requires this further gloss, only that the two are fully consistent. Pointing to Marx's cynical quips does not begin to settle the interpretive issue.

The thought I am relying on here is straightforward: people's actions are determined by their desires in conjunction with their beliefs, so even a very strong desire coupled with a belief that the desire is unrealizable may not show itself directly in behavior. Of course, if Marx or anybody else does wish to advance the hypothesis of a latent preference, he is obliged to provide empirical evidence, perhaps of an indirect sort, to the effect that the latent preference postulated is really present and not merely the social theorist's fantasy.

Marx's remarks on ideology do supply materials from which one could begin to construct such a case. People's proclivity to accept ideologically biased theories of fairness and, I would add, sometimes to act on their ideology even against their own perceived interests, is plausibly interpreted as reflecting a powerful psychological pressure to think that one is on the whole behaving fairly where this pressure is blocked by dim recognition that one's acceptance of genuinely fair shares would entail intolerable personal sacrifice. My object here is not to marshal evidence but to mark how one might do so on Marx's behalf and to note that Marx is properly sensitive to the hazards of putting forward unfalsifiable claims about human nature.

Setting the issue of evidence aside, I want to take as a clue to Marx's core beliefs about motivation his thought that an egalitarian society is possible only on the basis of the enormous wealth that is accumulated under capitalism. That increased wealth enhances prospects for egalitarianism is explicable only if one assumes that Marx ascribes to human nature a set of motives or dispositional traits roughly along the following lines:

- 1) Persons strongly desire a commodious existence.
- 2) Persons desire to cooperate with others on fair (equal) terms.

At least two phrases in these statements are subject to variation in meaning owing to the varying pressure of prevalent social ideology: "commodious existence" and "fair terms." But here I leave aside the complications that would have to be introduced into this account in order to incorporate Marx's ideas on ideology.

Under conditions of scarcity that according to Marx have characterized all of human history down to the mid-nineteenth century, cooperation on fair (equal) terms among all members of society would have been incompatible with the enjoyment of a commodious existence by any member of society. In this sense 1) and 2) conflict. People have tended to resolve this conflict in their impulses by restricting—or acquiescing in the restriction of—the scope of their desire to cooperate: the disposition to cooperate on terms perceived to be fair is normally effective only within groups, such as family, clan, caste, class, race, nationality, with which the individual identifies herself to a greater or lesser extent. Being gregarious, people for the most part do not behave as egoists. Being less than thoroughly altruistic, people for the most part confine their concern for others within the boundaries of salient group identity. Marx's particular

emphasis is on the salience of economic class identity. Summarizing, we have:

- 3) When cooperation with all on equal terms precludes commodious existence for any, people will tend to confine their willingness to cooperate to their group memberships—among which economic class looms large.

Attributing 1)-3) to Marx interprets him as holding that class oppression is necessary for social order throughout human history up to the point when capitalist economic abundance creates the possibility of a commodious existence for all. By "class oppression" I mean a division of the benefits of social cooperation among classes that is both unfair and coercively enforced. When there is not enough to go around, people will cluster in groups that struggle for a larger-than-average share. Class oppression is "necessary" for social order in the sense that social order is brought about by it and could not exist without it. To see that class oppression creates and sustains social order, it suffices to note that an egalitarian division of benefits in a nonaffluent society would be a standing temptation, greater than human nature could bear, to violent social conflict and civil war. Oppressive social order is of course unstable at times, but an egalitarian social order yielding a pinched and cramped standard of living for all is necessarily unstable, given human psychology as Marx perceives it. Provision of a commodious existence to some engenders loyalty to the providing regime, and a regime cannot survive unless it elicits such loyalty. But provision of a commodious existence to a privileged section of society is tantamount to class oppression.

G.A. Cohen has recently denied that class oppression is instrumentally necessary to social order throughout human history down to the time of capitalist abundance (Cohen, 1978, pp. 207-213). (He also denies that Marx's texts show whether or not Marx himself held this view.) In support of this denial, Cohen points out that there have been stable precapitalist oppressive societies in which a ruling elite extracts a surplus from a peasantry that is self-directing in its economic production and seems to receive no favors from the elite. It might be thought that in such circumstances the taxing or pillaging ruling elite does not, merely by its oppression, contribute anything at all to the maintenance of social order. Although plausible, such an inference would be mistaken. By filling the top position of privilege in a hierarchy, the ruling elite preempts the fierce conflict for that top slot that would prevail in its absence. As Marx famously puts it, the overcoming of alienation requires a high development of productive forces because, in the absence of abun-

dant wealth, equality of shares means only that "*want* is merely made general, and with want the struggle for necessities would begin again, and all the old filthy business would necessarily be restored" (Marx & Engels, 1976, p. 49).

Taken by themselves, 1)-3) do not say what will happen in the situation of economic abundance that in Marx's estimation is capitalism's signal achievement. In a capitalist society, the desire for a commodious existence and the desire to cooperate with all members of society on equal terms are, in principle, both satisfiable simultaneously. With a lessening of the conflict between 1) and 2), the tendency to parochial restriction of the willingness to cooperate begins to weaken.

- 4) When 1) and 2) are jointly satisfiable for all members of society, individuals tend to develop a desire to cooperate on an increasingly wide basis, with all others who are willing to reciprocate cooperation on the same equal terms, ultimately with humanity generally.

Of course this impulse to harmony is all but stifled: "The tradition of all the dead generations weighs like a nightmare upon the brain of the living" (Marx, 1979, p. 103). Marx singles out two complementary inhibiting factors.

- 5) Once having acquired power and privilege far above the average, individuals will not voluntarily relinquish the positions of advantage that confer these extraordinary benefits. (In short, power corrupts.)

According to Marx, besides the resistance of the privileged capitalist class, the other solid obstacle blocking the development of postcapitalist harmony is the passivity of the underprivileged class.

- 6) Low social status, lack of experience in getting one's way in the world, and denial of the means of commodious existence when these means are not objectively in scarce supply, all tend to inhibit people's willingness to enlarge the scope of the "others" whom they see as potential partners in reciprocal cooperation. Contrariwise, success in the pursuit of advantages by the disadvantaged will tend to break down this inhibition against reciprocal altruism. (In short, powerlessness corrupts.)

Poor conditions force people to struggle narrowly for their self-interest and, especially insofar as their struggles meet with frustration, persuade them that they are not just poor but poor people. In contrast, successful class struggle educates people toward desirable character traits.¹³ Or at least

¹³A recurrent theme of revolutionary analysts of Marxian persuasion is that the experience of small-scale

there is an other-things-being-equal tendency in this direction, which, as Marx acknowledges, many adverse factors can deflect.

I am conjecturing that Marx is committed to claims somewhat along the lines of 4) through 6). I have no direct textual evidence for this attribution. Moreover, 4)-6) as they stand are too baldly stated. Contrary to what 5) seems to assert, it is not true that no king ever voluntarily abdicates the throne. It is not part of my present purpose to introduce the exact hedges and qualifications that would be needed to render 4)-6) weak enough to be invulnerable to obvious historical counter-example yet strong enough to support Marx's theory of proletarian revolution. What I do want to claim is that assumptions close to 4)-6) must be in the back of Marx's mind when he asserts that all history is the history of class struggle, that proletarian class struggle will produce a desirable end to class struggle, that voluntary resignation of power by a capitalist ruling elite is not a possible alternative route to this desirable end of class struggle, and that the process of proletarian class struggle will not be inimical to the widespread development of the citizenly virtues that will be needed in the commonwealth that is to arise in the final aftermath of class struggle. I want to claim that if one is to interpret Marx as espousing or at least gesturing toward a coherent theory, suitably refined versions of 4) to 6) will be essential parts of it.

The general level of wealth in society obviously affects the prospects for successful class struggle.

and localized conflicts can increase the participants' willingness to join in the very different activities of extraordinary class struggle. For example, Rosa Luxemburg writes, "Political and economic strikes, mass strikes and partial strikes, demonstrative strikes and fighting strikes . . . all these run through one another, run side by side, cross one another, flow in and over one another" (Luxemburg, 1925, p. 37). This would make no sense if the idea were that the practise of conditional cooperation by rational egoists in IIPD situations will increase the willingness of any egoist to cooperate non-egoistically where IIPD conditions do not obtain. But it can make sense to suggest that witnessing altruistic or morally motivated behavior on a small scale can enlarge people's sense of possibilities, disabuse them of ultra-cynical beliefs about the motivations of others, and thus increase their disposition to take risky initial steps toward large-scale cooperation (the success of which can in turn stimulate local protest activity). Cooperation with neighbors can induce one to cooperate with strangers who are also cooperating with their neighbors, but only if the basis of neighborly cooperation is not I'll-scratch-your-back-if-you-scratch-mine, for this arrangement cannot be extrapolated to cooperation with strangers at a distance, not at any rate in the range of cases of concern to Marx.

Spartacus in ancient Rome did not have a chance, but the modern proletarian has a more realistic basis for hope. The greater the wealth at the disposal of the ruling class, the greater the incentive to make concessions to underclass demands, and such concessions inaugurate a cycle of increasing proletarian self-esteem, self-confidence, and widening cooperation.¹⁴ Wealth does not guarantee a successful end to this cycle, but wealth's absence guarantees its failure: "Right can never be higher than the economic structure of society and its cultural development conditioned thereby" (Marx, 1978a, p. 53).

The thumbnail psychology sketched so far does not specify the exact strength of the motives it postulates. In this vagueness the sketch is true to Marx's thought. To round out the sketch, some account of ordinary versus extraordinary motivation is wanted. According to Marx, a time of social crisis and revolutionary upheaval can elicit unstinting heroism from people who do not in ordinary times give any indication of this potential for self-sacrifice. What explains this?¹⁵ Marx believes:

"Against Marx's view, Lipset marshalls some evidence for the claim that the granting of substantial concessions including "full political and economic citizenship" to the working class dampens its radicalism (Lipset, 1982, p. 2).

¹⁵John Roemer has an explanation that is very different from the one I offer (Roemer, 1978, 1979). He denies—and I concur—that IIPD reasoning is a very likely candidate for explaining "convulsive action" such as mass strikes. His further description almost suggests that convulsive action is irrational; the crowd is seized with madness. "The participants act, in a sense, despite their better judgment. They do not calculate rationally whether or not to strike, walk out, or fight the police. People are pushed, they retreat, they seek individual solutions; they are pushed farther, conditions become intolerable, some incident occurs, and there is an eruption" (1979, p. 763). No doubt workers like other people sometimes behave irrationally, but explanations of collective action on this basis will never vindicate Marx, who predicts that workers will engage in convulsive class struggle *and* that they will have *good reason* for doing so. Rather differently, Roemer also suggests that collective action proceeds not from irrationality but from a switch from individual to collective rationality. He doesn't elaborate, but I think he means that workers identify with their own class and act for its good rather than their own. But I have pointed out that the group identification in class interest is, in Marx's account, progressively generalized and moralized. Fully class-conscious workers act for the good of humanity. A revolutionary time arises precisely when ordinary people begin thinking self-consciously about the role they might play in world history, like Paris Communards. Roemer (1978) has a further interesting suggestion. Insofar as they perceive themselves as "participants in a highly social production process who operate under

7) Extreme situations can elicit extremes of behavior; in particular, a revolutionary social crisis can elicit extremes of heroism (as well as of dastardly conduct).

In a time of crisis, the possible gains and losses from bold collective action or its absence are greater than in normal times. Under favorable crisis circumstances, the expected gain to the collective from one's individual participation, discounted by the probability that one's action will bring about the desired outcome, may be spectacular. Hence, whatever altruistic willingness one has to sacrifice oneself for the collective will be far more likely to express itself in action at such times. The cautiously prudent person who becomes a bold revolutionary need not have undergone any drastic character change. Rather what has changed is the ratio of collective benefits to individual costs consequent upon bold action. Similar considerations explain how a prudent, seemingly virtuous person may give vent to extreme vice in a time of turmoil. (See Barry, 1970, pp. 30-32.)

The truth of 1)-6) all at once would not support the conclusion that it is ever rational for any individual person to revolt against capitalism. For 1)-6) at most warrant inferences about what people will want, not about what they will do. Desiring to cooperate with others on a reciprocal basis, I may yet reasonably refuse to cooperate if I lack good reason to believe others will reciprocate. Many persons could be in this position: all of us want to cooperate and none of us believes a sufficient number of us want to cooperate. The intensity of this problem increases as the number required for cooperation increases, because establishing the trustworthiness of strangers is difficult, so the problem will be severe for the emergence of class struggle. A further obvious component of the problem is that those who stand to lose if class struggle succeeds will have an incentive to, as it were, snip the telegraph wires that threaten to carry information that would promote mutual trust among wary class members. Moreover, one may suspect that elements of ordinary

working-class culture will tend to inhibit growth of trust. If the individual harbors moralized desires that strike him as utopian, the resultant dissonance may lead him to want to deny their existence even to himself. A public display of cynicism that aims to persuade the agent himself may be the upshot of this dynamic and may in turn reinforce a similar dynamic in others. Finally, beyond the ignorance *that* others are willing to cooperate, class struggle may also be blocked by ignorance of *how* others are willing to cooperate. People may agree to man the barricades but be unable to agree on which barricades to man, and in this case, too, revolutionary action will fizzle.

The problem of misinformation and mutual distrust, though formidable, hardly seems intractable; it is essentially the difficulty of achieving a coordination equilibrium in a large-number assurance game with multiple coordination equilibria. This problem is helped toward solution by growth in mutual knowledge of players' preferences and strategy choices, whereas a PD situation is only entrenched by mutual knowledge of strategy choices.¹⁶ The problem is rendered less formidable if people's disposition to cooperate includes a willingness to go the first mile rather than merely to reciprocate cooperation by others. We can distinguish a reciprocal and an anticipatory cooperator: the former is disposed to cooperate if and only if he believes others will cooperate also; the latter is disposed to cooperate on the first play (at least where the cost of initial cooperation is not excessive and there is no conclusive reason to believe others will not cooperate) and to continue cooperating on subsequent plays if sufficient others have cooperated on the previous move. In the absence of 1) any initial evidence of a widespread cooperative spirit and 2) excessive first-play cooperation costs, a population of anticipatory cooperators will succeed in establishing cooperation where a population of reciprocators would fail. In this sense a revolution may require the "faith that moves mountains."

Conclusion

The human nature assumptions I have ascribed to Marx are very far from constituting a testable

completely binding constraints" (p. 155), workers do not think in terms of optimizing choice among a range of possible strategies. But this suggestion is wrong. Capitalism may severely restrict workers' freedom but could not entirely eliminate it—even a slave has options. Nor does Roemer's association of a "no choice" mentality with the psychology of convulsive action seem plausible. In time of upheaval worker-radicals debate strategic choices with gusto. Exhilarating political argument widely shared is an important component of the sense of liberation experienced in periods of social turmoil.

¹⁶More accurately: strategy choices here may become interdependent if, for example, information about players' strategy choices becomes available serially. If one player announces his intention to meet others at the czar's palace, the others will follow suit even though many other meeting places were equally eligible before this announcement. (What looks like revolutionary authoritarianism may sometimes be a way of marking as salient one solution to a coordination problem.)

theory. At best these loose assertions form a vision or proto-theory from which a theory might be constructed. But hardly any progress has been made in producing nontrivial theories of human motivation from a nonegoist starting point, so in this respect Marx's analysis has not been superseded. The assumption of rational egoism that he eschews, despite its clarity and elegance, is at variance with established facts of political behavior that any political theory must comprehend. There is spadework yet to be done on the conceptual territory Marx begins to survey.

The doctrine that I have put in Marx's mouth says only that people have some disposition to be moral, where being moral includes cooperating in single-play PD situations, that this disposition is inhibited by material scarcity, which tends to increase the incentive for violating moral norms, and that capitalism for all practical purposes eliminates the necessity of material scarcity. Marx is a moderate millenarian. But the doctrine, if weak, is far from trivial: it denies that human dispositions are indefinitely malleable at the hands of the social environment. When there is not enough to go around, people will fight to advantage themselves over others, and class-divided society inexorably follows. When there is enough to go around, an egalitarian society is feasible, in the sense that its coming would make no improbable demands on human nature, and the measure of "enough" to go around is the per capita income of England after the middle of the

nineteenth century. My characterization of Marx's thought as "millenarian" is not intended to carry any pejorative stigma. Marx is not alone, nor obviously wrong-headed, in resting his normative theory on secular faith that human nature permits the possibility of wide social cooperation to bring about a just or egalitarian society. But the promissory notes in the writings of Marx and others of his ilk who take a "soft" line on human nature have yet to be converted into cash.¹⁷

¹⁷If, as I urge, introducing moralized motivation can save Marx's theory from the charge of incoherence levelled by Olson, it is otiose to deny that Marx's writings are amenable to interpretation in terms of theories of individual rational choice along the lines of expected utility maximization. Holmstrom (1983) asserts that Marx does and Marxists should reject the expected utility approach in favor of a new socialist theory of rationality, but she says very little about what such a theory of rationality might amount to. Nor, it should be added, does Marx even implicitly commit himself to criticism of a time-bound bourgeois theory of rational choice. If I am right, there is no specifically Marxist motivation to cast about for any such criticism or theory. Gary Becker writes, "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (Becker, 1976, p. 4). On my account, the Marxist will want to flinch and relent by postulating stable moralized alongside stable egoist preferences (or equivalently by postulating moral inhibitions that can constrain maximizing behavior).

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Althusser's Marxism without a Knowing Subject

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The quest for unassailable "foundations" for knowledge has preoccupied Western thinkers at least since Descartes. Without some such foundation or Archimedian standpoint, it was argued, our knowledge of the external world as well as our basis for moral and political judgment would fall prey to relativism, historicism, and ultimately nihilism. Recently, though, this Cartesian quest for foundations has come under attack from some of the most powerful minds of our age.

In this article I examine the contribution of Louis Althusser to this current of thought and assess whether his critique of foundationalist epistemologies and ethics can avoid the pitfalls of relativism. Althusser is compared to other thinkers who share his anti-Cartesian persuasion (Wittgenstein, Heidegger, and Foucault) and is criticized for his use of a kind of structuralism to abolish the "knowing subject" as the locus of thought and action. I conclude that Althusser's "antihumanism" has produced a convenient ideology for a new class of Marxist intellectuals to exert their claims to power over ordinary human agents who have been reduced to "bearers" or "supports" of certain systemic, structural relations.

The title of this article is drawn from Karl Popper's 1967 lecture "Epistemology Without a Knowing Subject." Here we read:

We may distinguish the following three worlds or universes: first the world of physical objects or physical states; secondly, the world of states of consciousness, or of mental states, or perhaps of behavioral dispositions to act; and thirdly, the world of objective contents of thought, especially of scientific and poetic thoughts and works of art. (Popper, 1972, p. 106)

It is not my purpose here to examine Popper's theory of the "three worlds" of knowledge or even to compare Popper's theory of scientific method to that of Althusser, although such a comparison might not be unfruitful. I want instead to examine Althusser's own attempt to produce a theory of the products of the human mind, what Popper calls those "unembodied world-three objects" apart from and independent of the "knowing subject."

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The main texts consulted are Althusser *For Marx* (1969), *Reading Capital* (1970), *Lenin and Philosophy and Other Essays* (1971), *Politics and History: Montesquieu, Rousseau, Hegel, and Marx* (1972), and *Essays in*

The attempt to eliminate the subject from the product of knowledge may well strike the reader as perverse. Ever since Descartes it has been the aim of modern philosophy to discover some "foundation" or Archimedian standpoint on which we can ground knowledge (Bernstein, 1983; Rorty, 1979). Virtually the textbook case of this approach is to be found in the opening pages of Descartes's *Meditations*, in which he tells of his efforts to find a secure foundation for knowledge in the "thinking subject" (*ego cogitans*) reflecting upon itself. Indeed, the radical challenge of Descartes was his claim that not tradition, not prejudice, not received opinion, but only the ego's own critical rationality can provide a touchstone for what is to count as knowledge. Although few philosophers today accept the substance of Descartes's argument, his search for some absolute, self-evident, and unimpeachable starting point for knowledge has captured the imaginations of both empiricists and rationalists alike. Without such a foundation, it is argued, our knowledge of the external world, as well as our basis for moral judgment and action, would fall prey to some form of relativism, historicism, or even worse.

And yet, Descartes's emphasis on the primacy of the knowing subject has not gone unchallenged. In opposition to the Cartesian paradigm with its image of the solitary thinker reflecting upon the possibility of knowledge, there has grown a powerful anti-Cartesian counter move-

Self-Criticism (1976). To conserve space, I refer to these works by initials; for example, *For Marx* is cited as *FM*.

ment which has sought to "decenter" the privileged position of the subject. Hegel was perhaps the first to challenge this conception of the autonomous thinker by showing that reason itself has an intrinsically social and historical character. The knower is not apart from, but embedded in, the stream of history that he seeks to comprehend. The search for an Archimedean point for knowledge was pushed even further back owing to the progressive onslaughts of Marx, Nietzsche, and Freud, all of whom, in very different ways, sought to dethrone the autonomy of reason, showing it to be determined by certain subrational or extrarational forces that it failed to comprehend and control. In our own day the works of Lévi-Strauss, Foucault, and Althusser have further contributed to the "end" or "death" of man by showing that the subject cannot be analyzed apart from the overarching structures or systemic relationships in which it finds itself. The Cartesian image of the autonomous self reflecting in splendid isolation has been declared nothing more than a myth in which Western thought has been imprisoned for over three centuries.

This leads us back, then, to a consideration of the possibility of an epistemology without a knowing subject. For whatever we have heard lately about the advent of a "post-structuralist," a "post-Wittgensteinian," or a "post-Heideggerian" age, it is by no means clear that the Cartesian approach to knowledge is simply dead. In our century it has reappeared under the guise of Husserl's "transcendental phenomenology," Sartre's "existential ontology," and more recently in the various "possessive neo-individualist" theories of politics and society (Dallmayr, 1981, pp. 12-16). The works of Rawls, Nozick, and Dworkin all presuppose some Cartesian understanding of the self as unencumbered by any natural or empirically acquired ends or purposes as the only intelligible locus for their theories of rights and justice.

The question, then, is: To what extent can the search for an Archimedean standpoint for knowledge be abandoned and at the same time the pitfalls of relativism, historicism, and ultimately nihilism be avoided? If the Cartesian search for a certain and reliable foundation for knowledge and morality is indeed a myth, it does not follow that the rejection of foundationalism *tout court* will provide us with anything better. In this article I hope to show that a middle position between foundationalism and Althusser's anti-Cartesianism may therefore be defensible. Such a position may help to avoid the difficulties that Althusser's radical antifoundationalism raises for political thought and practice. As I shall argue, the attack upon foundations has simultaneously been an attack upon some of the major concepts

and categories of the western liberal tradition, such as freedom, autonomy, dignity, and rights. It may be that what is at stake in this seemingly abstruse epistemological debate is the very survival of the humanistic tradition of political thought in general and liberal values in particular.

The Banishment of the Author

Althusser's first assault on the subject occurs in his theory of reading and interpretation. No reading, he has maintained, is "innocent," by which he means that no reading can avoid making certain assumptions and presuppositions that prejudice the reader in a certain way (RC, pp. 14-15; cf. pp. 74-78). No interpretation, then, can be entirely neutral or value-free. Althusser's own reading is premised on the assumption that behind the "explicit discourse" of a text, there is a second "silent discourse," the meaning of which is "unconscious" to its author and must therefore be "dragged up from the depths." It becomes necessary then to abandon anything like the author's intention as providing a touchstone for interpretive validity and to focus on that esoteric or "symptomatic" discourse which must be examined for the "absences," "lacunae," and "silences" that the first conceals.²

Althusser launches a powerful attack on the thesis that the interpretation of a text must attempt to recapture an author's meaning in the manner he intended it to be taken. The main presuppositions of this kind of hermeneutic recovery are first, that the author is the sole determiner of the text's meaning; second, that this meaning is in principle objective, reproducible, and accessible to the interpreter who may be reading it from a different tradition or point in time; and third, that the meaning of the text is worth recovering. Such an understanding of the interpreter's art has a long history of its own, going as far back as Spinoza's attempt to recapture the literal meaning of Scriptures, but which more recently has been identified with writers as different as Leo Strauss, Eric Hirsch, and Quentin Skinner.³ Whatever the differences between them, they all maintain that the primary goal of interpretation is to understand the meaning of a text through the recovery of the author's intention and his understanding of his own situation which requires, so far as possible, the elimination of prejudices and assumptions arising from the inter-

²This is not to be confused with the distinction between esoteric and exoteric writing made famous by Strauss (1952, pp. 22-37); see also Strauss (1959, pp. 221-232).

³A useful summary is provided by Gunnell (1979).

preter's own historical horizon. Indeed, in one currently important version of this approach, the interpretation of a text in the past is likened to the understanding of "speech acts" in the present.⁴ The task of the interpreter is to determine "what [the] author, in writing at the time he did write for the audience he intended to address, could in practice have been intending to communicate" (Skinner, 1969, p. 49). It is clear from this remark that temporal distance or alien modes of thought should present no special difficulty to the interpreter trying to capture "what the author . . . could have been intending to communicate." All that is necessary is a knowledge of the prevailing "linguistic context" which determines the "range of possibilities" with which an utterance could have been conventionally uttered.⁵

In contrast to the hermeneutic recovery of meaning, however, Althusser proposes a second, quite different style of reading "with nothing in common with the first" (*RC*, p. 24). This style of reading has nothing to do with understanding a writer in the way that he understood himself, but attempts to go beyond the author's own self-understanding to the "unconscious" presuppositions by which his thought was determined. These unconscious presuppositions Althusser calls the "problematic" of a work or text. This term is taken by him to signify a hierarchical structure of problems which sets internal limitations on what an author can and cannot say. Put in this way, the problematic serves as a kind of ideational infrastructure to the history of ideas in the sense that it determines what kind of problems may be posed as well as what kind of solutions to these problems may be found acceptable. The problematic may serve, then, to mark off the conceptual boundaries that identify the thinking of a writer, a school, or an entire tradition of thought. In this respect the problematic resembles, as Jameson has noted, nothing so much as Collingwood's theory of "absolute presuppositions" particularly in its idealistic character (Jameson, 1972, p. 137).⁶

The importance of the problematic as an interpretive device has, of course, been widely discussed in a variety of disciplines ranging from literary criticism to the history and philosophy of science. Readers familiar with this literature will recognize a family resemblance to Thomas Kuhn's theory of paradigms or conceptual models that

govern the thinking of a scientific community.⁷ The theory of paradigms has been adapted to the study of the history of political thought by Pocock, who has argued that the "linguistic paradigm" of a writer ought to take precedence over the author's professed intention, which can almost be, as it were, deduced from this paradigm. Taking exception to Skinner's appropriation of Austin's speech act theory, Pocock (1971) writes:

Once history is seen in linguistic depth, the paradigm with which the author operates takes precedence over questions of his "intentions" or the "illocutionary force" of his utterance, for only after we have understood what means he had of saying anything can we understand what he meant to say, what he succeeded in saying, what he was taken to have said, or what effects his utterance had in modifying or transforming the existing paradigm structures. (p. 25)

Like Pocock, Althusser accords interpretive priority to the study of language "structures" over individual utterances or even entire texts. The problematic is, according to him, "the particular unity of a theoretical formation" (*FM*, p. 32). It is the overall framework of a theory which puts the basic concepts into relation with one another, determines the nature of each concept by its place and function within the whole, and confers on each concept its peculiar significance. It determines the form or "horizon" within which problems can be posed as well as solutions entertained.

This introduces us to a fact peculiar to the very existence of science: it can only pose problems on the terrain within the horizon of a definite theoretical structure, its problematic, which constitutes its absolute and definite conditions of possibility, and hence the absolute determination of the forms in which all problems must be posed, at any given moment in the science. (*RC*, p. 25)

The problematic, then, delimits the theoretical field governing what is and is not to be included within it. It is, as he puts it, a "determinate unitary structure" unifying all the elements contained therein (*FM*, p. 67).

Althusser develops this concept of the problematic in the context of his controversy over the status of the works of the "young Marx" (*FM*,

⁴The origins of this theory can be found in Austin (1962) and Serle (1969); for a recent effort to develop this theory see Habermas (1979, pp. 1-68; 1984, pp. 288-295, 305-307, 319-328).

⁵For some critical commentaries see Tarcov (1982) and Shapiro (1982).

⁶See also Collingwood (1972, chap. 5).

⁷The critical literature on this subject is too voluminous to recount here; for a sample see Lakatos and Musgrave (1970); MacIntyre (1977); Bernstein (1978, pp. 84-93; 1983, pp. 51-93); see also Miller (1972, pp. 804-806).

pp. 51-86). For some there is a direct continuity between Marx's early "ethical" writings especially, the 1844 *Manuscripts* with their talk of "alienation" and the reappropriation of the "human essence" and the later or mature analysis of the production process in *Capital*. This continuity is said to be guaranteed by the reappearance in the later works of Marx of certain key concepts, notably "alienation," under the guise of the "fetishism of commodities." This is taken to prove that there is no break or caesura between the young and the old Marx as the guarantee of continuity is provided by Marx himself (Avineri, 1968, p. 40). Yet Althusser claims that this purported continuity between the young and old Marx is the result of a "naïve" or "eclectic" reading which takes the literal presence of certain terms at their face value without pausing to consider whether or not the problematic that gives them their meaning had changed. The problem with this approach is that it introduces a sort of cryptoteleology into the study of ideas in its search for continuities and resemblances, germs and anticipations, of a later doctrine within an earlier one (*FM*, pp. 56-57). By doing this, a kind of false coherence or system is placed upon works that may in fact be radically different.

One seemingly peculiar feature of the problematic upon which Althusser insists is that it plays the *active* role in the determination of a theory. Indeed, he attributes functions to the problematic that other epistemologies had done to the human subject. In words that we are enjoined to take "literally," Althusser claims that it is no longer the individual subject who thinks and constructs theories, but the problematic that thinks in and through the subject.

The sighting is thus no longer the act of an individual subject, endowed with the faculty of "vision" which he exercises either attentively or distractedly; the sighting is the act of its structural conditions, it is the relation of immanent reflection between the field of the problematic and its objects and its problems. . . . It is literally no longer the eye (the mind's eye) of a subject which sees what exists in the field defined by a theoretical problematic: it is this field which sees itself in the objects or problems it defines—sighting being merely the necessary reflexion of the field on its objects. (*RC*, p. 25)

This attempt to dissolve the knower into the products of his knowledge is not altogether absurd. By drawing attention to the problematic as the unconscious infrastructure governing the "production" of particular utterances, Althusser wants to show that it is impossible for even the most careful writer to be the sole determiner of the text's meaning. Against the view that an

author has some privileged access to his text's meaning, what is being suggested is that the writer's avowal of his own purpose is to some extent unnecessary once we have the text. What, for instance, Kant may have intended to prove by argument in the *Critique of Pure Reason* is one thing. What he succeeded in saying is another, which can be distinguished from his purpose in writing it. The upshot is that the finished text acquires an "autonomy" of its own apart from the author's private thoughts or mental states. As a critique of the unduly mentalistic concepts like will and intentionality, this is probably correct.⁸ What is more difficult to discern is how this banishment of authorial or communicative intent affects the criteria for determining interpretive accuracy.

In the first place, the suggestion that a problematic or paradigm governs the thinking of a writer or speaker implies a sort of linguistic determinism which cannot account for varieties of linguistic usages. Although ordinary or conventional speech may well be determined by such closed and self-contained frames of reference, these cannot explain how a writer can and indeed often does challenge and go beyond existing linguistic boundaries and in the process say something new. This was certainly the case with Machiavelli, who endowed traditional moral concepts such as liberality, piety, and goodness with new meaning and implications. To attempt to explain Machiavelli's usages solely within the dominant problematic of his day would scarcely do justice to the novelty and originality of his thought. The same would also be true for the other great innovators in the history of political thought. Thus Althusser's linguistic determinism is at fault for viewing language solely in terms of the restraints and limitations that it imposes rather than the creative and dynamic propensities that it affords. Accordingly, when conceptual change is acknowledged, it can only appear as a radical break or rupture (*coupure*) with the past in which one self-enclosed problematic or framework of meaning takes the place of another. Such change is only possible because it breaks all the rules all at once, which is precisely what I want to argue *cannot* happen (*FM*, pp. 32-34; *RC*, pp. 27, 44-45).⁹

Second, Althusser is vague about how the prob-

⁸An early attempt to exorcise the Cartesian "ghost in the machine" can be found in Ryle (1949, pp. 15-24).

⁹Althusser is indebted for his epistemological theories to Bachelard; for a sample of his writings see Bachelard (1934, 1938, 1968); an account of his epistemological history can be found in Lecourt (1969, 1975); see also Schmidt (1981, pp. 86-93).

lematic of a text can be identified and described. Rejecting the view that we can have access to a text through the communicative intent of the author, he seems to regard access not as something discovered by but as created or produced by the reader, the result of an ongoing dialectic between reader and text. But if the problematic is not something already there in the text, how do we know if we have in fact found it? Strictly speaking, we could not speak of finding it at all. The implication is that there can be no single correct reading of Marx or any author. There are only readings produced by the act of interpretation. But by denying that the recovery of an author's intention may serve as the foundation for interpretive accuracy, Althusser leaves himself open to the charge of rejecting any claims for scholarly objectivity or even serious debate among rival interpretations. Thus, to the question of why one should recommend or adopt Althusser's reading of Marx rather than the many others that have presented themselves over the last century, we could do nothing more than express a sort of resigned methodological agnosticism (Giddens, 1976, p. 63; Smith, 1984a, pp. 80-81).

To be sure, Althusser would not be happy with the relativistic implications of his views. After all, he has read and criticized the works of other interpreters—Engels, Plekhanov, Kautsky, Lukacs, Gramsci, and Sartre, to name just a few. Matters of interpretive accuracy cannot, therefore, merely be a matter of indifference to him. His manner of reading Marx assumes a position of superiority to those who have read him before. But in just what respect his reading is superior he is loath to say. Presumably to attempt to identify fixed criteria would be to fall back on the kind of "foundationalism" he has everywhere sought to reject. The most he is willing to concede is that our criteria of interpretive accuracy are internally bound up with the problematics that we are called upon to compare and evaluate. But far from answering this difficulty, this solution evades it. For if our criteria of validation already presuppose what it is that is to be evaluated, there is no way of determining whether one problematic, paradigm, or language game is to be adjudged in any way superior to or more adequate than any other. The result would be a circularity in which the problematics in question would be called upon to validate themselves.

In fact, as I shall show in the next section, this is precisely what Althusser intends in his rejection of the classical "problem of knowledge," the search for permanent foundations or guarantees for knowledge or truth. First, however, I must point out that the model of interpretation just enumerated prohibits us from engaging in a number of activities in which we characteristically do

engage. Whenever we translate from one language to another, analyze critically the values and beliefs of societies different from our own, or even when Althusser criticizes other readings of Marx, we are implicitly assuming the existence of certain common criteria of evaluation that transcend any one problematic or form of life. Of course, that we characteristically do engage in these activities does not render them immune from criticism, but because we are able to rise above any one framework or form of life, we are able to depict either or both in terms of a more synoptic or comprehensive perspective. What I have in mind here is something like Gadamer's conception of a "fusion of horizons," where we are able to depict both our own and an alien way of life as alternative possibilities to certain human constants at work (Gadamer, 1975, pp. 269-274). One advantage of this approach is that it protects us against the claims of a presumptuous Cartesianism to transcend *all* problematics and forms of life without collapsing into ethnocentric prejudice or mere relativism. Thus, if Althusser is to salvage his own reading of Marx from epistemological relativism, it can only be by admitting to a great deal more overlap among different epistemic standards than his theory of problematics would be willing to concede.

The Problem of Knowledge

"To conceive Marx's philosophy in its specificity . . . is to conceive knowledge as production" (RC, p. 58). In describing knowledge itself as a kind of production or practice, namely, "theoretical practice," Althusser has two aims in mind: one positive, the other critical. The positive goal is to highlight the "autonomous" character of theory. As opposed to other Marxist theoreticians who would explain all the basic forms of life activity as derivative from the economic sphere, Althusser proposes to consider the independence of theory (Deprun, 1970, pp. 67-82). "We must recognize," he says, "that there is no practice in general, but only distinctive practices" for "there can be no scientific conception of practice without a precise distinction between the distinct practices" (RC, p. 58). The autonomy of theory apart from the other practices that compose the "social formation" is guaranteed by its own "internalist" or "immanent" form of justification. For Althusser, knowledge is not verified either empirically by reference to "facts" or "brute data" or pragmatically as an expression of social needs or "interests" (as many Marxists might argue). Rather it is said to contain its own canons and protocols of validation that are internal to knowledge alone (RC, p. 141).

The claim that knowledge is "production" is

also used as a polemical weapon against any form of epistemological "foundationalism." By foundationalism I mean here the search for transhistorical or transtheoretical measures, standards, or guarantees against which knowledge may be verified. For Althusser, the search for guarantees is a part of the traditional "problem of knowledge" which he rejects (RC, pp. 52-54). The problem with these previous epistemologies is the belief that absolute or a priori foundations could be discovered upon which knowledge or science could be grounded. An example of this foundationalism would be the claims embodied in classical empiricism. For the empiricist, everything that is to count as knowledge must be capable of verification through perceptual experience. This requirement is concretized in the famous "verification principle," according to which if our statements about the external world are to be meaningful, they must be testable against reality or some portion of it. Only knowledge based on experience can lay claim to objectivity. The problem here is simply that the principle of verifiability cannot itself be verified on perceptual or testable grounds. It therefore contradicts its own premises for determining what knowledge is, and this contradiction, Althusser holds, underlies all such attempts to ground knowledge. The same difficulty underlies the rationalist's search for such principles as self-evidence and logical inviolability as the basis for certainty. Because these principles must claim to precede the knowledge they are called upon to validate, they must either seek other justificatory principles by which to validate themselves or stand condemned of self-contradiction.¹⁰

Instead of attempting to determine a priori rules of evidence for what is to count as knowledge or how knowledge is to be verified, Althusser proposes a quite different procedure, where the knowledge or science in question is called upon to validate itself. The first step in this procedure is the argument that knowledge is not so much discovered as *created*. It is a form of "production" or "practice." He proposes a tolerably specific definition of what is meant by practice.

By *practice* in general I shall mean any process of *transformation* of a determinate given raw material into a determinate *product*, a transformation effected by a determinate human labour, using determinate means (of "production"). In any practice thus conceived, the *determinant* moment (or element) is neither the raw

material nor the product, but the practice in the narrow sense; the moment of the *labour of transformation* itself, which sets to work, in a specific structure, men, means, and a technical method of utilizing the means. (FM, pp. 166-67)

There are four distinct kinds of practice: economic, political, ideological, and theoretical (FM, pp. 167, 229). It is the "combination" or "articulation" of these practices that constitutes the overall "social formation" or "mode of production." Yet while each of these practices is distinct, they still share a common or "homologous" form: a raw material, "the labour of transformation itself," and a finished product. In the case of theoretical practice, the raw materials present themselves as a series of discrete mental events or ordinary, commonplace concepts. The point to be noted here is that evidence never presents itself as a world of "fact" or "brute data" open to direct inspection, but rather as an already existing universe of concepts or linguistic entities. It is not something immediately "given," but is a world already mediated by interpretation and judgment of an "ideological" sort. The raw materials of theoretical practice are, then, a type of thinking, but thinking at a very low level of intellection, what another tradition would call "pretheoretical" reflection or the standpoint of the "natural" attitude.

Characteristically, Althusser displays little concern with the diverse origins and natures of these commonplace concepts that form the basis for knowledge. Although they may precede the coming into being of science, they are an inert, pliant kind of stuff fashioned out of the exigencies of "lived experience." They are, for this reason, all but useless for scientific purposes, since science, as we have seen, comes into being only through a "rupture" with this experience. Accordingly, the decisive "moment" of any science consists in "elaborating its own scientific facts through a critique of the ideological 'facts' elaborated by an earlier ideological practice." To elaborate these "facts" is, moreover, "to elaborate its own 'theory' since a scientific fact—and not the self-styled pure phenomenon—can only be identified in the field of theoretical practice" (FM, p. 184). Whereas a theoretical practice may begin with an existing system of representations, its aim is to produce a "corpus of concepts" that both "rejects the old one even as it englobes it, that is, defines its 'relativity' and the (subordinate) limits of its validity" (FM, p. 185). The point is that theory recognizes no "facts" that do not already presuppose a prior theory. Even in science we move within a circle of interpretation that can never be brought to an end because it is always subject to further interpretation.

¹⁰This critique is brilliantly developed by Hegel (1971, pp. 131-145); for an excellent commentary see Norman (1976, pp. 9-28).

The direction, then, of Althusser's "knowledge = production" thesis is away from any search for epistemological foundations or transcendental guarantees for knowledge and toward some form of internal or immanent criteria of validation. By an internal or immanent (as opposed to a foundationalist) theory of knowledge I mean one that seeks validity claims *within* the knowledge to be verified. In contrast to the claims of a Kantian "first philosophy," which seeks to establish once and for all the conditions of possible knowledge, Althusser maintains that standards of cognitive acceptability are already at hand within existing sciences or "theoretical practices." There is no point, he believes, in trying to establish ideal rules to see whether our knowledge measures up or "corresponds" to an independently existing external world, but rather to see whether the knowledge in question lives up to its own self-imposed standards of adequacy. This, he hopes, will put to rest once and for all the traditional problem of knowledge.

Theoretical practice is indeed its own criterion and contains in itself definite protocols with which to *validate* the quality of its product . . . No mathematician in the world waits until physics has *verified* a theorem to declare it proved, although whole areas of mathematics are applied in physics: the truth of his theorem is a hundred per cent provided by criteria purely *internal* to the practice of mathematical proof, hence by the *criterion of mathematical practice*, i.e., by the forms required by existing mathematical scientificity. (RC, p. 59)

By insisting that theory supplies its own internal criteria of acceptability, Althusser is enabled to avoid the pitfalls of the traditional theory of knowledge. More positively, by adopting the standpoint of an internalist theory, he not only provides an alternative to foundationalist epistemologies with their search for transcendental truths or guarantees but is in accord with much of the recent work done in this area. In the later works of Wittgenstein, for instance, it is argued that epistemic standards must be sought not in some sort of "metalanguage" or "first philosophy" but within established ways of proceeding.¹¹ The use of language itself provides us with criteria of reflective acceptability, so that it is only necessary to check our judgments against existing linguistic standards to discover whether or not we are justified in making them. Nevertheless, the procedure of immanent validation raises

serious difficulties of its own, namely, an uncritical relativism concerning the content of knowledge. Since Althusser's internal criteria are strictly relative to existing ideological or theoretical practices, they can tell us nothing about the truth of these practices.

In the first place, it could be argued that although the logicomathematical procedures of proof referred to in the above passage may be valid for certain restricted areas of scientific inquiry, they cannot hold true for the social sciences, which are bound by the demands of fact or evidence. If there is no body of hard factual evidence that is taken to be objective and independent of the inquirer into it, then we have no compelling basis on which to distinguish truth from falsity. The result of Althusser's rejection of the "problem of knowledge" is to render theory immune to empirical falsification (Callinicos, 1976, pp. 59-60; Thompson, 1978, pp. 10-13, 16-25, 33-35, 39-40). Althusser admits as much when he says that it is necessary to "purify our concept of the theory of history . . . of any contamination by the obviousness of empirical history" (RC, p. 105). But the question remains, if every theory has its own immanent criteria of validity, how is it possible to compare and evaluate different theories? On this account it would seem to be impossible to choose rationally between, say, a scientific and a theological account of the origin and development of the species or between a Marxist and a liberal conception of the state, since both could be held to contain their own immanent logic and standards of intelligibility. The point is that unless our conceptions are at some level based on fact, our whole construction could be rigorously coherent and still be a delusion.

To some extent Althusser's relativism stems from the philosophical source from which his epistemology is culled. The doctrine that "truth is its own criterion" is taken directly from Spinoza, whom Althusser regards as "the only direct ancestor of Marx" (RC, p. 102; cf. ESC, pp. 132-41).¹² In the *Ethics* Spinoza had used this doctrine to indicate the *systematic* character of truth, that truth lies in the whole (Spinoza, 1952, p. 115).¹³ To describe a statement or proposition as true for Spinoza is not to say that it adequately pictures or represents the world. Rather truth lies in the relation between one statement and some logically "concatenated" set of statements that it presupposes. Spinoza could, however, adopt this

¹¹Wittgenstein's turn from ontological realism toward conventionalism has been noted by Rosen (1969, pp. 1-27).

¹²For the relation between Althusser and Spinoza see Eco (1968, p. 360); Deprun (1970, pp. 77-79); Anderson (1976, pp. 64-66).

¹³Spinoza's theory of truth is treated in Hampshire (1962, pp. 86-90, 97-107).

rigorously coherentist theory of truth and still avoid epistemic relativism because he also enlisted the support of a monistic metaphysics to guarantee that the order and connection of things always remains parallel with the order and connection of ideas. "*Ordo et connexio idearum rerum idem est, ac ordo connexio rerum*" (Spinoza, 1951, p. 86). Lacking any such "psycho-physical parallelism,"¹⁴ Althusser can offer no such promise that the real object (*objet réel*) will in any way coincide with its conceptual representation (*objet de pensée*) (RC, pp. 35-42, 62-63, 66-67).

Althusser's response here is to dismiss the whole problem of knowledge, that is, the verification and empirical validation of truth claims as "ideological." In his own words: "We can say, then, that the mechanism of production of the knowledge effect lies in the mechanism which underlies the action of the forms of order in the scientific discourse of the proof" (RC, p. 67). But this offhand dismissal of the problem of foundations by no means succeeds in resolving our difficulties. Without some rules of procedure independent of the practice in question, not only will we be unable to decide between competing accounts but we will be thrown back into the morass of epistemic relativism discussed in the last section.

This raises a further problem. Not only does Althusser's rejection of foundationalism eliminate the constraints of evidence, it tells us nothing of how criteria themselves change over time. Curiously, Althusser's argument that every problematic contains its own internal norms of intelligibility is not unlike the position argued by Winch (1958). In discussing the differences between the claims of theology and those of science, Winch suggests that "intelligibility takes many and varied forms" and that there is no "norm for intelligibility as such." "For instance," he writes, "science is one such mode and religion is another; and each has criteria of intelligibility peculiar to itself. So within science or religion actions can be logical or illogical. . . . But we cannot sensibly say that either the practice of science itself or that of religion is either illogical or logical; both are non-logical" (Winch, 1958, pp. 100-101).

In other words, just as Althusser maintains that no mathematician requires the aid of physics to verify mathematical theorems, so Winch is suggesting that both science and theology have their own internal criteria of intelligibility peculiar to themselves and as such must be judged simply on their own terms. These criteria can tell us what is

to count as true or false, "logical or illogical" within the disciplines or modes of life in question. But they cannot tell us how these disciplines or modes of life can be compared or evaluated, or whether or not they make sense. There is a kind of latent positivism within this approach according to which various theories and language games merely *are* and, as such, must be accepted. There is a paradoxical agreement between Althusser and Wittgenstein's dictum that "What has to be accepted, the given, is—so one could say—*forms of life*" (Wittgenstein, 1968, p. 226). The result of what Gellner (1974, p. 20) has called this "skeptical abstinence from transcendent claims" is to re-endorse uncritically those patterns of life and interaction that merely happen to exist. And this is precisely what we need not, and ought not, to accept.

But this seeming acceptance of the inviolability of our theoretical practices glosses over the discrepancies that can arise not only between but within forms of life, language games, and problematics. The criteria of reflective acceptability operative at any one time are not monolithic; they may not only contain internal incoherences and contradictions but may be challenged by rival practitioners of the same field. Thus Alasdair MacIntyre has said in response to Winch that "at any given date in any given society the criteria in current use by religious believers and scientists will differ from what they are at other times and places. *Criteria have a history*" (MacIntyre, 1979, pp. 66-67; emphasis added). But if the criteria change, they may not be considered all of one piece, and to refer to them as though they were misses a crucial dimension of epistemological change. The more important point is why at some points in the history of thought existing criteria for validating knowledge claims are regarded as satisfactory, whereas at other times anomalies and incoherences become so great as to sanction as "epistemological break."

Finally, Althusser's insistence on the "radical inwardness" of our criteria leads to a severance of theory from practice. He begs the question by referring to theory itself as a kind of practice, but such a conception is totally at odds with Marx's more pragmatic theory of truth (Kolakowski, 1968, pp. 38-66). The basic point of departure for all of Marx's epistemological reflections is the conviction that the relation between the species and its environment is fundamentally one of *need*. As Marx indicates in a number of places, reality itself is a human creation, and theory, man's "practical consciousness," our awareness of the world of things, is defined by its ability to assist us in appropriating the world as the sum total of the possible objects of need. Thus, in the *German Ideology* we find statements like "consciousness

¹⁴The emphasized term comes from Collingwood (1942, p. 8).

is . . . from the very beginning a social product and remains so as long as men exist at all," and more specifically that "language is as old as consciousness, language is practical consciousness that . . . only arises from the need, the necessity, of intercourse with other men" (Marx, 1970a, pp. 49-50).

The implication here is that all thinking is inherently practical or problemsolving in nature. Not truth but success in practice, success in enabling us to acquire the objects of need, is the primary category of evaluation. As Marx indicates in his second *Thesis on Feuerbach*: "The question whether objective truth can be attributed to human thinking is not a question of theory but is a practical question. . . . The dispute over the reality or non-reality of thinking which is isolated from practice is a purely scholastic question" (Marx, 1970a, p. 615). The conception, then, of a "theoretical practice" unconnected to the satisfaction of practical needs would have struck Marx as a typically idealist fantasy.¹³

There may be good intellectual reasons for rejecting Marx's pragmatist or instrumentalist conception of knowledge. After all, not all thinking (e.g., the playful examination of ideas in a Platonic dialogue) is ruled directly by practical necessity. Yet by cutting theory off from any foundation in either nature or social practice, Althusser's solution to the "problem of knowledge" remains subject to the same strictures Marx levelled against Hegel and the German idealists of the 1840s. The idealism of Althusser consists not in the denial of an independently existing external world, but in the positing of a self-generating conceptual universe with no ties to social practice and claiming its own immanent criteria of validity. There is, in the final instance, no interplay or dialectic between "social being" and "social consciousness," but, as with all systems of idealism, the latter is given an "autonomy" of its own apart from the sordid world of politics and history.

Althusser's answer, then, to the traditional "problem of knowledge" is the idea of an immanent critique. An immanent critique is one that rejects the search for natural or apodictic starting points for knowledge and the picture or correspondence theory of truth that accompanies it. Knowledge, one could say, both begins and ends within the sealed chamber of "theoretical practice." "It is perfectly legitimate to say," Althusser remarks, "that the production of

knowledge which is peculiar to theoretical practice constitutes a process that takes place *entirely in thought*" (RC, p. 42). The idea that there are neither transcendent standards, as the Platonist believes, nor "brute data," as the positivist argues, against which our knowledge can be judged adequate testifies to a condition that Rorty (1982, p. xxxix) has described as "decadence" or what I prefer to call left-wing Kantianism.

Althusser's position is "Kantian" because it claims that the mind has no direct access to reality independent of or apart from the linguistic practices that we use to describe, explain, and evaluate it. It is "left" because it maintains that even our most deeply held convictions and beliefs have no permanent fixity or foundation but are the products of a critique of earlier theoretical and ideological practices which are themselves liable to epistemological upheaval in the future. As we shall now see, Althusser's own theoretical practice is premised on a critique of the humanistic conception of the self as a knowing and acting subject. This essentially Cartesian understanding of the subject as possessed of such attributes of freedom, dignity, and rights is not only said to be based on a philosophically false anthropology but one that may now be nearing its end.

The Death of the Subject

Althusser's attack upon the Cartesian search for foundations of knowledge and action in the thinking ego has its final end or purpose in the rejection of any philosophical anthropology based upon the conception of "man." In his own words, Marxism must become a "theoretical anti-humanism" (FM, pp. 229-231, 241). By anti-humanism Althusser means that the self, the human subject, does not so much constitute but is constituted by the structural, systemic relations in which it finds itself. It is the belief not that men make history but that history makes men or that history makes itself that defines this movement (ESC, pp. 35-57). As I shall show later, this anti-humanism forms the Althusserian pendant to the structuralist dictum that "man is dead" (Dallmayr, 1981, pp. 21-29; Smith, 1984a, pp. 192-200).

This theoretical antihumanism has as its purpose the decentering of the subject. Indeed, this act of decentering has been crucial to every modern discipline claiming scientific status. As Freud has indicated, the "naive self-love" of man has become progressively decentered by the advances of modern science (Freud, 1969, pp. 284-285). The first blow fell when Copernicus discovered that the earth was not the center of the universe but a tiny speck within a cosmic space of infinite vastness. The second blow came when

¹³Althusser has made some efforts to correct this "theoreticism" (i.e., idealism); see FM, p. 15; ESC, pp. 119-125; for evaluations of these "self-criticisms" see Callinicos (1976, pp. 107-114) and McCarney (1980, pp. 70-79).

Darwin dislodged the species from its supposedly privileged position within creation, showing it to be just one in a long line of evolutionary forms. The third blow fell when Freud himself showed that the ego, once regarded as the sovereign subject, is not even "master in its own house" but is directed by deeper unconscious drives and purposes. And finally, this process of decentering has culminated in our own day when Althusser, Foucault, and Lévi-Strauss have tried to eliminate the influence of man altogether as a subject of historical and ethnographic research.¹⁶

Althusser's own attempt to decenter the subject has taken the form of an attack on humanism and Enlightenment philosophies of "man." The distinctive feature of traditional humanism, that of Feuerbach for instance, has been the desire to recover or reappropriate for man those attributes that traditional philosophies had ascribed to God or some metaphysical Absolute (*ESC*, pp. 195-207). The most important of these attributes is the capacity for free choice or creative causality. Indeed the belief that we are free agents capable of initiating and therefore responsible for our own actions stems originally from Christianity and the "myth" that God addresses man "by name" (*LP*, p. 166). Humanism is, then, merely an inverted theology. It merely substitutes a generic mankind or human subject as the omnipotent knower and maker of the world. To be sure, this dependence of humanism on theology may be a negative one, but it is a dependence all the same.

The Althusserian critique of humanism itself follows a line of thought first developed by Heidegger in the *Letter on Humanism* (1977, pp. 193-242). There he argued that the "atheist" existentialism of Sartre was nothing more than an outgrowth of the Western "metaphysical" tradition. This metaphysical tradition, stretching from Plato to Marx, has consisted of a series of increasingly disastrous attempts to impose our own conscious designs and efforts on nature. Metaphysics conceals a fundamentally technical interest in gaining mastery and control over both the natural and the social worlds. Such anthropocentric humanism has led to the subjugation and exploitation of all beings, including man, by an aggressively self-centered humanity. Humanism is thus a form of anthropocentric hubris: the deification of man instead of God. To eliminate

any such metaphysics of the subject, it is necessary to decenter or dissolve man by returning to a more primordial sense of Being. Only when the human is understood as rooted in rather than standing over Being can the arrogance of humanism be held in check.

Following Heidegger, Althusser has repudiated humanism not as a "metaphysics" but as an "ideology." Ideology is to be understood here in contrast to science. Ideology, Althusser writes, is distinguished from science in that in ideology, "the practico-social function" dominates the theoretical function (*FM*, p. 231). Elsewhere he comments that ideology is governed by certain "interests" beyond the requirements of knowledge alone (*RC*, p. 141). Ideologies perform the further task of "interpellating" individuals as "subjects." The category of the subject, like that of ego or consciousness, is said to be "constitutive of all ideology" or "all ideology has the function (which defines it) of 'constituting' concrete individuals as subjects" (*LP*, p. 160). It follows, therefore, that ideologies are ensembles of false beliefs not, as Marx believed, because they interpret the world from the standpoint of any one particular class within society, but because they are tied to the claims of a "constitutive subject" or of individuals as self-directing agents. The conception, then, of "a subject endowed with a consciousness" must be rejected as an "absolutely ideological 'conceptual' device" (*LP*, p. 157). It follows further that Marxism qua science must be subjectless, that is, a "process without a subject," a process without end or purpose, unilluminated by any rational necessity or transcendent Absolute (*PH*, pp. 181-183; *ESC*, pp. 94-99). Once history, conceived as a rational or meaningful whole, can be shown to be unintelligible or opaque in its essence, it becomes possible to reduce "the philosophical (theoretical) myth of man . . . to ashes" (*FM*, p. 229).

Althusser's own form of antihumanism begins with the denial that man or even groups of men form the primary unit of Marx's analysis. Marx, Althusser tells us, was not interested in man as such but in certain "ever-pregiven" relations of production which distribute the roles and functions that agents play out in their daily lives. Rejecting Marx's early philosophical anthropology as tainted with the "ideology" of man and its belief in a substantive human nature ("species being"), Althusser cites approvingly from Marx's last work, his *Marginal Notes on Adolph Wagner's "Handbook of Political Economy"* to show that by the end of his life Marx had left the last vestiges of his earlier humanism far behind.¹⁷

¹⁶See Nair (1974, p. 169): "With Sartre there is too much history, with Lévi-Strauss, Foucault, Althusser, and Lacan there is no longer any history; yesterday object and structure were dissolved into the subject; today subject and consciousness are buried in the object. The soul of the world was free and conscious choice, now the unconscious is king and the world has lost its soul."

¹⁷Poster (1975, pp. 67-71); Schaff (1978, pp. 128-139); Kolakowski (1978; p. 486).

"My analytical method," Marx says there, "does not start from man but from the economically given social period" (Cited in *FM*, p. 219). Not our lived experience but the various structures of social life—economic political, ideological—constitute the real "subjects" of history.

Within Marx's own writings, Althusser finds warrant for this antihumanism in Marx's use of the term *Träger*, which is a fairly commonplace German word that means literally "bearer" or "support." Althusser does not, therefore, merely import this term from the outside to give his Marxism a structuralist turn. Rather it is a concept that Marx uses regularly throughout *Capital* to show the way in which economic agents are turned by capitalism into "personifications" of the social relations of production. Thus at one point we read that, "In the course of our investigations we shall find, in general, that the characters who appear on the economic stage are but the personifications [*Träger*] of the economic relations that exist between them" (Marx, 1970b, p. 85). The difference is that whereas Marx uses this concept as a means to indict capitalist society for treating men as things, Althusser uses it to show that the very idea of man is a chimera that we would be better to drop altogether.

Althusser uses Marx's *Träger* concept to show that economic agents are never anything more than "bearers" or "functionaries" within a given mode of production. There are only a limited number of places and functions that these agents can occupy which are continually reproduced and continually develop what they demand of their occupants. Men cannot, therefore, be regarded as active creators or makers of this process. They are never anything more than its "supports."

The structure of the relations of production determines the *places* and *functions* occupied and adopted by the agents of production, who are never anything more than the occupants of these places, insofar as they are the "supports" [*Träger*] of these functions. The true "subjects" (in the sense of constitutive subjects of the process) are therefore not these occupants or functionaries, are not, despite all appearance, the "obviousness" of the "given" of naive anthropology, "concrete individuals," "real men"—but the definition and distribution of these places and functions. The true "subjects" are these definers and distributors: the relations of production (and political and ideological social relations). (*RC*, p. 180)

Elsewhere Althusser goes so far as to suggest that the production and reproduction of social life is not something carried out by intelligent agents with awareness of what it is they are doing, but takes place, so to speak, behind the backs of men who are never anything more than the supports of

the "mode of production" within which they find themselves. There is no action in the Althusserian universe where this is understood as purposive behavior directed toward the pursuit of some freely chosen end or purpose. There is only a set of reactive responses determined "in the last instance" by the needs of the production process. The stage, Althusser says, is set and the players merely perform according to scripts already written out for them in advance.

Now we can recall that highly symptomatic term "*Darstellung*," compare it with this "machinery" and take it literally, as the very existence of this machinery in its effects: the mode of the stage direction (*mise en scène*) of the theatre which is simultaneously its own stage, its own script, its own actors, the theatre whose spectators can on occasion, be spectators only because they are first of all forced to be its actors, caught by the constraints of a script and parts whose authors they cannot be, since it is in essence an *authorless theatre*. (*RC*, p. 193)

These passages provide literally textbook examples of what Dennis Wrong (in another context) has called the "oversocialized conception of man" (Wrong, 1961, pp. 183-193; cf. Zetterbaum, 1971, pp. 240-246). On this account what we are is so completely determined by the "places and functions" that we occupy that there is nothing else left over. There is no Archimedean point of exemption of the kind searched for by Descartes, no transcendental subject of the Kantian or Husserlian variety capable of standing outside experience, for the fact is that we are conditioned beings all the way down. By rejecting the idea that there is a permanent nature or essence of man which is, to be sure, shaped and molded by culture but which may also stand in a position of conflict or tension with that culture, Althusser has provided a structuralist conception of the self as a personification or passive functionary of the empirically determined circumstances in which it finds itself. The oversocialized conception of the self aims to provide an answer to the age-old question, "How is society possible?" or "What are the sources of social order?" by claiming to demonstrate that we are exclusively the products of the social roles we inhabit.

The idea, of course, that human beings are molded by circumstance or that we become what we are in and through interaction with others has long ceased to be controversial. One may be a feudal lord, a serf, a property-owning bourgeois, or a university professor, and to an extent one's behavior is determined by the norms and expectations governing those roles. But to say that *everything* we do (or refrain from doing) can be explained by the "places and functions" we oc-

cupy is by no means obvious. Althusser is so concerned to deny the humanist thesis that men make their own history and in doing so make themselves that he overlooks the fact that the production and reproduction of social life are above all skilled performances sustained and made to happen by intelligent social agents who act in the light of their understood situations. Seen in this way, individuals are never merely representatives of a set of prescribed social roles, nor can human actions be explained solely in causal or functional terms, that is, in terms of how they keep the system going. Social actors must be understood at least in part as intentional subjects acting in response to an understood situation and whose actions must also be seen in terms of their symbolic or meaningful character for the agents themselves. It is the meaningful side of human action that Althusserian structuralism fails to grasp (Giddens, 1976, pp. 44-48, 74-77; Winch, 1958, pp. 43-53, 116-120).

It should now be clear that the aim of this "oversocialized" conception of the self is to jettison whatever permanent core or substance was once thought to belong to a human being and to substitute in its place the conception of the self as a bearer or functionary, a "role player" as we might say.¹⁸ When, for instance, premodern thinkers regarded the self, they thought largely in terms of character or even soul. The soul in turn was thought to have a distinct nature defined by a hierarchy of needs dictated by the concerns for human excellence and nobility. When even modern thinkers like Kant spoke of the essential freedom or "dignity" of man, he meant not each individual but the totality of the species as "represented" by or embodied in the individual subject. In contrast, then, to this search for any defining center or core to which the universal concept of man might refer, Althusser prefers to dissolve the self into the social *relationships* which uniquely determine it. Indeed, he is fond of quoting from Marx's sixth *Thesis on Feuerbach* as an anticipation of this decentered self. "The human essence," Marx says there, "is not an abstraction inherent in each individual . . . it is the ensemble of social relations" (Cited in *FM*, pp. 227-228, 242-243).

This dissolution of the self into its nexus of social relations is no mere eccentricity on the part of Althusser. His denial of any finality or fixity is, as I have already indicated, part of a broader counter movement now underway in contem-

porary social and political theory whose outcome has been declared under the slogan "the death of man." Along with Althusser, the most prominent member of this school has been Michel Foucault, who has written that Nietzsche's declaration of the death of God has been followed one hundred years later by the end of his murderer. In fact the conception of man as an agent or center of initiative endowed with the capacity for free, purposive, and responsible action is only as old as Rousseau and Kant. "Before the end of the eighteenth century," Foucault writes, "man did not exist . . . he is a recent creature which the demiurge of knowledge fabricated with its own hands less than two hundred years ago" (Foucault, 1970, p. 308). But just as this "creature" has come into being, so too in our own day do we see it beginning to pass away. "As the archaeology of our thought easily shows, man is an invention of recent date. And one perhaps nearing its end" (Foucault, 1970, p. 387).

This proclamation of the end or death of man should not be taken simply as a prophecy of doom. The dissolution of the subject does not mean that human beings as a species are bound to disappear. What will disappear is not man as such but one historically specific conception of man as a thinking and active subject who is simultaneously both knower and maker of the world. What will also disappear is the attempt to ground the human in terms of purportedly permanent attributes like freedom, autonomy, dignity, or rights. These categories and concepts belong to only one epoch in the "archaeology of knowledge," which is now nearing its end. Thus it is the search for some more basic and permanent foundation to the self that is destined to disappear, "like a face drawn in the sand at the edge of the sea" (Foucault, 1970, p. 385).¹⁹

This dissolution of the self with which I have been concerned here results from a critique of Enlightenment humanism that Althusser shares with some of the most powerful thinkers of our age. By contrast to the conception, extending as far back as Protagoras, that "man is the measure of all things," the Althusserian critique of humanism stresses the insurmountable limitations imposed upon man by those non-human or extrahuman relations of production, the "places and functions" that we occupy. Man is no longer conceived as the active creator of the world, but as a supporting agent in a complex web of relationships whose axis revolves around the relations of production.

¹⁸This is developed by Goffman (1959); for an excellent critique of this kind of sociology see Wrong (1961, pp. 183-193); see also Smith, (1984b, especially pp. 528-535).

¹⁹A more developed account than I am able to give here can be found in Garaudy (1969, pp. 229, 238, 241-250); Poster (1975, pp. 334-339); Dreyfus and Rabinow (1982, pp. 28-43).

Like Spinoza's Substance or Heidegger's *Dasein*, these "structures" denote the preexisting world that is the product of neither individual or collective design and of which human agents are ultimately attributes. The result of this critique has been an astonishing reversal of the teaching of Descartes. Rather than "the masters and possessors of nature," we have become its servants.

Conclusion

The Althusserian critique of humanism and its search for substantive foundations for both knowledge and action is directed against some of the most deeply held convictions of the western liberal tradition. The most cherished of these convictions is man, the human subject, whose moral worth resides in a capacity for reasoned judgment and the ability to conform conduct to the dictates of this judgment. The capacity for judgment, and of free agency that this implies, is the distinctive mark of the human. What distinguishes liberalism not only from classical political philosophy but from the variety of modern antiliberalisms is its uncompromising insistence on the power of autonomous human reason as the basis of our claim to be treated with equal "concern and respect."

The attempt to displace this predominantly liberal conception of man as an active and knowing subject, the bearer of rights and obligations, is at bottom what Althusser understands by "Leninism" (RC, p. 141). Leninism, the justification of rule by an elite or a central committee, is only possible when human beings are denied the capacity to act, the ability to make or create their history, and are instead regarded as an inert or recalcitrant mass, there to be molded and manipulated by a class of master technicians or engineers.

Indeed, Althusser's seemingly benign insistence on the "autonomy" of theory is but one more sophisticated way of justifying the dominance of the Marxist intelligentsia claiming to speak for or in the true interests of the proletariat. The result has been the creation of an inward-looking, self-sufficient theoretical culture with no generic ties to the working classes, a development, needless to say, that Marx scarcely intended or expected to happen (Berki, 1975, p. 72). This goes some way toward explaining, I think, why this kind of Marxism has met with widespread acceptance in the universities and among the intellectuals while being ignored by ordinary people.

There is, of course, a final irony in this situation. If Marx was correct when he argued that the ideas of thinker must in some sense express the values or interests of his class or social position, then this must be *ipso facto* true for Althusser and his contemporaries. Althusser, I would suggest, is part of a new technocratic intelligentsia which is now in the process of formation. That such a "new class" of Marxist intellectuals is now seeking to provide itself with some legitimacy is a phenomenon attested to by writers on both sides of what it is no longer fashionable to call the Iron Curtain (Djilas, 1957; Kristol, 1976; Parkin, 1979). It is a distinct possibility that a theory that regards men as attributes of agentless structures may well express a reality dominated by impersonal bureaucratic, if not to say technocratic, methods of control. Such methods may well reduce human agents to *Träger*, mere functionaries rewarded by their ability to adapt or respond to the exigencies of the system of production. It is an arresting thought that Althusser's Marxism without a knowing subject is the best expression to date of the soul of modern bureaucratic rule, what one critic has called "the rule of nobody" (Arendt, 1969, p. 81).

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Alexis de Tocqueville on Political Science, Political Culture, and the Role of the Intellectual

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According to Tocqueville, the most important determinant of the character of any society is its political culture (moeurs). A political culture is shaped not only by sociological conditions and laws, but also, in modern times, by ideas propounded by intellectuals. In Tocqueville's day, two dominant schools of thought were contending for influence over the public mind in Europe: philosophic rationalism and traditionalism. Neither one of these schools, Tocqueville argued, promoted a political culture that could reconcile liberty and democracy. Tocqueville conceived his "new political science" as an alternative to these schools that could meet this challenge. Unlike the opposing schools, the new political science could not be propagated directly as an ideology. Its implementation relied on an indirect strategy—using institutions to inculcate certain "mental habits" among the citizens. This in turn called for ways of limiting the role of intellectuals in influencing political culture.

Alexis de Tocqueville left America in 1832 with the intention not merely of describing its political and social institutions, but of creating a "new political science for a world itself quite new" (p. 17).¹ The object of the new political science was to clarify the forces shaping the modern world and to provide "those who now direct society" with the knowledge to meet its greatest challenge: reconciling liberty and democracy (the social state characterized by an "equality of conditions") (pp. 9, 12). As Tocqueville told John Stuart Mill in 1836, "America was only the frame, my picture was Democracy" (*Mem 2*, p. 38). Mill recognized the scope and novelty of Tocqueville's project when he described *Democracy in America* as "the first philosophical book ever written on Democracy as it manifests itself in modern society" (1961, p. 122).

Tocqueville addressed *Democracy in America* to the entire generation of modern readers, but his immediate audience was clearly European, not American. No narrow chauvinism was at work here, but instead a frank assessment of the status of philosophy in different nations of the world. Nineteenth-century America hardly needed and could scarcely digest a work like *Democracy in America*: "The spirit of the Americans is averse to general ideas. . . . They pay very little attention to the rival European schools [of philosophy]. Indeed, they hardly know their names" (pp. 301,

429). America at the time lay at the intellectual periphery, but for the moment at least ignorance was bliss. Without either a taste for abstract political thinking or a large class of intellectuals, America alone had succeeded in reconciling democracy and liberty. Nor was this a mere coincidence: America, Tocqueville contended, had met the challenge of the modern age not in spite of, but in large measure because of, the lack of influence of recent doctrines in political philosophy and the absence of a class of intellectuals to spread them.

According to Tocqueville, the role of political philosophy in Europe could hardly have been more different. Contemporary thought, purveyed by a powerful intellectual elite, had a tremendous influence on European societies. Political philosophy in Tocqueville's time was divided between two basic schools that were openly competing for dominance over the public mind—philosophic rationalism (the more influential school) and traditionalism. In Tocqueville's view, the tenets of both of these schools tended ultimately to undermine liberty. Political thought had lost a constructive role as a guide to political action and was promoting the march towards despotism. This intellectual crisis demanded a theoretical response—a new political science—the rudiments of which Tocqueville discovered in his study of America's ostensibly unsophisticated political culture. Seldom has an anthropological expedition yielded so much for a philosophic debate.

Although not the first to study what political scientists today call political culture, Tocqueville applied that concept to the analysis of democratic states with greater rigor than any previous thinker. The character of a regime, in Tocqueville's view, is determined more than anything else by its *moeurs*, defined as "the sum of the moral

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¹*Democracy in America* is cited by page number without any title abbreviation. Tocqueville's other texts are cited by the abbreviations noted in the reference list.

and intellectual dispositions of men in society . . . the habits of heart . . . [and] the sum of ideas that shape mental habits" (pp. 305, 287). The importance Tocqueville ascribed to *moeurs* (or political culture) accounts both for the scope of his political science, which extends to what we today would consider to be sociological and cultural elements of society, and for its distinctive method of studying a political system, under which institutions are analyzed as much in terms of their impact on the mental habits of the citizens as by their internal efficiency. Of all the aspects of the American regime, it was the *moeurs* of the people that Tocqueville believed best accounted for America's success. The United States was characterized by less brilliance and possessed less efficient political institutions than some nations in Europe, but it had developed the mental habits that could sustain liberty (pp. 307, 308).

In searching for the factors that form a political culture, Tocqueville, following Montesquieu, discussed the influence of such fundamental causes as geography, history, and laws.² Because these factors are in some degree particular to the experience of each nation, it is more correct in discussing Tocqueville's thought to speak in the plural of political cultures than of a single political culture. Yet what struck Tocqueville about the modern age was the emergence of certain factors that might influence all developed nations along the same lines: "Democratic peoples . . . in the end come to be alike in almost all matters. . . . [T]he same ways of behaving, thinking and feeling are found in every corner of the world" (pp. 660, 615). The two most important forces Tocqueville identified as causes of this common development were the spreading social condition of equality and the emergence of modern political thought as an active force capable of influencing the political cultures of all societies.

To understand the possibilities implicit in this path of development, Tocqueville abstracted from the particular nations and political cultures of his day (e.g., the United States, France, and England) in an effort to discover the picture of a common regime or political culture (pp. 417-429). This abstract regime, which Tocqueville originally referred to as "Democracy," consisted of a set of

properties and tendencies that, if left to their normal course of development, would more likely eventuate in despotisms, whether of the usual or new tutelary variant, than republics. Although these tendencies operated in some degree on all modern societies, they were refracted by the special characteristics of each nation's political culture, with the result that different nations were more or less likely to control the despotic tendencies of democracy and establish liberty. Tocqueville chose the United States as the obvious place to study the properties of democracy, but it turned out that the United States was in many respects atypical of his general model of democracy, mainly because of the special intellectual foundation of its political culture.³

This article proposes to demonstrate that Tocqueville intended his new political science as an alternative to rationalism and traditionalism that could help shape the political culture of modern states to avert despotism. In light of the threat to liberty posed by these rival schools, Tocqueville held that political science could not remain aloof or detached, but had to become an active force in history. Yet in no sense did he conceive of the new political science as an ideology that could convince public opinion by the force of its popular appeal. Rather, it represented a complex teaching that synthesized certain epistemological elements of rationalism and traditionalism into a truer picture of reality that was intended to help his contemporaries make responsible choices about their future. The factors shaping the development of democracy, although increasingly common to all nations, were not entirely beyond human control, either within each nation or within the developed world as a whole. The decisive choice between freedom and despotism would be largely determined by the doctrines and ideas that guided people's thought (p. 705).

In developing this thesis, it will be helpful at the outset to indicate the different parts of the argument. I begin by tracing Tocqueville's account of the development of rationalism and traditionalism, turn next to a sketch of the purpose and general character of his new political science, then outline his strategem for inserting the teachings of the new political science into society, and finally indicate the manner in which he mixed rationalism and traditionalism to create his distinctive synthesis.

²As Tocqueville wrote in a letter to Kergorlay in 1836, "There are three men with whom I commune a little every day; they are Pascal, Montesquieu, and Rousseau." Of these Montesquieu seems to have been the most influential, and 15 years later Tocqueville wrote the same friend that he was modeling his next work, *The Old Regime*, on Montesquieu's *Considerations on the Causes of the Grandeur and Decadence of the Romans* (Herr, 1962, pp. 48-49).

³Tocqueville had trouble with his use of the term democracy to describe a regime that was not free, and by the end of his life ceased using the term in the earlier sense. See his notes for the second volume of *The Old Regime*, the key passage of which is translated in Aron (1968, pp. 240-241).

Rationalism and Traditionalism

Tocqueville's point of departure in both of his major works was the reaction of his contemporaries to democracy and to the shattering event that brought democracy to the center stage of European consciousness, the French Revolution. On one side, according to Tocqueville, stood the proponents of a radical version of rationalism, who saw themselves as the heirs of the French philosophes. For these thinkers, the French Revolution marked the dawn of a new era in which philosophy for the first time became an active force in transforming society. Philosophy stripped away the cloak of tradition that had supported superstition, injustice, and parochialism, providing in its place universally valid principles, derived from reason, that could instruct the enlightened on how to re-make society and construct new political orders (pp. 429-433; *OR*, pp. 13-169).

On the other side were the proponents of traditionalism or "organicism," who turned for their inspiration chiefly to the writings of Edmund Burke. This school, which emerged as a conscious reaction to philosophe ideas, attacked the abstract, "speculist" character of rationalist thought, seeing in its universalism and contempt for tradition a dangerous doctrine that threatened political stability, diversity, and the noblest elements of civilization. Only by a renunciation of this doctrine and a return to respect for tradition—meaning an appreciation for hierarchy, diversity, and transcendence—could the world be saved from the ambition of the philosophes, who, according to Burke, had ushered in "a barbarous philosophy" that had "rudely torn off . . . all the decent drapery of life" (Burke, 1955, p. 87). About all both schools could agree on was that philosophe ideas had prepared the way for, if indeed they had not been the chief cause of, the French Revolution.⁴

Tocqueville analyzed the development of rationalist thought up to his time by distinguishing four different stages. The first stage, which occurred in the sixteenth century, focused on the relationship of man to God; religious reformers "subjected the dogmas of ancient faith to reason," while still maintaining a belief in the Deity (pp. 430-431). The second stage, a development of the

seventeenth century, saw the application of this way of thinking by Bacon and Descartes to the realms of natural science and philosophy (metaphysics); these philosophers consciously defined the mode of thought known as "method" and launched a philosophic project that "destroyed the dominion of tradition and upset the authority of masters" (p. 431). The essence of method was a reliance on individual reason, in the light of experience, as the standard for evaluating truth, rather than prejudice, revelation, or tradition (pp. 429, 431).

It remained, however, for a third stage to extend the principles of method to the social and political spheres, turning the world upside down and spreading in the public mind a mode of thinking "by which all ancient things could be attacked and the way opened for everything new" (p. 431). This approach was popularized by the eighteenth-century thinkers, and in particular by the French philosophes and economists (p. 431; *OR*, pp. 158-169). The key to the philosophe position was the idea that "what was wanted was to replace the complex of traditional customs governing the social order of the day by simple, elementary rules deriving from the exercise of human reason and natural law" (*OR*, p. 139). Tocqueville was aware, of course, of other strands of contemporary rationalist political thought that were not as radical or abstract as the French school, and he was personally very appreciative of certain English rationalists like Hume (*OR*, pp. 152-154). Yet his purpose here was not to survey the thought of different writers, but to sketch the core principles of the ideas that had helped to shape the political cultures of his day. In Tocqueville's view, the philosophe school had clearly exercised the greatest impact upon the public mind, becoming "the common coin of thought" throughout much of Europe (p. 431).⁵

Under the influence of the philosophes, French writers of the next generation extended their mode of thinking to "an unrestrained passion for generalizations" (p. 438). This marked yet a fourth stage in the evolution of rationalism contemporaneous with Tocqueville. "No writer . . . is satisfied with an essay revealing truths applicable to one great kingdom, and he remains dissatisfied with himself if his theme does not em-

⁴The idea of studying Tocqueville's thought as a response to these two schools was suggested by J. P. Mayer (1960, pp. 111-112), who also noted that the influence of Burke on Tocqueville has never been sufficiently explored in the scholarly literature. For my treatment of rationalism, I also rely heavily on the ideas of Peter Augustine Lawler, Edward Gargan (1965) and Pierre Manent (1982).

⁵The account of rationalism here is presented from Tocqueville's point of view. An account from the outside would also be helpful, and one reader of this article suggested distinguishing between an analytic rationalist tradition, which went from Bacon and Descartes through Hume, Smith, Montesquieu, and Tocqueville, and a rationalist tradition, in which the thinker adopts the perspective of an omniscient observer, which would include Saint Simon and Hegel.

brace the whole of mankind" (p. 438). French thought, which in contrast to English thought brought almost no practical experience to bear in writing on politics, was characterized by a "literary spirit" largely devoid of any contact with reality (*Rec*, p. 84; *OR*, p. 148). It built abstract, utopian models, ignoring the real world and real constraints. In France at least, this "literary spirit" came to shape the public's intellectual dispositions: "It was this vision of the perfect state that fired the imagination of the masses. . . . Turning away from the real world around them, they indulged in dreams of a far better one and ended up by living, spiritually, in the ideal world thought up by the writers" (*OR*, p. 146; cf. p. 670, *Rec*, pp. 78-79).

By the end of the eighteenth century, what passed for rationalist thought was something very different from the original rationalist method identified by Bacon and Descartes and later pursued by Hume and Montesquieu. Whereas rationalism had begun as an inductive or empirical mode of thought characterized by the careful movement from the particulars of experience to generalizations, it evolved by the end of the eighteenth century into an abstract and deductive mode of thought in which authors fabricated their own internal systems and then sought to impose them on reality. Tocqueville regarded this shift from an empirical mode of thought to "a passion for generalization" as a clear perversion of the original spirit of rationalism. All that remained was a suspicion of tradition, and even this had been transformed into an irrational prejudice against the old. Although Tocqueville did not object to the spread of all general ideas, and in particular endorsed the idea of the rights of man, he was convinced that the mental habits inculcated by modern rationalist writers were destructive of the cause of liberty. It was only in America that rationalism in its original, empirical form was practiced in daily politics: "America is the one country in which the precepts of Descartes are least studied and best followed" (p. 429).

Traditionalism received a less systematic treatment from Tocqueville, probably because of its more recent origin. Although Tocqueville was thoroughly familiar with the precepts of this school from the thought of French writers like Joseph de Maistre, he apparently did not trace these ideas to their source until making an in-depth study of Burke's thought during his visit to England in 1833.⁶ Tocqueville ultimately disagreed with the traditionalist outlook, but he

shared many of its points of criticism of philosophic rationalist thought, and among his contemporaries he clearly directed his works to moderates of the traditionalist persuasion, hoping to change their minds (pp. 16, 17).

Although the traditionalists of Tocqueville's time ranged across a broad spectrum of opinion, their diagnosis of the evils of philosophic rationalism contained a common theoretical position. Traditionalists rejected abstract, metaphysical thought in politics and dismissed a priori reasoning. Burke (1955, p. 69), for example, regarded political science as a "practical science" and emphasized the need to view political phenomena in terms of concrete circumstances and particulars. Traditionalism thus presented itself partly as a defense of the realm of prudence for the statesman against the misinformed intrusion of speculative philosophers. In this respect, traditionalism was consistent with the pragmatic character of original rationalist thought, which counseled making judgments on the basis of experience. But traditionalism went far beyond a reliance on this form of reason in its assault on rationalism.

First, traditionalists attacked metaphysical speculation, and sometimes reason itself, in the name of a veneration for the old and familiar. They upheld the "prejudices" of the community and glorified an "instinctive patriotism" (p. 235). For the traditionalist, Tocqueville explained, "the idea of evil is indissolubly linked with that of novelty" (p. 17).

Second, and following from this depreciation of reason, traditionalists celebrated the heritage of each nation as it emerged as the product of the particular and accidental forces that shaped its development. According to traditionalists, historical bonds of union formed the only sound basis for community, not individual reason or the individual's calculation of interest (p. 236). Each nation's constitution was the product of—and ought to be bounded by—its particular historical development. Beginning from this "organic" perspective on society, traditionalists viewed the homogenizing effects of universalistic ideas with profound suspicion. Universal ideas eroded the basis of community at the same time that they destroyed the charm of diversity in the world.

Finally, traditionalists strongly supported religious faith, especially as it manifested itself in the particular religious institutions of each society. Traditionalists identified speculatist philosophy with irreligiosity and sought to make

⁶For the influence of Burke on French thought, see Nisbet (1970, p. 48) and Reardon (1975, p. 22). Burke was also highly influential in German conservative

thought, which Tocqueville studied directly and learned about through Gobineau (Chapman, 1967, pp. 4-12).

common cause with the faithful by contending that religion was one part of an integral world view being attacked by rationalist ideas (*OR*, p. 154).

All of these properties of traditionalism can be found in Burke's writings, but they received a simpler, and more extreme, expression by Joseph de Maistre:

[Man's] cradle should be surrounded by dogmas; and when his reason awakes all his opinions should be given him. . . . Nothing is more vital to him than prejudices. . . . Religion and political dogmas, mingled and merged with each other, should together form a general or national mind sufficiently strong to repress the aberrations of the individual reason. (Reardon, 1975, p. 24)

After sketching the development of these two schools of thought up to his day, Tocqueville turned to their likely evolution and to their consequences on political culture. Although each school claimed in its own way to support liberty, Tocqueville contended that both ultimately denied that individuals could control their own destiny and therefore eroded a fundamental belief that supported liberty. To maintain liberty, Tocqueville argued, requires more than the acceptance of an idea of rights and certain favorable institutional arrangements, however important each of these may be. Liberty requires a metaphysical foundation in certain views respecting historical causality and free will. It depends on the fact that people can choose and that their choices can make a difference, and on the fact that people *believe* that they can choose and that their choices can make a difference.⁷ Without this belief, people lose their "will" to choose and can see no reason to support the idea of rights or legal institutions designed to protect liberty (p. 705). It was precisely this belief in man's free will, according to Tocqueville, that rationalism and traditionalism called into question.

Philosophic rationalism began with the seeming assertion of boundless human choice: "It has been said that the character of the philosophy of the eighteenth century was a sort of adoration of human intellect, an unlimited confidence in its power to transform at will laws, institutions, customs" (*OR*, p. 281). Man can make the world

according to the dictates of reason, overthrowing the tyranny of tradition and accident. Yet in its ever-expanding quest for universal principles of explanation, philosophic rationalism led paradoxically to the negation of the conditions for choice. The search for general and universal causes ended by positing the source of human activity not in the decisions of particular individuals, but in mankind as a whole or, beyond, in some immanent force in nature or history. Tocqueville labeled this mode of thinking "philosophic pantheism." By tracing the springs of human activity to abstractions like mankind or history, this doctrine rendered human choice meaningless. Choices for mankind or societies, Tocqueville argued, can only be made by individuals on behalf of the species; the species itself cannot choose. Once one posits the power over human affairs in the species, that power lies with no one in particular and hence with an invisible system that operates on all: "Individuals are forgotten, and the species alone counts" (p. 451). With vast, general causes thought to be controlling their destiny, people lose all will to assert themselves, and they become weak and phlegmatic.

If philosophic rationalism carried to the extreme leads to one form of denial of human choice, traditionalism carried to the extreme leads to another. The result is equally paradoxical in light of the traditionalists' expressed goal of preserving a realm of discretion for the statesman. Yet by depreciating reason and making the heritage of each society both the cause and standard for its regime, traditionalists implied that there were no fundamental choices; all was as it had to be, or nearly so. The protection traditionalists sought to gain for the statesman in the realm of prudential decision making was thus bought at the cost of denying human choice on fundamental questions. "Each nation," in the traditionalist view, "is inexorably bound by its position, origin, antecedents, and nature to a fixed destiny which no effort can change . . . [it is] the product of pre-existing facts, of race, or soil, or climate" (pp. 705, 496). Following Tocqueville, John Stuart Mill gave his classic formulation of the traditionalist position: "[Governments] are a sort of organic growth from the nature and life of that people: a product of their habits, instincts, and unconscious wants and desires, scarcely at all of their deliberate purposes" (1958, p. 4).

Traditionalism, then, leads to a doctrine of historical inevitability, although not of the sort with which we are most familiar today. Ever since Hegel, and especially Marx, doctrines of historical inevitability have tended to assume the form of universalistic theories in which the forces at work in history are said to include all of mankind. This post-Hegelian form of historicism folds the

⁷In his correspondence with Gobineau, who contended history was the product of racial characteristics, Tocqueville told his friend, "I believe that [your doctrines] are probably quite false; I know that they are certainly very pernicious" (*ER*, p. 227). Tocqueville judged an idea by the dual criteria of its "philosophic merits" and "its moral and political effects" (*ER*, p. 232).

universalistic ideas inaugurated by the philosophes into an inevitable movement of *world* history in which all are swept along in accord with some plan that flows either from the movement of Mind or material forces. But doctrines of historical inevitability were originally an outgrowth of traditionalism, and their source can be traced to Burke. This earlier form of historicism sought to demonstrate that the cause of human societies lay with the particular and accidental forces that operate on each nation (or *volk*).⁸ Although Tocqueville glimpsed the possibility of a universalistic historicism and advanced one such mild variant of his own, he identified the historicism of his day mainly with those who sought to explain the fate of *particular* nations or peoples.

Notwithstanding their attempt to defend rank and hierarchy, traditionalist doctrines of historical inevitability represented a democratic mode of thought in the sense that peoples, races, or climates, but not individuals, control historical development. While Tocqueville thought that traditionalist historicism might preserve a certain diversity in the world among nations or races, he believed that it was no less hostile than philosophic pantheism to the idea of human choice. It left man with the view that his actions could make no difference and that "societies unconsciously obey some superior dominating force," which was precisely the belief that prepared the way for despotism (p. 495). By extending Tocqueville's analysis, one can glimpse in this organic variant of historicism, as transformed by the atheism of Nietzsche, the seeds of the radical "democratic" Right (fascism), just as it is possible to see in rationalism, as modified by Marx, the grounds of the radical "democratic" Left (communism).

Thus rationalism, by leading to philosophic pantheism, and traditionalism, by leading to historicism, deny free will. To promote the beliefs that sustain liberty, Tocqueville sought to free man from the sway of these ideologies. His alternative, the new political science, presupposed a significant, although not unlimited, degree of

choice: "One may say that it depends on us whether in the end republics will be established everywhere, or everywhere abolished" (p. x). Political science was designed to help man choose well, which demanded knowledge of the true historical situation and the capacity to face reality, avoiding the mistakes of the rival schools of alternatively exaggerating and underestimating the possibilities that were available.

Against the philosophe's excessive pride that all societies could be re-made according to the simple principles of reason, Tocqueville countered with the claim that regimes were still bound by the particular experiences of each nation. Despite the homogenizing forces set in motion by growing equality and the spread of modern thought, the political culture of each nation retained certain distinct characteristics, and Tocqueville certainly had no desire to impose uniformity for its own sake. Insofar as political science might assist those who directed society, it was not by prescribing precise models that should be adopted in the same fashion in every nation, but by illustrating general principles that should be applied in their own way as far as circumstances within each nation would permit: "The laws of the French republic can be, and in many cases should be, different from those prevailing in the United States . . . [L]et us adopt the principles rather than the details of her laws" (p. xiv).

Against the traditionalists, Tocqueville countered with a twofold argument to meet their two very different (and conflicting) assumptions. On the one hand, traditionalists virtually denied that there could be a general science of politics that could assist legislators. As the world, in the traditionalists' view, is governed by contingencies and accident, and as constitutions are the product of particular cultures, the best the legislator can do is modify a constitution according to standards that emerge from understanding a nation's particular genius. According to Tocqueville, this view underestimates the degree of choice and the role that a general science of politics might play in informing the tasks of legislating and constitution making.

On the other hand, traditionalists assumed that the movement toward equality of conditions and away from a respect for hierarchy and forms could be thwarted by the development of a counterideology. For Tocqueville, this option was closed: "Burke did not see . . . that there could be no question of putting the clock back" (OR, p. 21). Although traditionalists purported to look at facts, they refused to acknowledge that the movement toward the social state of democracy and the penetration of society by some form of rationalism were historical facts in their own right that could neither be denied nor reversed. In his In-

⁸For historicist accounts that see a movement or plan for the whole world, see Hegel's *Philosophy of History* and Marx's and Engel's *Communist Manifesto*. For the origin of historicist thought in Burke, see Strauss (1953, pp. 294-321) and Pangle (1973, pp. 192-193). Although Burke shied away from anything like a pure doctrine of historical inevitability, the latter tendency became much more pronounced in those who drew their inspiration from his thought. It is also possible to trace the seeds of Burke's emphasis on history and circumstance in Montesquieu, whom Burke considered "the greatest genius which has enlightened this age" (Chapman, 1967, p. 23). But the emergence of history as possibly the most important standard is far more evident with Burke and with those who drew directly on his thought.

roduction to *Democracy in America*, Tocqueville advanced a mild variant of universalistic historicism that challenged traditionalists to see that the real choice lay not between the old order and democracy, but between democratic despotisms and democratic republics (Zetterbaum, 1967). Given the traditionalists' expressed concern for liberty, they might, when faced with this choice, be persuaded to become adherents of the new political science.

The New Political Science

Tocqueville's new political science combines generals and particulars to form a body of knowledge designed to promote the cause of liberty in modern times. It can thus be characterized not only by its normative goal, which makes it quite different from modern political science, but by its distinctive epistemological foundation. Tocqueville's political science falls into a middle ground between rationalism, which extended general ideas to the point of eliminating any prudential consideration of specific circumstances, and traditionalism, which virtually denied the existence of any general ideas that could inform or instruct prudential judgment. But it is much easier, Tocqueville tells us, to imagine the existence and utility of such a middle ground than to put it into practice.

The central problem for a science of politics concerns the epistemological status of "general ideas." All general ideas, Tocqueville maintains, distort reality in some degree by comprehending the distinctness of different objects under the same concept. Human thought creates a world of partially fictitious wholes that assigns greater similarity to things than actually exists in the real world: "General ideas do not bear witness to the power of human intelligence but rather to its inadequacy, for there are no beings exactly alike in nature" (p. 437). It was this difficulty, among other concerns, that led Burke and other traditionalists to doubt the worth of a theoretical political science. For Tocqueville, however, the proper response was not to deny the value of general ideas, but rather to recognize their limitations and avoid using them in an extreme or abstract fashion. Without recourse to general ideas, no science, indeed no advancement in human understanding, would be possible, for man "would soon be lost in a wilderness of detail and not be able to see anything at all" (p. 437; Gargan, 1965, pp. 42-44).

Political science is the careful construction of knowledge in which general ideas are founded not on abstract principles that flow elegantly from self-contained intellectual systems, but on the painstaking study of particular cases and the

"slow, detailed, and conscientious labor of the mind" (p. 440). "One man . . . notices a fact and another conceives an idea; one man finds a means and another discovers a formula" (p. 268; cf. p. 438). Yet even this knowledge, because it relies on general ideas, is imperfect and does some violence to the understanding of particular circumstances. This difficulty creates an inevitable—although not unbridgeable—gap between the statesman and political scientist, "For while it is sometimes necessary to brush rules of logic aside in action, one cannot do so in the same way in treatises, and man finds it almost as difficult to be inconsistent in his words as he generally finds it to be consistent in his actions" (p. 20).⁹ Tocqueville prepares his readers for this problem by warning them that he must sometimes bring his own ideas "to the verge of the false and impracticable," although he never consciously "fitted the facts to opinions instead of subjecting opinions to the facts" (pp. 20, 19).

A perfectly accurate understanding of reality, Tocqueville tells us, cannot be attained, for it would require a humanly impossible method that could somehow reap all the benefits of generalizing while avoiding all of its defects. Only God can employ such a method, for He alone "can survey distinctly and simultaneously, all mankind and each single man" (p. 704; cf. p. 437). Science does the best it can using the limited tools of human intelligence to emulate this God-like standard.

The thought of philosophe intellectuals, by contrast, ignores completely this God-like standard by attempting to subsume more and more of "reality" into abstract general ideas without regard to the existence of particulars. Instead of minimizing the defect inherent in the structure of human thought, the philosophe intellectual exacerbates it. If all that were involved in this process was the activity of some isolated individual interpreting the world, the danger would be slight. But in fact philosophe thought was designed expressly to create a popular political program, the aim of which was to eliminate complex customs and traditions and reshape reality in accord with the "rules of logic and a preconceived system" (OR, p. 147). In line with this program, the rationalists of Tocqueville's day tended to support the powers of the largest unit of authority, the central government, no matter whether it was a strong, "enlightened monarchy or a centralized republic" (pp. 97, 723-724). Philosophe intellectuals saw the central government with its centrally directed administrative apparatus as one of the chief instruments for "recasting [a nation] in a

⁹I have changed Lawrence's translation in this passage.

given mold, of shaping the mentality of the population as a whole and instilling the ideas and sentiments [of the intellectuals] into the minds of all" (*OR*, p. 162).

Even more important for Tocqueville than the philosophe's program for change was the consequence of its framework of thought on the mental habits of society. By its inattentiveness to particulars, philosophe thought promoted a disregard for diversity and a partiality for uniformity. By contrast, the new political science, in its concern for facts and practice, implicitly encouraged a respect for diversity. Thus, apart even from a consideration of the content of their programs, the new political science embraced a mode of thought more favorable than philosophe rationalism to a political culture that supported liberty. In his most general and revealing statement on the consequences of rationalist general ideas, Tocqueville wrote:

To force all men to march in step toward the same goal—that is a human idea. To encourage endless variety of actions but to bring them about so that in a thousand different ways all tend toward the fulfillment of one great design—that is a God-given idea. The human idea of unity is almost always sterile, but that of God is immensely fruitful. Men think they prove their greatness by simplifying the means. God's object is simple but His means infinitely various (pp. 734-735).

Rationalist intellectuals were either ignorant of the distortion of reality in their thought or else chose from ambition to disregard it. Yet far from reducing their influence on society, this defect tended to increase it, for "what is merit in the writer may well be a vice in the statesman, and the same things that often make beautiful books can lead to great revolutions" (*OR*, p. 147; cf. *REC*, pp. 84-85). Here Tocqueville suggested the central problem of the intellectual in politics: theoretical constructs that are a false reflection of reality are apt to have a much greater impact on the public mind than those that present a true picture of reality. The products of a "literary spirit" result in "beautiful" works, whereas the products of a scientific spirit must sacrifice beauty for accuracy.

Traditionalists had correctly identified this homogenizing tendency of modern philosophe thought as its most characteristic and dangerous trait. But while sharing their concern, Tocqueville rejected their standards and doubted the efficacy of their methods for combatting philosophe ideas. Unlike the traditionalists, Tocqueville upheld reason at the same time that he attacked the reason of his contemporaries and defended the intellect at the same time that he attacked the modern intellectual.

The underpinnings of Tocqueville's new political science were not entirely original. Concern about the consequences of philosophe thought and about the need to blend the universal and the particular can be found in the works of the writers whom Tocqueville studied most closely (Rousseau, Montesquieu, and Pascal). Yet the fact that Tocqueville owed a debt to other thinkers, and especially to Montesquieu, does not reduce the significance of his call for a new political science, for on Montesquieu's own grounds political philosophy must be "presented not only in the form of a philosophic treatise but simultaneously in the form of a tract for the times" (Pangle, 1973, p. 305). Just as the principles of political science must be adapted to each country, so the principles of political philosophy must be adapted to each new age (p. 543). In the brief period between Montesquieu's death and Tocqueville's publication of *Democracy in America*, modernity had undergone one of its greatest transformations. Tocqueville's claim to originality (if his words can be read in those terms) was not for a new political science per se, but for a "new political science . . . for a world itself quite new." Tocqueville's achievement lay, as Mill had said, in writing the "first philosophic book . . . on Democracy."

Political Science as an Active Force in the World

Given the complicated character of his new political science, one immediately wonders how Tocqueville hoped to make it into an active force in modern society. Although the goal of political science—reconciling liberty and democracy—was not hostile to the general spirit of modern times, its complexity left it without widespread appeal. It could, of course, be taught directly to the few who studied his works, but Tocqueville was under no illusions that it could become a popular ideology that could, like philosophe thought, "fire the imagination of the masses." "Generally speaking, simple conceptions take hold of people's minds. A false but clear and precise idea always has more power in the world than one which is true but complex" (p. 164).

Since political science could not win a public war of ideas against philosophe ideology, Tocqueville employed a more indirect strategy based on encouraging certain mental habits in society that would be compatible with, if not necessarily exactly the same as, the lessons taught by political science (p. 287). These mental habits were characterized less by any particular content (in the sense of a set of ideas outlining a program or agenda) than by a way of thinking that people

would employ. Tocqueville did not seek to fight ideology with ideology, not only because such a battle would have been futile, but also because a society directed by ideology (and intellectuals) could not, in his view, remain a free regime.

To be more specific, Tocqueville's strategy involved showing those who direct society what kinds of social and political institutions could limit the intellectuals' influence over public opinion and help citizens to learn to reason from particulars to generals rather than from generals to particulars. Under Tocqueville's strategy, public opinion would form more along the lines of the American than the French model. The influence of ideas produced by intellectuals at the "top" would diminish, whereas that of other sources of authority (e.g., political leaders and lawyers) and of opinion generated from the "bottom" would increase. Opinion generated from the bottom was a reflection of democratic sentiments and concrete interests as these were modified by the knowledge and experience gained by citizens involved in the task of self-government.

In this section, I shall examine Tocqueville's application of the practical aspects of the new political science to the press, the legal system, and local government, noting how he employs his distinctive standard of judging institutions by their effect on mental habits. In each case, Tocqueville illustrates how American institutions, especially when compared to those in France, serve to reduce the influence of general ideas and to promote reasoning from particulars to generals. Tocqueville's method of analysis here is slightly unexpected given his starting point of studying America in order to see the picture of democracy. Yet American political culture, as noted, does not follow democracy's natural path of development. In the instances discussed below, France becomes the picture for certain democratic tendencies in their pure (and dangerous) form, whereas America becomes the (partial) model for how these tendencies can be corrected in a framework that is consistent with democracy.

The Press¹⁰

Tocqueville's discussion of the press is the most striking case of his effort to limit the influence of modern intellectuals. In line with the position of most *philosophe* thinkers, Tocqueville was a staunch advocate of a free press, arguing that

citizens in free regimes need a free flow of information to govern themselves and that the possession by government of the power to regulate the press could lead by stages to despotism. Yet unlike the *philosophe* thinkers, who saw the press as a perfect vehicle for spreading their version of enlightenment to the public, Tocqueville preferred an arrangement of the press that would limit the intellectual's influence.

As an independent source of power over public opinion, the French press had no equal. The "vital part" of its newspapers was devoted to political discussion, and the "spirit" of French journalism demanded a "lofty and eloquent way of arguing about the great interests of state" (pp. 183, 185). French journalists were highly esteemed, as the relatively small number of newspapers were concentrated in Paris and attracted talented and cosmopolitan writers. It was with the aid of the press that intellectuals were able to spread their thought from the center to the peripheries and to enlighten the provinces with the latest and most advanced ideas.

The American press could hardly have been more different. Newspapers were filled with advertisements, and the presentation of the news lacked the kind of "burning argument" and lofty commentary that was commonplace in France (p. 184). There was an extraordinarily large number of newspapers in America, which were widely dispersed throughout the nation. The effect of this arrangement was to divide and limit the American press's influence, in comparison with that of the French press. In addition, American journalists were generally of low educational status and possessed only modest talents. When they expressed political viewpoints, they did so not by adopting an intellectual perspective and hiding their appeals to passions in philosophic language, but instead by making crude appeals to lower passions, often "attacking people [and] revealing their private lives and vices" (p. 185). Although American journalists won a wide audience by these techniques, the public, and certainly its more enlightened elements, sensed the narrow spirit of their inquiry and had little respect for journalists as a group.

Even though the press in America was less powerful than in France, its influence was still very great: "[a]fter the people, the press is nonetheless the first of powers" (p. 186). Yet it was not journalists as opinion makers who held most of this power; rather, it derived from the newspapers' role in communicating ideas among groups in society, such as political parties and associations. The American press therefore contributed to the formation of public opinion by political associations rather than by intellectuals, allowing opinion to build from the bottom to the

¹⁰In this section, I borrow freely from the excellent discussion of Tocqueville's thought by Bourricaud (1980, pp. 37-69).

top, rather than from the top to the bottom (p. 520).

By any criterion of literary or journalistic excellence the American press was inferior to that in France, and Tocqueville never doubted that the "deplorable abuse of thought" that took place in the American press would adversely affect "the taste and morality" of the American people (p. 185). But the harm done to American mental habits in this respect was more than offset by the benefit derived from blocking a portal of access for the intellectuals' general ideas. Tocqueville was quite willing to sacrifice sophistication and even to admit a degree of coarseness into society as the price for protecting citizens from the thinkers who sought to run their lives.

The Legal Profession

Tocqueville's discussion of the legal profession provides a second example of his effort to limit the power of intellectuals and general ideas (p. 267; cf. p. 185). Tocqueville sought to promote the legal spirit in society to serve as a counterweight to the literary spirit, and lawyers as a counterweight to intellectuals.¹¹ The legal way of thinking, with its roots in the judicial process, has something in common with the empirical strain of early rationalist thought: "[I]t pronounces on particular cases and not on general principles" (p. 160). Judges and lawyers are trained to reason from particulars to generals and therefore develop an affinity for order and forms that leads them as a rule to shun momentary popular passions. To the extent that the legal profession influences the thinking of society as a whole, it imbues the citizenry with some of their mental habits. Tocqueville, for example, considered the importance of the institution of the jury as much in terms of its impact on mental habits as on its efficiency as an instrument of justice.

After sketching the general spirit of the legal profession, Tocqueville turned to the particular spirit of French and Anglo-Saxon lawyers. Although lawyers in both nations reason from particulars to generals, French lawyers, following continental theories of jurisprudence, analyze particulars in the light of general principles, whereas Anglo-Saxon lawyers, working in the

common law tradition, consult "the law of precedents" and consider particulars in light of "the legal judgments of their fathers." "The first thing an English or American lawyer looks for is what has been done, whereas a French one inquires what one should wish to do." The spirit of French law is more abstract, as it is based on a "whole system of ideas," whereas that of English and American law is more historical (p. 267).

The spirit on which Anglo-Saxon law rested antedated not only philosophic thought, but the emergence of rationalism. It incorporated many characteristics of traditionalism, and Tocqueville's discussion of English and American law virtually reads as a commentary on traditionalist thought. English and American lawyers proceed with a "superstitious respect for all that is old" (p. 269) and value laws "not because they are good but because they are old" (p. 268). With their expertise resting on an "obscure" tradition, they relate to the public "somewhat like the Egyptian priests, being, as they were, the only interpreter of an occult science" (p. 268). English lawyers will introduce change, but only by hiding the fact and going to "absurd lengths" to avoid admitting to the "crime" of being an "innovator" (p. 268). The character of English law is decidedly organic—a point Tocqueville emphasizes by employing the metaphor of a "trunk of an old tree" on which lawyers continually graft "the strangest shoots" (p. 268).

Although Tocqueville appreciated much in the traditionalist character of Anglo-Saxon law, his ironic tone and admission that this system of legal thought was filled with "defects" leave no doubt that he considered traditionalism to be unsuitable as the first principle for the entire government (p. 268). In any case, its merits aside, the Anglo-Saxon legal spirit could not serve openly as the foundation for a modern society, as its premises were out of step with rationalist standards of legitimacy. Anglo-Saxon lawyers could "raise no banner of their own" and won a measure of influence not by openly defending their principles, but by insinuating their spirit into society, "penetrating each component class and constantly working in secret upon its unconscious patient" (p. 270). In spite of these reservations, however, Tocqueville thought that the Anglo-Saxon legal spirit provided a salutary corrective to the excesses of certain democratic tendencies, moderating the citizenry's fascination with novelty and limiting its susceptibility to the appeal of the intellectual's general ideas. The legal spirit in the United States served in its context to promote the ends of the new political science, even though it was not grounded strictly on the mental habits of early rationalist thought (p. 268).

¹¹Tocqueville provides a rough definition of spirit (*l'esprit*) as, in any particular case, "certain ways to which all must conform . . . the sum of these common ways is called a spirit; there is the spirit of the bar, the spirit of the court" (p. 185). We are obviously also meant to think of Montesquieu's usage of the term.

Communal Government¹²

Tocqueville's discussion of communal liberty and citizen participation in local governments is the best known example of his application of the principles of the new political science to practical affairs. It is important, however, to avoid confusing his ideas on citizen participation with an endorsement of populism in national politics (p. 253). Tocqueville was a staunch opponent of direct democracy in national politics and a strong defender of the American founders' goal of establishing representative institutions that preserved broad discretion for deliberation and statesmanship. His idea of participation emphasized activities in matters falling within the sphere of the citizens' own experience, not in affairs of state such as foreign policymaking. The importance of participation was based as much on the mental habits promoted by the activity of deciding as on the content of what people decided (pp. 511, 694).

By taking part in political affairs, citizens learn the mental habits of the pragmatic strain of rationalism. These habits serve to reduce the influence of general ideas and increase the weight of experience in the formation of public opinion: "If, then, there is a subject concerning which a democracy is particularly liable to commit itself blindly and extravagantly to general ideas, the best possible corrective is to make citizens pay daily practical attention to it" (p. 442). For citizens to become enlightened in more than a cosmetic sense depends, in fact, on their being able to complete their literary education by participating in the practical task of governing: "True enlightenment is in the main born of experience, and if the Americans had not gradually grown accustomed to rule themselves, their literary attainments would not help them much toward success" (p. 304).

Local government also functions for Tocqueville as a major "secondary power" that stands between the individual and the central state (p. 676). Tocqueville seldom discussed liberty without also discussing the practical means by which it could be secured, and these included the possession by citizens of a sense of their *power* to defend their rights. Secondary powers in Tocque-

ville's thought serve as rallying points for citizens to resist the overwhelming weight of central authority. Yet secondary powers are threatened in democratic times by the many forces tending towards concentration of authority in central governments, and it is only by great effort and intelligence that they can be maintained: "In the dawning centuries of democracy individual liberty and local liberties will always be products of art; centralized government will be the natural thing" (p. 674).

Of the secondary powers Tocqueville identified—civic associations, political associations, and business enterprises—he considered the commune to be the most durable, for it alone rested on a "natural" foundation (although it too could only be maintained by "art"). The commune is "natural" in the sense of being a form of social organization that grows organically rather than being consciously constructed: "[It] is the only association so well rooted in nature that wherever men assemble it forms itself. . . . [M]an creates kingdoms and republics, but communes seem to spring directly from the hand of God" (p. 62). Tocqueville introduces here a key distinction between the "natural" (in the sense of the organic) and the conventional (that which man makes by the application of his own intelligence). Tocqueville's final standard, it should be observed, is not the natural in the organic sense, but rather "nature" or "natural right" as discovered conventionally by human reflection on the world and its possibilities (p. 237). In contrast, however, to the fabricated "human ideas" propagated by philosophe intellectuals, Tocqueville's understanding of natural right never departs entirely from the natural in the organic sense, in part because the failure to recognize organic needs leads to social dysfunctions. A satisfactory modern regime must combine a man-made principle of rule with respect for the natural foundations of community that are found in the communes.

The conflict Tocqueville identified between the standards of philosophe rationalists and the standard of the natural (organic) is played out on the plane of practical politics in the conflict over the role of local governments in the modern state. The commune develops outside the sphere of human contrivance and derives much of its operating spirit not from abstract theory or ideology, but from custom and practical experience. Philosophe intellectuals, who rely for their power on units of central opinion formation and their alliance with central authority and centralized administration, regard communal liberty as an enemy of their designs. They want to impose their standards on the commune and thus run roughshod over communal custom and local ex-

¹²Lawrence sometimes translates *commune* as township, where Tocqueville was suggesting a connection between the American local community and local communities found everywhere. When referring specifically to the American local unit of government, Tocqueville often included the English word "township" in parentheses. I have substituted "commune" for "township" in some of the translations in this section.

perience: "A very civilized society finds it hard to tolerate attempts at freedom in a local community; it is disgusted by its numerous blunders and is apt to despair of success before the experiment is finished" (p. 62). Here Tocqueville depicts the characteristic vice of modern intellect, which is an absence of forbearance and an impatient impulse to do things for people rather than to let them act for themselves. Philosophes rationalists prefer to impose the right opinion in every case rather than inculcate the mental habits that would allow people to decide for themselves (and therefore inevitably to make mistakes).

The question of where to draw the line between central and communal authority was for Tocqueville a complicated matter that depended on such considerations as security needs, the stage of economic development, and the degree of enlightenment in the communes. Although not a dogmatic opponent of all "intrusions" of the central authority in local affairs, Tocqueville nonetheless expressed a strong preference, all other things being equal, for decentralization.¹³ In the northern states of the United States, Tocqueville found his practical ideal of the modern commune. Free of feudal customs, these localities had developed a new tradition based on equality and the exercise of pragmatic rationalism in politics. These communes were enlightened, but they were still far from being the centers of sophistication adored by philosophes intellectuals. Tocqueville had no hesitation, however, in defending them, as his goal was not a society directed by the most advanced discoveries of modern thinkers, but one that allowed free and enlightened citizens to practice the art of self-government.

Tocqueville's comparison of opinion formation in France and America risks slipping at times into an ideal-type analysis in which the United States, in order to be contrasted with a nation influenced by philosophes rationalist thought, is presented as a society devoid of intellectual reflection and successful only because of the unconscious operation

of certain geographical and historical advantages. In fact, while Tocqueville pointed to many of the fortunate circumstances that contributed to America's success, his analysis makes clear that the establishment of the Constitution in the 1780s was a remarkable achievement of political sagacity. America was adverse to "general ideas," if by general ideas one means the sort of ideas generated by the philosophes and their opponents. But the American founders were certainly not adverse to the study of the first principles of politics, and important elements of the American public grasped the principles of science on which they had based their work (pp. 113-114, 152, 164).

Although Tocqueville had nothing but praise for the intelligence of America's founders, it is nevertheless significant that he did not consider any American to have been a "great writer" on politics (p. 301). The understanding of what made America work was only partly contained in the thought of *The Federalist*, which, because it concentrated on the principles for establishing a national government, provided an incomplete account of state and local governments and the fundamentals of American political culture (p. 61). Tocqueville's analysis in effect combined the concerns of the Federalist and the small republic or anti-Federalist traditions in American political thought by blending certain attributes of the small republic—or at any rate worthy facsimiles thereof—with the qualities needed to sustain a great, modern commercial republic (p. 287). The result, he evidently thought, was a more complete political science than that of any American thinker.

The Synthesis of Rationalism and Traditionalism

To promote liberty in modern times, Tocqueville sought to reduce the influence of intellectuals and general ideas and to instill in citizens the mental habits of the early rationalist method of reasoning from experience and particulars. Although Tocqueville was as critical as Burke of the modern philosophes strand of rationalism, he nevertheless made rationalism, not traditionalism, his principal standard for modern society. He rejected traditionalist notions of prejudice, superstition, and emotion as the foundation for government on the grounds that they were inconsistent with the spirit of modern times and incapable of supporting a regime of equality in liberty.

Tocqueville's project can therefore be described as an effort to rescue the original core of rationalism from its later distortion. Yet this statement needs to be qualified, for Tocqueville thought that rationalist standards, even those derived from the early inductive approach, were not entirely adequate as a foundation for a free

¹³Tocqueville acknowledged the need for greater centralization in Europe than the United States because of the necessity of reforming its feudal mores (p. 90). In the United States, he suggested that the federal government would have a similar role to play with respect to eliminating prejudice against Indians and Blacks (p. 337). In future times, economic conditions would call for a very active role for the federal government as protector and regulator (p. 684). Finally, although Tocqueville clearly preferred as a general rule to strengthen decentralizing trends against the tendencies of democracy toward constant centralization, in the United States he felt that the drift toward decentralization was too strong and threatened the nation's unity and security (pp. 390-393).

1200

and healthy society. Moreover, the distortion of early rationalist thought that took place in the eighteenth and early nineteenth centuries was not, in Tocqueville's estimation, a mere accident, but a logical result of certain flaws and limitations implicit in the original rationalist position. Thus, even to save the original core of rationalism, Tocqueville argued that it had to be supplemented and modified. These changes were based on insights Tocqueville drew from traditionalist principles, although he sought to apply these insights in ways that were compatible with the spirit of rationalism.

The originality of Tocqueville's new political science inhered largely in its synthesis of these two schools. Although predominantly a rationalist and a liberal, Tocqueville sought to integrate certain traditionalist values such as religiosity, community, and spiritedness into the democratic liberal state. These values Tocqueville considered worthy in themselves, but he also stressed their utility. The failure of liberal rationalist regimes to incorporate these values would lead to severe dysfunctions that in the end would threaten their prosperity and freedom. The dual nature of Tocqueville's analysis, at once friendly to liberalism but mindful of its problems, helps to explain his powerful influence on both liberal thinkers and those of the European sociological tradition who have stressed the major difficulties of liberal society.¹⁴

Tocqueville discussed the central problem posed by the entire rationalist project in his extensive treatment of the nature of intellectual authority in modern times (pp. 429-500). The original rationalist position as it filtered into society held that each person could make up his own mind, using his personal assessment of reality as the standard of truth. But this position, Tocqueville argued, was untenable, for most human beings lack the time, inclination, and capacity to decide on a great many matters of fundamental importance: "Man has to accept as certain a whole heap of facts and opinions which he has neither leisure nor power to examine and verify for himself . . . [s]omewhere and somehow authority is always bound to play a part in intellectual and moral life" (p. 434).

Aristocratic societies were arranged implicitly in acknowledgment of this fact, and people experienced no great problem in receiving opinions from tradition or from those considered wiser. In democratic societies, however, individuals are imbued with the rationalist idea that they can make

up their own minds, and, considering themselves fully equal with everyone else, refuse to accept the intellectual authority of any other individual. Nevertheless, sensing their own weakness and needing to resolve matters, they turn to public opinion as the source for their ideas (p. 435). By bowing to no one in particular, they flatter themselves that they have not sacrificed their own judgment. With their minds thus at ease, they accept a source of intellectual authority that is potentially even more powerful than those found in aristocratic societies. Rationalism, which begins from the supposition that each person can make up his own mind, may paradoxically end with a state of affairs in which few think for themselves and in which "public opinion becomes more and more mistress of the world" (p. 435).

The central question in Tocqueville's analysis of democratic society thus becomes who or what forms public opinion. Tocqueville provides two basic answers: first, public opinion may emerge from the interests and sentiments of the people; second, public opinion may be formed by intellectuals. There are differences in the character of opinion as it derives from these two sources, though not as great as one might suppose. The opinion that emerges from the people is coarser and reflects, among other things, untutored desires for material gratification and for some kind of equality (pp. 552, 504); the opinion formed by intellectuals fans the passions for equality and uniformity, dressing them in high-brow philosophic language and endowing them with utopian expectations (*OR*, pp. 158-159). Accordingly, both sources produce opinions that are democratic in content; but the opinion that derives from intellectuals is rigid and dogmatic, whereas that which derives from sentiment is more amenable to modification by political experience and to direction by political leaders (*OR*, p. 142).

To explain why the opinions fostered by intellectuals are democratic, Tocqueville presents a three-part sociology of ideas and intellectuals. First, he argues that only those ideas that are broadly consistent with prevailing social conditions can attract public support (p. 431). Social conditions do not determine the exact content of thought or the ideas of every thinker. But social conditions do suggest certain ways of thinking about reality, which most thinkers then ascribe—often erroneously—to all times and places. For example, given the weakness of individuals in democratic times, most thinkers are led to posit that history is moved by general causes rather than individual efforts (pp. 439, 494-496). More important, social conditions set boundaries to the kinds of ideas that are potentially acceptable. Thinkers who stand outside these boundaries—

¹⁴Included on this list would be Emile Durkheim and, in the United States today, Robert Nisbet and Peter Berger.

like traditionalists—are unable in the end to exercise much influence. Thus, although intellectuals may shape the public mind, they can do so only so far.

Second, intellectuals in the modern era are perhaps especially prone to reflecting the common views of their times. Those who become intellectuals are not members of a distinct class having a set of values fundamentally different from the rest of society, but rather emerge from society after having been initially formed by democratic tastes and passions. They employ their thought to give expression to these sentiments and are often little more than the most articulate conveyors of democratic prejudices (pp. 457, 460).

Finally, Tocqueville suggests a narrower and more self-interested reason for the intellectual's adherence to democratic ideas. Like all others in democratic times, intellectuals must earn a living and make their way in the world by practicing a career; they engage in their trade as a vocation, not an avocation (p. 458). As a result, their success depends on how well they can sell their ideas to their contemporaries. The intellectual *qua* intellectual is not accorded deference, but rather can gain influence only by his ability to appeal to a portion of the public and supply it with the kinds of ideas it can grasp. The intellectual is therefore led by ambition and the desire to secure a livelihood to produce ideas that can meet with widespread approval. In content, this means purveying ideas that are consistent with the democratic passions, chiefly equality and uniformity; in style, it means writing in broad generalities, because democratic citizens, needing "sound" opinion on many matters but unwilling or unable to spend time on difficult inquiries, crave general ideas (p. 442).

Although the opinion generated by intellectuals tends to be democratic, it differs, as noted, from the opinion that emerges directly from popular democratic sentiments. Intellectuals create opinions that appeal to the imagination and that move people beyond a common sense calculation of their interest to imagine utopian visions; and even though intellectuals must sell their ideas, they are not (with the exception of the masters of popular entertainment and culture) bound by the immediate views of the mass public. They produce for a more limited high-brow or literary-minded public that is more likely to get excited by their new thoughts (pp. 461, 473). The intellectual's influence over the public mind is exercised by a two-step process in which potentially quite radical ideas are first digested by a literary-minded sector and then by degrees disseminated to the public at large.

If this analysis helps explain the intellectuals' ability to manage opinion in modern society, the

problem for Tocqueville was how their influence could be limited by mental habits reflecting the empirical strand of original rationalist thought. For this, Tocqueville thought it better to work with opinion as it emerged naturally from democratic passions and sentiments, educating and moderating it by means of the solutions noted in the last section. Yet for these solutions to work, Tocqueville made it clear that steps would also have to be taken to deal with certain fundamental moral and intellectual problems that rationalism, even in its empirical form, left unresolved. It was here that Tocqueville turned to traditionalist insights.

Tocqueville's most important qualification of a rationalist foundation for society was his assertion of the need for religious belief. Tocqueville stands out as one of the staunchest supporters of religion among the great liberal thinkers, and it is on this point that his break with rationalist intellectuals of his time was most explicit (p. 295; *OR*, pp. 148-157). In matters of belief about religion, as on the question of the rights of man, Tocqueville made an exception to his position against general ideas, or rather took the occasion to clarify his position about the domains of opinion in which general ideas were needed: "General ideas respecting God and human nature are . . . the ideas above all others which ought to be withdrawn from the habitual action of human judgment" (p. 443). If the scope of critical inquiry in these areas were not limited, people would be thrust into an intellectual vacuum leading to despair, paralysis, and eventually susceptibility to false and dangerous pseudo-religions.

By contrast, the philosophes held that people could live without a religious dimension in their lives. But this view, according to Tocqueville, was both false and detrimental to society. People possess a "taste for the infinite," which, if repressed in one form, must surface in another (p. 535). Two results follow from the attempt to destroy religious beliefs. In one case, most people abandon religious faith and immerse themselves in a consuming pursuit of material gratification. But within this secularized society, people experience a gnawing feeling of emptiness, causing some to turn to fanatical religious sects or to mysticism (p. 535). In the other case, people displace their religious sentiments onto politics, thus creating a religion of revolution. This had already occurred in France, where the French Revolution "though ostensibly political in origin . . . assumed many of the aspects of a religious revolution" (*OR*, p. 11). Tocqueville here identified the mindset of the modern intellectual revolutionary—a revolutionary of a "hitherto unknown breed"—who saw no meaning to life outside the political realm, but who, because of this

denial, infused politics with all of his repressed spiritual feelings (*OR*, p. 157).

In contrast to rationalism, Tocqueville's new political science recognized the spiritual needs of man as a fact of human existence which a healthy social order must take into account and channel in a constructive way. Tocqueville based his view in part on the traditionalist view regarding man's incapacity to support intellectual doubt, but he did not extend this principle to a reliance on "prejudice" in political affairs. His model for the intellectual foundation for modern society was taken from the United States, where citizens managed to join a belief in God with a pragmatic rationalist approach to truth in other spheres. Although resting on different foundations, religion and rationalism were joined together in America into an intellectual whole, with religion not only coexisting with rationalism, but actually enhancing its role in practical affairs. The limitation of rationalist inquiry on a few metaphysical questions provided spiritual comfort and solace, enabling people to exercise rationalism more effectively in the domain of politics (p. 444, *OR*, p. 153). Hence Tocqueville's famous claim: "I am led to think that if [man] has no faith he must obey, and if he is free he must believe" (p. 444).

In addition to neglecting man's spiritual needs, rationalism in Tocqueville's view promoted the vice of a radical and asocial individualism. According to rationalist tenets, each individual separately judges truth on the basis of his own reason and experience: "[E]ach man is narrowly shut up in himself, and from this basis makes the pretension to judge the world" (p. 430). This leads to a malady that Tocqueville called "individualism," but which, given the modern connotation of that term, might better be called privatism. Privatism has the effect of making individuals ignore all political and social concerns and withdraw into a life preoccupied with the well-being of self and family. It helps pave the way for despotism, which "sees the isolation of man as the best guarantee of its own permanence" (p. 509).

Although Tocqueville argued that people possessed a natural yearning for community, he did not think it was sufficient to withstand the disintegrating impact of rationalist intellectual doctrines and modern social conditions. To integrate man into the community required a mixture of rationalist and traditionalist principles. Unlike traditionalists, Tocqueville thought that the fundamental bond between the individual and community in modern times had to rely on a calculation of interest, not on feelings of patriotism (p. 236). To a certain extent, rationalism contained the remedy of its own defect, for citizens could learn from participating in

politics that their private interest would be promoted in the long run by furthering the public interest. As a result of the mental habits formed by this principle of "self-interest rightly understood," citizens would develop a concern for the community as such (pp. 510-511).

Yet the integration of the individual into the community, which is essential to fulfilling man's yearning for a social existence, could not be accomplished without also relying on the effective attachment of individuals to their communities as a supplementary bond: "Every citizen of the United States may be said to transfer the concern inspired in him by his little republic to love of the common motherland" (p. 162; cf. p. 94). Tocqueville's view of the political community thus reflected a synthesis of rationalism and traditionalism. A modern state is not, as certain traditionalists would have it, a mere enlargement of an organic unit. Neither, however, is social organization entirely a human convention created by asocial individuals who contract to form a community, as so many rationalists posited. The two conceptions need to be combined into a view of the state that recognizes individual rights but that also accords a great deal of independence to local governments.

The final difficulty with rationalism, in Tocqueville's view, lay in its inability to account for the deepest source of man's attachment to liberty. Rationalism can help people see their interest in liberty and, through the principle of self-interest rightly understood, lead them to appreciate its utility for producing such goods as wealth and power. Ultimately, however, this utilitarian standard is inadequate. Liberty can be maintained in the final analysis only if people value it as an end worthwhile in itself. This kind of commitment to liberty inheres in the "will" rather than any calculation of self-interest (p. 705). As human "will" falls into the category of a moral sentiment of the heart, it must be evoked by a rhetoric that goes beyond interest and that appeals to traditionalist notions of pride and spiritedness. Tocqueville accordingly urges those who direct democratic society to exhort citizens to have a "higher idea of themselves and of humanity" (p. 632):

What has made so many men, since untold ages, stake their all on liberty is its intrinsic glamour, a fascination it has in itself, apart from all "practical" considerations. . . . The man who asks of freedom anything other than itself is born to be slave. [The love of liberty] is something one must *feel* and logic has no part in it. It is a privilege of noble minds which God has fitted to receive it, and it inspires them with a generous fervor. But to meaner souls, untouched by the sacred flame, it may well seem incomprehensible. (*OR*, p. 169)

Conclusion

In analyzing the thought of any major political theorist, it is necessary to begin by making an effort to understand the world in the same terms as that theorist. Yet in Tocqueville's case, to stop at that point would fail to fulfill the spirit of his enterprise. Tocqueville's political science calls on others to continue the project of attempting to reconcile liberty and democracy by engaging in concrete analyses of the specific social conditions and institutions in each era and each regime: "Different times make different demands; the goal alone is fixed . . . the means of getting there ever change" (p. 543).

Here, of course, is not the place to pursue such inquiries, but it is clear that America has experienced fundamental changes since Tocqueville's time in some of the elements shaping its political culture. America's intellectual elite, once shut off from the latest ideas and fashions, now often sets the trend for Western intellectual development. Although American political culture probably continues to remain more resistant to the immediate influence of deductive intellectual thought and "general ideas" than the political cultures of certain European nations, recent studies of American society—especially the literature on the emergence of the so-called "new class"—point to a growing influence of America's intelligentsia in shaping the way people think (Bell, 1973; Bruce-Briggs, 1979).

This increased influence of the intellectual stratum in America has been accompanied by a weakening of the specific counterweights to the rapid transference of "general ideas" throughout society. In comparison to Tocqueville's time, the dominant jurisprudential theories are less traditionbound and more open to influence by contemporary intellectual ideas. The news media have become more centralized, and major journalists, who now enjoy an extraordinarily high status, have developed close ties to intellectual centers. Finally, the political system, at least until quite recently, has become more centralized, with many matters previously considered local now decided at the level of the federal government. America can no longer rely, as it could in Tocqueville's time, on its insularity or the peculiar character of its institutions to escape the consequences of the propagation of intellectual ideas in society. Like the European nations he addressed in the nineteenth century, contemporary America requires, by Tocqueville's standards, a science of politics to combat the potential hazards of the easy ideas produced by literary writers about politics.

Over the past two decades students of American politics have re-discovered the concept of political culture, using it to investigate areas of

society that for a time had been considered outside the normal boundaries of political science. Despite many recent advances in the techniques for studying social phenomena, modern researchers may still find that Tocqueville's approach offers some important insights. Whether or not one accepts his view about the relationship between basic "mental habits" and political values, it at least provides criteria for determining what is worthwhile and thus suggests a way of avoiding the sorts of inquiries that catalog every societal factor imagined to have a bearing on political beliefs. Moreover, far from merely making ritualistic claims about the centrality of political culture, Tocqueville shows how one can keep the concept continually in mind to analyze and evaluate institutions and public policies. Finally, Tocqueville's analysis of the connection between political thought and political culture—and between changes in the one and transformations in the other—points to the need to transcend the artificial modern division between political theory and empirical political science. If one follows Tocqueville's approach, political culture must be understood ultimately by reference to the philosophic ideas that form the mental structures which govern how people see the world and formulate their opinions. The task of maintaining a political culture of liberty leads to a consideration of the philosophic ideas capable of being embodied in the mental habits of citizens which can provide people with the means and the will to maintain their freedom.

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Assessment of Political Power in the Israeli Knesset

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Immediately after the election to the tenth Israeli parliament (Knesset), 21 students of political science, 24 Knesset members, and seven parliamentary correspondents were each asked (a) to assess the political power ratios of the 10 parties represented in the Knesset and (b) to judge the ideological similarity between them. As ascertained by Saaty's analytic hierarchy scaling technique, the power ratio judgments proved sufficiently consistent to justify the construction of individual ratio scales of perceived political power. The ideological proximities were adequately represented by two-dimensional ideological spaces. Analyses of the derived power measures showed that the higher the political sophistication of the subject, the higher the combined power attributed to the religious parties and the lower the combined power assigned to the two largest parties Likud and Labor. The derived power measures were then compared to the predictions of six power indices, three of which only consider the ideological space. Of the six models, the generalized Banzhaf power index best accounted for the perceived power of 62% of the subjects, whereas the classical Shapley-Shubik index provided the best fit for 31% of the subjects. The generalized power indices were found only partly satisfactory with a need for further revision.

There is general agreement among social scientists that the study of power is central to the understanding of conflicts and their resolution. This agreement encompasses conflict situations ranging from dyadic interactions to the formation and dissolution of conflicts in large weighted voting bodies. Thibaut and Kelley (1959) placed power in the center of their psychological investigation of the intricate and often subtle bargaining behavior in mixed-motive dyadic conflicts. Examining large social bodies, Parsons (1963) suggested that power is one of the key concepts in the Western tradition of thought about political phenomena, and Lasswell and Kaplan stated more than 30 years ago that "the concept of power is perhaps the most fundamental in the whole political science; the political process is the shaping, distribution, and exercise of power" (1950, p. 75).

Yet despite this general consensus, there have been no attempts to study how people actually assess power in political voting bodies. Because the perception of political power in parliamentary democracies and in other political voting bodies may affect bargaining and influence policy decisions, the outcomes of such an empirical study, besides shedding light on an important aspect of human behavior, are potentially useful for answering questions about the formation of coalitions and the allocation of cabinet ministries among coalition parties.

In discussing the conceptual intractability of the concept of power, Parsons observed that "there is, on analytical levels, a notable lack of agreement both about its specific definition, and about many features of the conceptual context in which it should be placed" (1963, p. 235). Increasingly, Riker's (1962) cutting of the gordian knot of the notion of power, which replaces it by the more concrete notion of winning, has come to be adopted in the special and restricted domain of the distribution of power in weighted voting bodies. In correspondence with Riker's view, political scientists and other researchers have developed numerical indices of power for social systems that can be adequately modeled as simple games (Shapley, 1962), or cooperative n -person enterprises in which "winning" is the only goal and a specification of which coalitions are capable of doing so is the only structure.

Although numerical power indices have gained much popularity, our contention is that in their present form they are too limited for modeling

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perceived voting power. Theoretical developments in the related and more familiar area of coalition formation, which started about a dozen years ago, may prove useful for descriptive purposes and point the way for constructing more adequate models. As is well known, Riker's seminal work on the size principle (1962) inspired the construction of models that proposed alternative criteria for the formation of coalitions in weighted majority games such as the formation of minimum winning coalitions (Riker, 1962), coalitions of minimum size (Gamson, 1961), and coalitions with a minimum number of members. Like the classical power indices of Shapley and Shubik (1954) and Banzhaf (1965), these criteria have all presupposed only knowledge of the distribution of weights (e.g., seat distributions in parliamentary systems). About a dozen years ago these models were successfully challenged by coalition theories which, in addition to the distribution of weights, incorporate notions like ideological space or party distance and postulate the minimization of some form of ideological diversity of the coalition (Axelrod, 1970; DeSwaan, 1973). For example, in his "conflict of interest" theory, Axelrod (1970) predicted the formation of "minimal connected winning coalitions." Connected coalitions consist of coalitions of parties that are adjacent in a one-dimensional ideological ordering and, hence, minimize the "conflict of interest." Minimal connected winning coalitions are connected coalitions for which the deletion of either extreme member renders the coalition losing. Extensions to multidimensional ideological spaces, which remove the constraint imposed by Axelrod, have been proposed more recently by Winer (1979) and Grofman (1982).

Empirical studies (DeSwaan, 1973; Taylor & Laver, 1973) have shown that coalition-formation theories that incorporate information about ideological proximity are descriptively more adequate than theories that disregard this information. The major purpose of the present study is to test the hypothesis that this same conclusion also holds in the area of perceived voting power. Specifically, we use an experimental approach to measure perceived voting power in the Israeli Knesset in order to test competitively six models of perceived voting power. Three of these six models are the power indices of Shapley and Shubik (1954), Banzhaf (1965), and Deegan and Packel (1979), which in general yield different predictions. The other three models generalize the first three power indices by accounting for both the party seat distribution and the ideological proximities between the political parties in the Knesset.

Two additional questions are addressed in an attempt to assess the validity and generality of our conclusions. The first issue is methodological. We

attempt to ascertain how—if at all—voting power is perceived at a time when a political coalition is about to be formed. To measure perceived voting power, we use and test a recently proposed scaling procedure (Saaty, 1977, 1980) which possesses several desirable properties, among them an index of the consistency of the subject's responses and a ratio scale of perceived power. The ratio scale properties are required because the predictions of all six models are only unique up to a multiplication by a positive constant. The second question concerns the effects of political sophistication and experience of the respondents on their perception of power. To this end we have collected data from three groups of respondents, namely, graduate students of political science, Knesset members, and Knesset correspondents.

The second section defines several basic concepts of game theory and describes briefly the three power indices of Shapley and Shubik (1954), Banzhaf (1965), Deegan and Packel (1979), and their respective generalizations. The third section provides information about the subjects and the experimental procedure used to elicit their responses, and the remaining sections report the results of the study and discuss their implications.

Power Indices for Simple Games

As mentioned above, measuring perceived voting power requires examining two sets of power indices, each set consisting of three indices. To understand these six indices and the predictions they make in our Knesset study, several basic concepts from game theory must first be reviewed.

Simple Games

n -person cooperative games that model weighted voting bodies in which each coalition that might form can either "win" or "lose" are called simple (von Neumann & Morgenstern, 1947). Simple games are attractive because they constitute appropriate models for many n -person conflict situations, particularly in political structures such as parliaments and committees, but they are also appealing because the sparseness of their structure enables in-depth analyses of the game.

Consider a set $N = \{1, \dots, n\}$ of n players. Let N be the power set of N (the set of all 2^n possible coalitions), and define a partition of N into two mutually exclusive and exhaustive sets: W , the set of all *winning* coalitions; and L , the set of all *losing* coalitions. Meaning is assigned to "win-

ning" and "losing" by three requirements for this partition:

- 1) $\emptyset \in \mathcal{L}$,
- 2) $N \in \mathcal{W}$,
- 3) if $R \in \mathcal{W}$ and $R \subset S$, then $S \in \mathcal{W}$.

Conditions (1) and (2) guarantee that the null coalition of no players cannot win and that the grand coalition of all players must win, respectively. The heart of the definition is condition (3), which expresses the intuitive electoral meaning of "winning" in that no losing coalition may contain a winning coalition. A *simple game* can then be defined as a pair (N, \mathcal{W}) , where N is the set of players and \mathcal{W} is the set of all winning coalitions.

A further, almost universally applied, restriction is the condition

- 4) If $S \in \mathcal{W}$, then $(N - S) \in \mathcal{L}$.

Condition (4) guarantees that at most one coalition extracted at any single time from the body of n players can be winning. Simple games satisfying condition (4) as well as conditions (1) through (3) are called *proper* (Shapley, 1962).

There are two types of player in simple games, which deserve mentioning. Player $i \in N$ is called a *dictator* if every coalition of which he or she is a member wins, and every coalition he or she is not in loses. A *dummy* is a player in a simple game who can never, by joining a losing coalition, change it to a winning one.

Interest in proper simple games most often centers on a subset of the winning coalitions called minimal winning coalitions, and denoted by \mathcal{W}^m . A coalition S is said to be *minimal winning* if every proper subset of S is losing, whereas S itself is winning.

A proper simple game can be completely specified by its set of minimal winning coalitions. Alternatively, it is sometimes possible to specify the set of winning coalitions by assigning non-negative integer "weights" (e.g., delegation votes and parliamentary seats) w_1, w_2, \dots, w_n to the players, and then specifying a criterion W such that

$$S \in \mathcal{W} \leftrightarrow \sum_{i \in S} w_i \geq W.$$

Games in which this specification is possible are termed *weighted majority games*; often, but not necessarily, W is the integer just greater than $(w_1 + w_2 + \dots + w_n)/2$. It is convenient to use a shorthand notation $[W; w_1, w_2, \dots, w_n]$ to describe weighted majority games. This notation is known as a *weighted majority represen-*

tation of a simple game. A simple game may have more than one weighted majority representation. But when the weights have natural interpretations, as in parliamentary seats, there can be little quarrel about their nature.

The Shapley-Shubik Power Index. The Shapley-Shubik (SS) power index is a specialization of the Shapley (1953) value, which measures a player's power in terms of the relative expected frequency with which he or she turns a losing coalition into a winning coalition by casting the deciding vote. Consider all possible orderings of the n players in a simple game, which may be taken as all of the potential ways of building up toward the grand coalition. For each of these $n!$ permutations, some unique player joins and thereby turns what was a losing coalition into a winning one; this unique player is called the *pivot*. The SS power index of any player i , denoted by ϕ_i , is defined as the number of permutations in which player i is the pivot divided by the total number of alignments. The SS index (Shapley & Shubik, 1954) is computed from

$$\phi_i = \sum [(s-1)!(n-s)!] / n!,$$

where $s = |S|$ is the number of players in coalition S , and the summation is taken over all winning coalitions S for which coalition $S - \{i\}$ is losing. The power index for a simple game will be stated as the vector of power indices of its players, $(\phi_1, \phi_2, \dots, \phi_n)$.

Although the pivotal-player interpretation leading to the SS index is sufficient grounds for entertaining it, interest is increased because it may be uniquely derived from an axiomatic characterization (Dubey, 1975) and may be given additional interpretations (Roth, 1977; Straffin, 1978).

The Banzhaf Power Index. Banzhaf (1965) introduced another index for measuring power in simple games, in which all combinations of voters are considered in constructing the index, rather than all permutations as with the SS index. The Banzhaf (BZ) index of power may be developed in much the same manner as the SS index. For a simple game (N, \mathcal{W}) define a *swing* for player i to be a coalition S , where player i is a member of coalition S , S is winning, and coalition $(S - \{i\})$ is losing. Let η_i denote the number of swings for player i ; η_i , the "raw" BZ index, counts the number of times player i could change a coalition from losing to winning. Define

$$\beta_i = \eta_i / 2^{n-1},$$

where 2^{n-1} is the total number of coalitions containing player i , and therefore the maximum number of swings player i could possibly have. The vector $(\beta'_1, \dots, \beta'_n)$ is called the *absolute*

BZ index. In general, $\sum_{i=1}^n \beta'_i \neq 1$. However, as stated by Banzhaf, it is the ratio of "raw" Banzhaf indices which is of importance. It is common, therefore, to use the *normalized BZ index* $(\beta_1, \dots, \beta_n)$ where

$$\beta_i = \beta'_i / \sum_{i=1}^n \beta'_i, \quad i \in N.$$

Like the SS index, the BZ index has also been given an axiomatic characterization (Dubey, 1975; Dubey & Shapley, 1979) and alternative interpretations (Roth, 1977; Straffin, 1978).

The Deegan-Packel Power Index. Unlike the SS and BZ indices, which are based on a priori notions of power as the ability to be the decisive voter in an abstract legislative body, the Deegan-Packel (DP) index is an attempt to characterize power based on the observed ways in which voters in such legislatures supposedly behave. Deegan and Packel (1979, p. 122) intend "to reflect what we consider to be realistic dimensions of participant behavior and spoils distribution in certain game type situations; notably the exercising of power through interpersonal interactions that may be characterized as being 'political' in nature with all participants in victorious coalitions sharing equally in the spoils."

Following Riker (1962), the model assumes that only minimal winning coalitions will emerge. Formally, given a simple game (N, W) , define the collection of all minimal winning coalitions that contain player i by

$$W^m(i) = \{S \in W^m \mid i \in S\}.$$

The DP power index for player i , denoted by ρ_i , is defined by

$$\rho_i = \frac{1}{|W^m|} \sum_{S \in W^m(i)} 1/s, \quad (1)$$

where $|W^m|$ is the total number of minimal winning coalitions in the game and $s = |S|$ is as before. The DP power index is then the vector of individual player power indices, (ρ_1, \dots, ρ_n) .

Three assumptions regarding the behavior of players in simple games give rise to this definition of power:

1. Only minimal winning coalitions will form.

2. Players in a minimal winning coalition will divide the coalition payoff equally.
3. Each minimal winning coalition has an equal probability of forming.

Deegan and Packel (1979) show that the power index ρ_i in equation (1) is consistent with and determined by these assumptions. They have also given their power index an axiomatic characterization. The resulting axiom set is similar to that for the SS index, but admittedly (Deegan & Packel, 1979, p. 117) lacks the compelling universality of earlier sets.

Example. To illustrate the three power indices above, consider a parliamentary legislation with four parties A, B, C , and D controlling 30, 20, 10, and 40 seats, respectively. Suppose that to pass a piece of legislation a two-thirds majority is required. Then this system may be represented as a weighted majority game

$$[67; \overset{A}{30}, \overset{B}{20}, \overset{C}{10}, \overset{D}{40}]$$

This game has neither a dictator nor dummy players.

There are altogether $4! = 24$ permutations of the four parties. Parties A, B, C , and D occupy pivotal positions in 6, 2, 2, and 14 permutations (orderings), respectively. Hence,

$$\begin{aligned} (\phi_A, \phi_B, \phi_C, \phi_D) &= \frac{1}{24} (6, 2, 2, 14) \\ &= (0.250, 0.083, 0.083, 0.583). \end{aligned}$$

To compute the Banzhaf values, consider all the winning coalitions in the game: AD, ABD, ACD, BCD , and $ABCD$. Counting the number of swings yields $\eta_A = 3, \eta_B = \eta_C = 1$, and $\eta_D = 5$. Therefore,

$$\begin{aligned} (\beta_A, \beta_B, \beta_C, \beta_D) &= \frac{1}{10} (3, 1, 1, 5) \\ &= (0.333, 0.100, 0.100, 0.500). \end{aligned}$$

To compute the DP power values, note that there are two minimal winning coalitions in the game: $W^m = \{AD, BCD\}$. Equation (1) yields directly

$$\begin{aligned} \rho_A &= 1/2(1/2) = 1/4, \rho_B = \rho_C = 1/2(1/3) \\ &= 1/6, \rho_D = 1/2(1/2 + 1/3) = 5/12. \end{aligned}$$

The DP values are given by:

$$\begin{aligned} (\rho_A, \rho_B, \rho_C, \rho_D) &= \frac{1}{12} (3, 2, 2, 5) \\ &= (0.250, 0.167, 0.167, 0.417). \end{aligned}$$

In the present example the DP index provides the most egalitarian results, the SS index gives the least egalitarian results, and the BZ index falls in between. Each of the three power indices assigns the same voting power to parties B and C despite the 10 seat difference between them.

Nonsymmetric Generalizations of Power Indices

Even a cursory examination of the three power indices shows that the analysis of political voting situations they provide is in many ways insufficient or superficial. The power indices described above ignore the realities of political, economic, or psychological life where alliances, cohesive blocs, discrimination, favoritism, and partisan actions are the rule rather than the exception. Although it is possible that some or perhaps most of these factors will play only insignificant roles in the perception of voting power, it seems unlikely that all of them will be ignored. Experience suggests that some voting combinations or coalitions are more likely to form than others. Notwithstanding individual differences in weights, this asymmetry is most likely to be caused by ideological proximity in parliamentary systems.

Recognizing the seriousness of the limitation of the power indices described above, Owen (1971) proposed a generalization of the Shapley (1953) value, which explicitly considers the affinity among players. In a subsequent article, Shapley (1977) generalized the SS power index to account for asymmetry in preferences, habits, or outlooks of the players. Shenoy (1980) later proposed a similar generalization of the BZ index. And the DP index, too, may be generalized in a straightforward manner. All three generalizations are computational schemes that consider both the weight distribution and ideological proximity between all pairs of players. In all three generalizations, the n players are assumed to be represented as n points in some politically interpretable multidimensional Euclidean space, and the ideological proximity is assumed to be proportional to the Euclidean distance in this space.

The Generalized Shapley-Shubik Power Index. To allow for proximities between players, Shapley (1977) proposes representing the players as n points in an m -dimensional Euclidean space, R^m . This representation is quite common in psychological studies of cognition and nonmetric multidimensional representations of similarity data (Kruskal & Wish, 1978). It leaves ample scope for capturing many kinds of parameters without an excess of arbitration,

including the intuitive ideas of "modernization" and "extremism" which are prominent in political analyses (Shapley, 1977).

Both political issues and profiles are represented in the generalized model by m -dimensional vectors regarded as belonging to different, dual spaces. Figure 1 illustrates this representation for $n = 5$ and $m = 2$. The two arrows indicate the directions associated with two typical issues. Figure 1 shows that each arrow determines an order of the five players by the simple process of dropping perpendiculars to the shafts of the arrows. As an arrow turns, different player-orders are produced; a 180° turn reverses the order. Turning an arrow 360° may not generate all $n!$ possible orders. For example, in Figure 1 player B will never appear in any order as the initial or final element. When m is small and n is relatively large, as will be the case in many applications, the model envisages that only a very small fraction of the possible alignments of players will actually be ideologically consistent.

It is assumed that all issue directions are equally likely—"that the 'political winds' blow across the ideological space in a perfectly random way" (Shapley, 1977, p. 20). An arrow ξ can be chosen that represents an issue according to the uniform probability distribution over the unit sphere defined by

$$S^m = \{ \xi \in R^m \mid (\xi_1^2 + \xi_2^2 + \dots + \xi_m^2)^{1/2} = 1 \}. \quad (2)$$

Then the probability that ξ lies in any given region in S^m is proportional to the area of that region.

Now, for each player $i \in N$ and each issue (arrow) ξ define $P_i(\xi)$ as the set of players who are more enthusiastic about the issue direction ξ than i is. By "more enthusiastic" is meant that these players are ranked below player i in the player order generated by ξ . Also define S_i^m to be the set of issue directions in S^m for which $P_i(\xi) \in L$, and $P_i(\xi) \cup \{i\} \in W$. In other words, S_i^m is the set of issue directions for which player i serves as pivot in the simple game. Then the generalized Shapley-Shubik (GSS) power index is given by

$$\phi_i^\xi = \frac{\text{area of } S_i^m}{\text{area of } S^m}, \text{ for every } i \in N.$$

As expected, $\sum_{i \in N} \phi_i^\xi = 1$. When $m = n$ and

the n points are the unit vectors of R^m , GSS reduces to SS. Examples show that the GSS model has the desirable property that if player i

occupies a "central" position in R^m his or her power is enhanced relative to the SS model.

The Generalized Banzhaf Power Index. Shenoy (1980) proposed a generalization of the BZ power index, which combines elements of Shapley's generalization of the SS index and Straffin's (1978) probabilistic interpretation of both the SS and BZ indices. Shenoy's non-symmetric generalization may be described as a sequence of three steps.

The first step assumes that the players are presented as n points in R^m . Each dimension of R^m is interpretable in terms of psychological, economic, or political parameters, exactly as in the traditional factor analysis and nonmetric multidimensional scaling techniques. The ideological space is a subspace of R^m defined by

$$B_{1/2}^m = \{ \mathbf{x}^i \in R^m | ((x_1^i)^2 + \dots + (x_m^i)^2)^{1/2} \leq 1/2 \}, \quad i \in N.$$

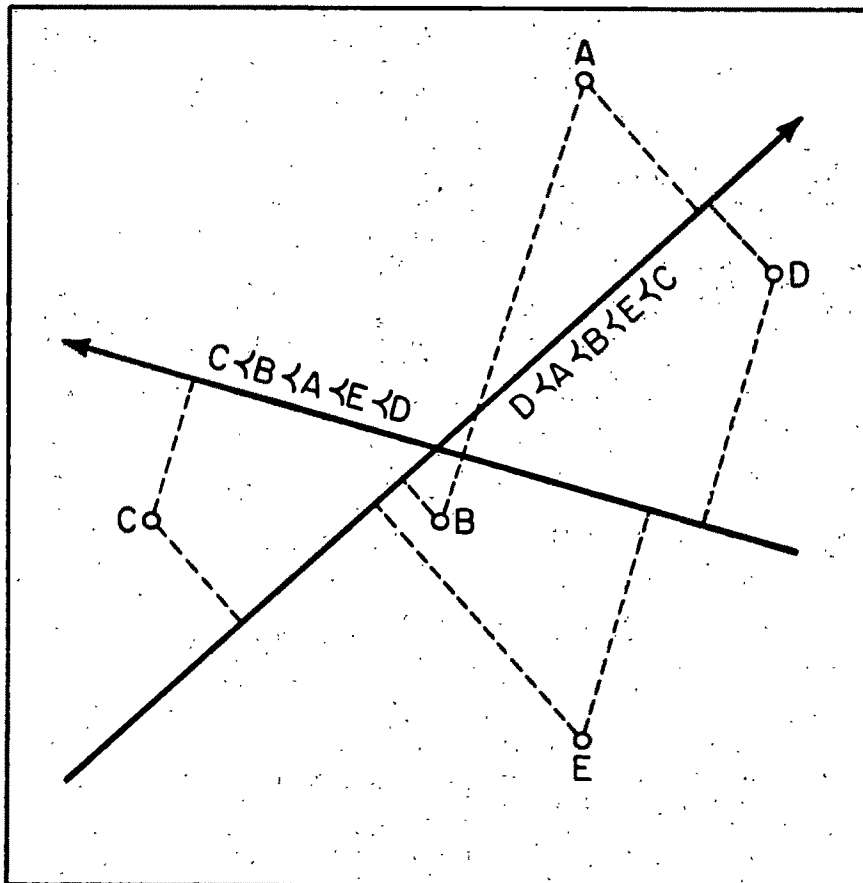
Each player i is thus represented by a point \mathbf{x}^i in $B_{1/2}^m$, which is called the profile of player i .

To implement the second step of Shenoy's computational scheme, it is assumed that the assembly of players will be called to vote on a series of proposals or bills called *issues*. An issue in the model is a linear function on R^m represented by a point ξ on the unit sphere S^m (Eq. 2). An issue, then, is some combination of the parameters of the ideological space. Although the players' profiles \mathbf{x}^i and the issues ξ are both m -dimensional, like in the Shapley (1977) generalization, they should be regarded as belonging to different, dual spaces, not the same space.

Let p_{ξ}^i denote the probability that player i will vote "yes" for issue ξ , where

$$p_{\xi}^i = \xi_1 x_1^i + \xi_2 x_2^i + \dots + \xi_m x_m^i + 1/2. \quad (3)$$

Figure 1. A Two-Dimensional Representation of Five Players with Two Different Issue Directions



Equation (3) shows that the probability that player i will support issue ξ is proportional to his or her distance from the issue direction ξ . p_{ξ}^i satisfies $0 < p_{\xi}^i < 1$ for every $x^i \in B_{\frac{1}{2}}^m$ and every $\xi \in S^m$ and hence is a well-defined probability.

In the third step of the computational scheme the generalized Banzhaf (GBZ) power index is computed. Consider first some issue ξ . Then the swing probability for player i , $\Pi_{\xi}^i(p)$, is the probability that an issue ξ that player i supports passes but would fail were i to change his or her vote, or that the issue ξ that player i opposes fails but would pass if player i changes his or her vote.

The GBZ power index for player i , denoted by β_i^g , is the probability that player i will make a difference in the outcome summed over all issues. To compute β_i^g , Shenoy makes the same simplifying assumption as Shapley, namely, that all issue directions in the unit sphere are equally likely. If the model is assigned a political interpretation with players corresponding to political parties and issues to legislative proposals, this latter assumption amounts to requiring that all types of bills are equally likely to be introduced to the multi-party parliament.

The Generalized Deegan-Packel Power Index. A nonsymmetric generalization of the DP power index, which utilizes information about the ideological proximity between the players, may take one of several forms. To allow comparison with the two generalized indices GSS and GBZ, the approach that we propose also represents the players as n points in R^m . Assumptions (1) and (2) of the original DP model are retained, but the third, and to our mind more controversial, assumption of equiprobability of minimal winning coalitions is replaced by the assumption that the probability of forming a minimal winning coalition is inversely proportional to the mean of the Euclidean distances between all members of this coalition. Thus, the more cohesive is the minimal winning coalition in the political space, the more likely it is to form.

Formally, let $p(S)$ denote the probability that some minimal winning coalition $S \in W^m$ will form. Whereas in the original DP model $p(S) = 1/|W^m|$, in the present model $p(S)$ depends on the inter-player distances $d(i,j)$, where

$$d(i,j) = \left(\sum_{k=1}^m (x_k^i - x_k^j)^2 \right)^{1/2}, \text{ for } i,j \in S.$$

Denote the mean distance for coalition S by

$$d(S) = \sum_{i,j \in S} d(i,j)/h, \text{ for } i < j,$$

where h is the number of pairs of players in the coalition S . Then the non-normalized probability that coalition S will form is given by $p'(S) = 1/d(S)$, which after normalization becomes

$$p(S) = p'(S) / \sum_{S \in W^m} p'(S).$$

Finally, the generalized Deegan-Packel (GDP) power index for player i , denoted by ρ_i^g , is defined by

$$\rho_i^g = \sum_{S \in W^m(i)} p(S)/s, \text{ for } i \in N.$$

Example. To illustrate the effect of ideological centrality on the three generalized power indices and to compare them to each other, consider a political voting body with three parties A , B , and C controlling 20, 10, and 10 votes, respectively. Assuming a regular majority for passing legislation, this voting body can be represented by the weighted majority game [21; 20, 10, 10]. Party A is a veto player in this game; no coalition may form without it.

Suppose that in terms of their ideological positions the three parties are represented in a 2-dimensional ideological space $B_{\frac{1}{2}}^2$ such that party A is in the center of this space and parties B and C are located on its edge. Specifically, assume that the profiles of the three parties satisfy $x_1^A = x_2^A = 0$, $(x_1^B)^2 + (x_2^B)^2 = (\frac{1}{2})^2$, and $(x_1^C)^2 + (x_2^C)^2 = (\frac{1}{2})^2$. This assumption implies that A , B , and C form an isosceles triangle in which $d(A,B) = d(A,C) = \frac{1}{2}$, whereas the distance $d(B,C)$ may vary from zero to one depending on the angle $\angle BAC$, which will be denoted by α ($0^\circ \leq \alpha \leq 180^\circ$). If $\alpha = 0^\circ$, parties B and C occupy the same position in the ideological space, whereas if $\alpha = 180^\circ$, parties C , A , and B form a single ideological dimension with $d(B,C) = 1$. Hence, α measures the centrality of A with respect to the ideological positions of B and C .

The upper part of Table 1 presents the GSS, GBZ, and GDP values for selected values of α . Consider first the GSS power index. Table 1 shows that for the present example ϕ_A^g increases linearly in α , assuming the value 0.5 when $\alpha = 0^\circ$ and 1 when $\alpha = 180^\circ$. When $\alpha = 60^\circ$, A , B , and C form an equilateral triangle. In this case,

$$(\phi_A^g, \phi_B^g, \phi_C^g) = (\phi_A, \phi_B, \phi_C) = (2/3, 1/6, 1/6).$$

Consider next the GBZ index. As before, the voting power of party A is enhanced as the

Table 1. Generalized Power Indices for Two 3-Person Weighted Majority Games

α	GSS			GBZ			GDP		
	ϕ_A^g	ϕ_B^g	ϕ_C^g	β_A^g	β_B^g	β_C^g	ρ_A^g	ρ_B^g	ρ_C^g
Game: [21; 20,10,10]									
0°	0.500	0.250	0.250	0.555	0.222	0.222	0.500	0.250	0.250
30°	0.583	0.208	0.208	0.562	0.219	0.219	0.500	0.250	0.250
60°	0.667	0.167	0.167	0.579	0.211	0.211	0.500	0.250	0.250
90°	0.750	0.125	0.125	0.600	0.200	0.200	0.500	0.250	0.250
120°	0.833	0.083	0.083	0.619	0.190	0.190	0.500	0.250	0.250
150°	0.917	0.042	0.042	0.632	0.184	0.184	0.500	0.250	0.250
180°	1.000	0	0	0.636	0.182	0.182	0.500	0.250	0.250
Game: [21; 10,20,10]									
0°	0	0.750	0.250	0.111	0.667	0.222	0	0.500	0.500
30°	0.083	0.709	0.208	0.124	0.657	0.219	0.171	0.500	0.329
60°	0.167	0.667	0.167	0.158	0.632	0.211	0.250	0.500	0.250
90°	0.250	0.625	0.125	0.200	0.600	0.200	0.293	0.500	0.207
120°	0.333	0.584	0.083	0.238	0.571	0.190	0.317	0.500	0.183
150°	0.417	0.541	0.042	0.264	0.552	0.184	0.330	0.500	0.170
180°	0.500	0.500	0	0.273	0.545	0.182	0.333	0.500	0.167

centrality of A increases. However, the effect of centrality on β_A^g is weaker than its effect on ϕ_A^g : β_A^g varies from 5/9 for $\alpha = 0^\circ$ to 7/11 for $\alpha = 180^\circ$. When $\alpha = 90^\circ$, the GBZ and BZ values coincide for the present example.

The degree of centrality of player A has no effect on the GDP values for the game [21; 20,10,10] because the GDP power index, as well as the DP index, only considers minimal winning coalitions in assessing power. In the present example there are only two minimal winning coalitions, AB and AC , and the distance between their members is unaffected by varying the ideological positions of parties B and C .

To construct the lower part of Table 1, we exchanged the number of votes controlled by parties A and B without changing the parties' profiles. The result is a new game [21; 10,20,10] in which A maintains its ideological centrality though not its veto power. Parties A and C are symmetrical in terms of the number of votes they control but not in their ideological profiles. Table 1 shows that for all three generalized power indices, the power of the central party A is enhanced as its centrality increases. The strongest effect of ideological centrality is obtained for the GSS index and the weakest for the GBZ index. Because the set of minimal winning coalitions now includes coalition BC as well as AB , the centrality of A affects the GDP as well as the GSS and GBZ values.

The two example games above, as well as other examples not reported here, show that for

weighted majority games of the kind discussed in the present article, the three generalized power indices are all sensitive to the ideological profiles of the players, albeit in different degrees. They generally enhance the power of the ideologically more central parties and reduce the power of the ideologically more extreme parties.

Method

The Tenth Knesset

The institutional framework of the Israeli political system was largely established by the provisional government, founded on April 12, 1948, by the Zionist General Council. The Provisional Council chose universal suffrage and a simple proportional representation system, which treats the entire nation as a single constituency for the 120-member *Knesset* (parliament). The task of forming a government is entrusted by the president to a member of the Knesset, typically but not necessarily the leader of the party commanding the largest number of seats. The composition of the government is a topic for party negotiations; it is only partially defined as consisting of a prime minister and an unspecified number of ministers, who may or may not be parliament members. Once a government is formed, it should immediately present itself to the Knesset and be regarded as constituted when it has received from the Knesset its vote of confidence.

Table 2. Frequency Distributions of Party Membership for the Tenth Knesset and for Group K

Party	Knesset		Group K	
	Frequency	Proportion	Frequency	Proportion
DFP	4	0.033	1	0.042
DMC	2	0.017	1	0.042
CRM	1	0.008	0	0
LP	47	0.392	9	0.375
MNR	2	0.017	1	0.042
U	48	0.400	11	0.458
MIT	3	0.025	0	0
NRP	6	0.050	1	0.042
AI	4	0.033	0	0
R	3	0.025	0	0
Total	120	1.000	24	1.000

The election to the tenth Knesset took place on July 1981 after a long and bitter campaign. Table 2 lists the ten parties that elected members to the Knesset together with the seat distribution. For a detailed description of the Israeli political system, see Arian (1966, 1973) and Seliktar (1982). Here we only present a brief characterization of the ten parties.

Hadash, which is an acronym for *Hazit Demokratit Leshalom* (Democratic Front for Peace, or DFP), is an orthodox, pro-Russian, anti-Zionist Communist party, committed to an aggressive class struggle and identified with the Arab stand in the Middle East conflict.

Hatnua Hademokratit Leshinui (Democratic Movement for Change, or DMC) favors liberal economic and welfare policies and a moderate approach to the Israeli-Arab conflict.

Hatnua Lezhuiot Haezrah (the Civil Rights Movement, or CRM) is primarily interested in protecting the secular character of the State of Israel and the civil rights of its citizens. The CRM advocates a liberal economic policy and a dovish stand in the Israeli-Arab conflict.

The *Labor Party* (LP) was established in 1968 by a merger of three parties—*Mapai*, *Ahdut-ha-Avoda*, and *Rafi*. The LP is the most pluralistic party in Israeli politics. Undergoing an evolutionary development since 1948 by perceptibly shifting its position from a pronounced left-wing ideology to a more left-of-center position, the LP professed in 1981 a moderate socialistic and welfare-state orientation toward socioeconomic issues without negating the role of a capitalistic economy. It accepted a modern degree of nonseparation between state and religion. Adopting a pro-Western orientation in foreign affairs, it steered a middle course between the maximalist and minimalist demands by evolving a program that seeks a peace settlement with the Arabs while maintain-

ing control of part of the occupied territories (Seliktar, 1982).

Hatnua Lehitadshut Mamlaktit (Movement for National Revival, or MNR) espouses the superiority of the state mechanism over its sectional components like the *Histadrut*—the general trade union organization in Israel. It opposes the imposition of a rigid wage structure by the *Histadrut* and its protection of labor rights to the detriment of efficient management, and it advocates a right-to-center stand on the Arab-Israeli conflict.

Likud (Union, or U) is a bloc of right-wing parties led by *Herut*, which for 30 years has been the main opposition party. Reflecting conservative principles, *Likud* calls for a laissez-faire economic policy and the abolition of *Histadrut* controls. Adhering to a highly nationalistic foreign policy, most of the party's activity has been directed toward promoting its national policy, which includes territorial demands and a militant stand toward the Palestinian issue. The *Likud* has maintained control of the Knesset from 1977.

Tenuat Masoret Israel (Movement for Israeli Tradition, or MIT) is a religious party primarily interested in improving the socioeconomic conditions and enhancing the political power of Oriental Jews.

Miflaga Datit Leumit (National Religious Party, or NRP) is primarily concerned with incorporating religious laws into the law of the state, especially in education, with defining the Jewish status of individuals, and with family laws. The party is committed to a liberal economy; it promotes a nationalistic foreign policy which includes territorial demands, although to a less extent than *Likud*.

Agudat Israel (Association of Israel, or AI) is an extremely religious party, rigidly committed to the rule of the Jewish religion. It is an anti-establishment party that, though participating in

secular coalitions highly incompatible with its stand, has endeavored to impose its philosophy through an extreme opposition.

Hathia (Renaissance, or R) is an ultranationalist right-wing party, which left the *Likud* because of its maximal territorial demands and extremely nationalistic foreign policy.

Subjects

Three groups of subjects participated in the study. Group *S* includes 21 undergraduate and graduate students of political science at the University of Haifa, who had to satisfy three requirements: They had taken at least one course on the structure of the Israeli political system, they were present on campus on the day the study was conducted (during the final examination period), and they agreed to participate in the study before taking their final examination.

Group *K* contains 24 Knesset members who were selected as follows. Immediately after the official announcement of the outcomes of the election to the tenth Knesset, letters were hand delivered to the 120 newly elected members, asking them to participate in a study on the assessment of political power and ideological similarity between parties. The Knesset members were each asked to devote approximately 90 minutes to an individual interview to take place at the time and location of his or her choosing. No response to the letter was required. One to two days later attempts were made to contact the Knesset member and secure the desired interview, which could only take place between the fifth and eighth day after the elections. Several Knesset members could not be located or approached, many refused to be interviewed for personal or political reasons, and others would not or could not work the interview into their schedule, typically mentioning the very short notice as the reason for their refusal.

Table 2 shows the distribution of the members of group *K* by party affiliation. The difference between the relative frequency distribution for group *K* and the population frequency distribution, shown on the left-hand side of Table 2, is statistically insignificant ($\chi^2(2) = 0.42$). Although the subjects of group *K* were not chosen randomly, there is no reason to doubt their representativeness.

Group *R* includes seven senior parliamentary correspondents of the Israeli media, who have been covering parliamentary events in the Knesset for some time. Group *R* consists of all the parliamentary reporters who agreed to the interview and succeeded in working it into their very tight schedule.

Experimental Procedure

Ideological spaces and party distance matrices have been typically constructed from party voting records or from proximity data collected while a particular government was in power. The ideological positions obtained by either of these methods are not entirely suitable for use in any attempt to assess perceived voting power because the parties' voting power is itself partially determined by the membership of the government (Taylor & Laver, 1973). To overcome this methodological difficulty, the timing of the study was chosen with much care. By having subjects assess ratios of voting power and subsequently ideological similarity between pairs of parties after the election to the tenth Knesset but before the formation of a coalition government, we have ensured as much as possible that the membership of the government has no effect on the subjects' responses.

Within a brief period of four days, two practiced experimenters delivered the instructions, elaborated on them when necessary, and collected the data. Whereas the subjects of group *S* were tested collectively in a classroom, the subjects of groups *K* and *R* were tested individually either in their offices or in the Knesset building. For all three groups, the study lasted between 60 and 90 minutes.

The task consisted of two separate parts. The first part used the scaling technique of Saaty (1977, 1980) for measuring political power on a ratio scale (see the Appendix). Each subject was given written instructions which told him or her that the purpose of the study was to assess the political power of the parties in the tenth Knesset. Although the notion of power was not defined, three examples of a hypothetical Knesset with only four parties were presented in weighted majority form to illustrate its complexity.

After the written instructions and their verbal elaboration, the subject was given a response sheet and a list of the ten parties in the Knesset with the number of seats they control. Then all 45 pairs of parties were randomly presented to the subject, one at a time. The subject was asked to attend to each pair separately and to indicate on the response sheet 1) which of the two parties was politically more powerful, and 2) by how much. No constraints were placed on the response scale; all rational numbers equal or larger than 1 were permitted. No time constraint was imposed; the experiment was self-paced.

In the second part of the task, similarity judgments were collected to be later subjected to non-metric multidimensional scaling (Coxon, 1982; Davison, 1983) for recovering the subject's political space. The task was again self-paced. All 45

pairs of parties were presented to the subject in a different random order, one at a time. For each pair the subject was asked to specify the degree of political or ideological similarity on a 0-100 response scale.

Results

Consistency of Power Ratio Judgments

The matrix A was constructed for each subject separately from the $n(n-1)/2$ power ratio judgments (see the Appendix), and then the maximal eigenvalue (denoted hereafter by λ rather than λ_{\max}) of A was extracted and the consistency index was computed from Eq. (A2). The values of λ and μ are presented in Table 3, which also shows the response scales of 1 to r used by the individual subjects.

Of the 52 subjects, 45 chose to use numbers (mostly integers but occasionally fractions) between 1 and 10, 5 used numbers between 1 and 9, one subject between 1 and 8, and another subject between 1 and 50. Because almost all the subjects restricted themselves to a 1-9 or 1-10 response scale, the Monte Carlo results reported by Saaty and mentioned in the Appendix are applicable to our data. Table 3 shows that none of the eigenvalues exceeded or even approached the critical

level 19.66. The largest value of λ in Table 3 is 14.16 (K20), and only 9 of the 52 λ 's exceed 12. Turning next to the consistency index μ (Eq. (A2)), Table 3 shows that for only 10 of the 52 subjects $\mu > 0.2$.

Combining the results of the two consistency tests, we concluded that for most of the subjects a meaningful ratio scale of political voting power could be constructed. Although the consistency of the responses of 10 subjects may be suspect, it was decided not to omit any subjects from subsequent analyses.

Analysis of Perceived Power

Having established the consistency of the subjects' pairwise comparisons, the eigenvectors of A were next extracted, one for each subject. We shall refer to the eigenvector w ($w_i \geq 0$, $i=1, \dots, n$; $w_1 + \dots + w_n = 1$) as the vector of perceived power. Table 4 presents the means and standard deviations of the perceived power of the ten parties, computed separately for each group and over all 52 subjects.

A comparison of Tables 2 and 4 shows that the four mean vectors of perceived power differ from the vector of proportions of Knesset seats controlled by the parties. Perceived power in the present study cannot be solely accounted for in terms

Table 3. Eigenvalues, Consistency Measures, Response Range, and Two Goodness-of-Fit Measures for Subjects of All Groups

Subject	λ	μ	r	RSQ	STR	Subject	λ	μ	r	RSQ	STR
S01	11.08	0.12	10	0.90	0.15	K06	10.35	0.04	10	0.93	0.12
S02	10.25	0.03	10	0.86	0.17	K07	10.22	0.03	10	0.90	0.16
S03	10.21	0.02	10	0.81	0.17	K08	10.51	0.06	10	0.93	0.12
S04	11.30	0.14	10	0.84	0.16	K09	11.24	0.14	10	0.85	0.19
S05	10.47	0.05	9	0.96	0.09	K10	11.51	0.17	10	0.92	0.14
S06	10.95	0.11	10	0.91	0.15	K11	10.56	0.06	10	0.84	0.15
S07	11.18	0.13	10	0.89	0.18	K12	12.53	0.28	10	0.77	0.18
S08	10.85	0.10	10	0.86	0.16	K13	13.89	0.43	10	0.94	0.12
S09	10.56	0.06	10	0.82	0.17	K14	10.49	0.06	10	0.80	0.21
S10	10.30	0.03	10	0.83	0.17	K15	10.96	0.11	10	0.88	0.15
S11	11.06	0.12	10	0.93	0.13	K16	10.36	0.04	10	0.95	0.11
S12	10.94	0.11	10	0.92	0.12	K17	12.98	0.33	10	0.90	0.15
S13	10.71	0.08	50	0.78	0.19	K18	10.64	0.07	10	0.88	0.19
S14	10.96	0.11	10	0.77	0.22	K19	10.95	0.11	9	0.87	0.15
S15	10.62	0.07	10	0.76	0.21	K20	14.16	0.46	10	0.92	0.17
S16	12.12	0.24	10	0.86	0.16	K21	13.45	0.38	10	0.87	0.14
S17	12.53	0.28	10	0.95	0.10	K22	10.97	0.11	10	0.86	0.15
S18	10.96	0.11	10	0.82	0.17	K23	11.16	0.13	9	0.94	0.12
S19	10.71	0.08	10	0.85	0.20	K24	11.85	0.21	10	0.79	0.18
S20	10.43	0.05	10	0.90	0.13	R01	11.35	0.15	10	0.85	0.17
S21	11.28	0.14	9	0.81	0.18	R02	11.22	0.14	10	0.92	0.15
K01	12.21	0.25	10	0.91	0.15	R03	11.18	0.13	10	0.88	0.16
K02	11.65	0.18	10	0.94	0.13	R04	11.05	0.12	10	0.93	0.11
K03	10.64	0.07	9	0.88	0.15	R05	10.64	0.07	10	0.87	0.16
K04	12.59	0.29	10	0.84	0.17	R06	11.50	0.17	10	0.88	0.17
K05	10.29	0.03	8	0.88	0.15	R07	11.36	0.15	10	0.95	0.09

Table 4. Means and Standard Deviations of Perceived Voting Power

Party	Group S		Group K		Group R		Overall	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
DPF	.026	.010	.020	.012	.018	.006	.022	.010
DMC	.029	.009	.026	.012	.020	.004	.027	.010
CRM	.025	.009	.022	.010	.020	.005	.023	.009
LP	.187	.091	.162	.082	.109	.080	.165	.087
MNR	.044	.017	.045	.016	.038	.016	.044	.015
U	.338	.059	.323	.091	.292	.081	.325	.065
MIT	.060	.024	.072	.031	.104	.031	.071	.030
NRP	.139	.045	.149	.042	.175	.055	.149	.043
AI	.108	.046	.130	.049	.173	.047	.127	.048
R	.043	.016	.052	.057	.052	.014	.048	.018

of the distribution of seats. The comparison shows that the mean perceived power of the Labor and Likud parties was considerably lower than their proportion of seats. On the other hand, the perceived power of each of the three religious parties MIT, NRP, and AI exceeded their proportion of seats by a factor of 3 or 4; the three religious parties marshal together only 10.8% of the Knesset seats, whereas their combined perceived power averaged over all the subjects equals 34.7%.

The discrepancy between objective power, as measured by the proportion of seats, and subjective or perceived power may be explained in terms of the likelihood of being included in a winning coalition and thereby sharing the spoils. Several observations of parties with the same number of seats but different perceived power support this explanation. DPF, which had no chance of entering a coalition, even a coalition of national unity, was assigned a mean power of 0.022, whereas AI, which had the same number of seats but was considered by most analysts as a highly likely candidate for membership in the coalition, was assigned a mean power approximately six times larger. Perhaps less dramatic but more important is the comparison between the two major parties U and LP, which controlled 48 and 47 seats, respectively. Probably sharing the general consensus that U was more likely to form a winning coalition than LP, a consensus that proved true later, the subjects in the present study judged on the average that U was twice as politically powerful as LP.

Table 4 also shows group differences in the mean perceived power of several parties. As the political sophistication of the group members grows, the mean perceived power of LP drops from 0.187 for group S through 0.162 for group K to 0.109 for group R. The mean perceived power of U also decreased in the same direction, although at a slower rate, from 0.338 for group S

through 0.323 for group K to 0.292 for group R. Owing to large within-group variability, neither of these two group differences was statistically significant by a one-way ANOVA ($p \geq .05$). However, when the perceived powers of both dominant parties were combined, a one-way ANOVA yielded a significant group effect ($F(2,49) = 3.51$, $p < .05$). In a similar fashion, the powers assigned to MIT, NRP, and AI were combined to get a general measure of religious power. The mean religious power was 0.308, 0.350, and 0.451 for groups S, K, and R, respectively. A one-way ANOVA resulted in a significant group effect ($F(2,49) = 5.18$, $p < .05$), showing that as the political sophistication increases more political power is attributed to the religious parties in Israel.

Better understanding of the perception of political power may be gained by studying the relationship between the perceived power of different parties over individual subjects. Party size, political ideology, or the history of conflict and cooperation between the parties may result in a positive relationship between the perceived power of certain pairs of parties or a negative relationship for others. To investigate these relationships, all 45 pairwise correlations between perceived power were computed over all 52 subjects. Because the ten weights for each subject sum up to the same constant, the null expectation is for a low negative correlation between parties. Consequently, we chose the significance level of 0.01 for testing the correlations. The results are presented in Table 5.

Table 5 shows several patterns of correlations which are politically interpretable.

1. Regardless of their political ideology, small parties with three or fewer parliament seats (DMC, CRM, MNR, MIT, R) are perceived to resemble one another because of their size. Of the ten correlations in Table 5 between the five "small" parties, eight are positive and the remain-

Table 5. Correlations between Measures of Perceived Power

	DFP	DMC	CRM	LP	MNR	U	MIT	NRP	AI	R
DFP	1.00									
DMC	.38*	1.00								
CRM	.60*	.78*	1.00							
LP	.43*	.14	.18	1.00						
MNR	-.03	.52*	.34	-.33	1.00					
U	-.26	-.35	-.45*	.09	-.26	1.00				
MIT	-.18	-.01	-.03	-.70*	.27	-.47*	1.00			
NRP	-.29	-.25	-.19	-.72*	.08	-.43*	.56*	1.00		
AI	-.42*	-.22	-.20	-.78*	.13	-.40*	.59*	.73*	1.00	
R	-.09	.20	.24	-.32	.31	-.38*	.41*	.11	.20	1.00

$p < 0.01$.

ing two (between DMC and MIT and between CRM and MIT) are practically zero. Of the eight positive correlations, five are significant.

2. Like the five small parties mentioned immediately above, the three religious parties MIT, NRP, and AI are also positively correlated with one another. All three correlations between the religious parties are significant (0.56, 0.59, and 0.73 for MIT and NRP, MIT and AI, and NRP and AI, respectively). Although the three religious parties may compete with one another for the religious vote, they do not compete with each other for power. Rather, they are perceived as constituting one bloc with a relatively high or low power depending on the importance and attention given by the subject to the bargaining tactics used by the religious parties and their ability to satisfy their political demands.

3. The perceived power of each of the religious parties is negatively and significantly correlated with the perceived power of the LP. The correlations between LP and MIT, LP and NRP, and LP and AI are -0.70 , -0.72 , and -0.78 , respectively, each of which accounts for 50% or more of the response variability. Similarly, the correlations between U and each of the three religious parties are also negative and significant (-0.47 , -0.43 , and -0.40). Finally, the correlation between U and LP (0.09) is practically zero. Politically, this pattern of seven correlations makes much sense because immediately after the 1981 elections a coalition between U and the religious bloc or between LP and the religious bloc was considered much more likely than any other coalition, in particular a national unity coalition including U and LP.

Inspection of Table 5 shows that 14 of the 18 significant correlations are accounted for by patterns 1, 2, and 3 above; the remaining four significant correlations all concern the DFP.

Spatial Analysis of Proximity Data

In addition to the power ratio judgments, each subject assessed the ideological similarity between any two parties on a 0-100 response scale. The resulting 10×10 symmetric proximity matrix of each subject was subjected to a nonmetric multidimensional scaling (Coxon, 1982; Davison, 1983; Kruskal & Wish, 1978; Schiffman, Reynolds, & Young, 1982). A version of ALSCAL (Young & Lewyckij, 1979) in the SAS computer package was used. For any prespecified value of m , the ALSCAL algorithm computes and prints two measures of goodness-of-fit in addition to the $m \times n$ coordinates of the configuration. The RSQ measure is the squared linear correlation between the proximity measures and the corresponding Euclidean distances in the m -dimensional configuration. The other measure of goodness-of-fit, denoted by STR, is known as S-STRESS. The present study employed S-STRESS formula one, which is defined in terms of squared distances and squared disparities and is recommended (Davison, 1983) for proximity data. Both the RSQ and STR measures for the two-dimensional spatial solutions are presented in Table 3.

The common criteria of fit, reproducibility, and interpretability were used to determine the appropriate dimensionality of the spatial configurations. For most subjects, a dimension by STR plot began to level off at two dimensions, suggesting that the appropriate solution may be the two-dimensional configuration. Three-dimensional solutions had to be rejected in any case, not because of any belief that political spaces are appropriately interpretable in terms of one or two dimensions only, but because of the recommendation (Young & Lewyckij, 1979) always to keep the n/m ratio no smaller than 4. With $n = 10$ in the present study, $m \leq 2$. The RSQ results also sup-

ported the decision that $m = 2$. The mean RSQ measures for groups S, K, and R were 0.857, 0.881, and 0.897, respectively, showing that more than 85% of the variability in the similarity judgments could be explained by the linear relationship between the proximities and the two-dimensional Euclidean distances.

Reproducibility dictates that the spatial solution be composed of dimensions that emerge consistently across subgroups (Davison, 1983). In analyses not reported here the proximity matrices of all the subjects in each group were jointly subjected to an individual differences nonmetric multidimensional scaling, using the ALSCAL program to fit the weighted Euclidean model. The resulting three two-dimensional configurations for groups S, K, and R were practically identical to one another (Golan, 1984, Chap. 6). Moreover, approximately 60% of the individual two-dimensional solutions exhibited more or less the same shape, with most of the parties maintaining their relative positions in the configuration.

Most of the individual two-dimensional configurations as well as the three configurations for groups S, K, and R mentioned above were shaped like a horseshoe, with the ordering of the ten parties on this horseshoe corresponding more or less to their left to right ordering in Table 2. The ordinal positions of adjacent parties in the ordering were often reversed, though, particularly between AI and R and between DMC and CRM. Because no systematic differences among the three group configurations were discovered, all the 52 proximity matrices were jointly subjected to the ALSCAL program. As before, the weighted Euclidean model for individual differences was applied. The resulting two-dimensional solution, also shaped like the same horseshoe, is depicted in Figure 2. Based on several arguments, which are presented elsewhere (Golan, 1984), it was concluded that the solution in Figure 2 (as well as most individual solutions which have a similar shape) is politically interpretable. The horizontal axis may be interpreted in terms of the joint and highly correlated effects of three dimensions, namely, defense policy, economic ideology, and attitude towards religion. The vertical axis is interpreted as a moderation-radicalism dimension, with the more moderate parties placed at the top of the configuration and the more aggressive, traditionally anti-establishment parties placed at the bottom.

Model Testing

In addition to the vector of perceived power w , which was derived from the subject's power ratio judgments, six theoretical vectors were computed. The vectors ϕ , β , and ρ are the predic-

tions of the SS, BZ, and DP power indices, respectively. As these indices only consider the seat distribution, they do not allow for individual differences. The other three vectors are ϕ^g , β^g , and ρ^g . As they also consider the political space, which differs in general from one subject to another, the generalized indices each yield different predictions for different subjects.

Table 6 presents the predicted vectors ϕ , β , and ρ for the tenth Knesset. When compared to each other, the two vectors ϕ and β are seen to be quite similar; the difference $|\phi_i - \beta_i|$ never exceeds 3.5%. The Banzhaf index, though, yields more egalitarian predictions than the Shapley-Shubik index: $\beta_i < \phi_i$ for the two dominant parties LP and U, and $\beta_i > \phi_i$ for the remaining eight, smaller parties. Both these power indices differ radically from the DP index, which predicts more egalitarian results than both. Political considerations would tend to reject the DP offhand, as it prescribes higher voting power to each of the two religious parties NRP and AI and even to the Communist party DFP than to the dominant party U! Clearly the assumptions underlying the DP index do not apply to the tenth Knesset.

Table 7 displays the means and standard deviations of the voting power prescribed by the generalized indices GSS, GBZ, and GDP. Attending first to the GBZ and GDP indices, Table 7 shows relatively small standard deviations of predicted power for each of the ten parties. The ratio of mean to standard deviation for each of these two generalized indices is about 20, indicating very tight frequency distributions of power with small between-subject variability. A comparison of the mean vectors β^g and ρ^g in Table 7 with their respective vectors β and ρ in Table 6 shows only small discrepancies. The maximal difference between β_i^g and β_i is 2% (party LP) and the maximal difference between ρ_i^g and ρ_i is 1.7% (party DFP). We conclude from this comparison and the relatively small standard deviations of predicted power that the generalizations of the two power indices BZ and DP had only a marginal effect.

The generalization of the SS index, however, had more pronounced effects. Table 7 shows relatively large standard deviations for the GSS, which exceed the respective means of most of the parties, and a comparison of Tables 7 and 6 also shows marked differences, with the GSS index assigning more power than the SS index to the right-of-the-center parties U, MIT, NRP, and AI and less power to the remaining parties. On the average, the GSS index concentrated most of the voting power (78.7%) in the hands of four parties which, only a short time after the study, actually

Figure 2. A Two-Dimensional Representation of the Ten Parties Yielded by the INDSCAL Program

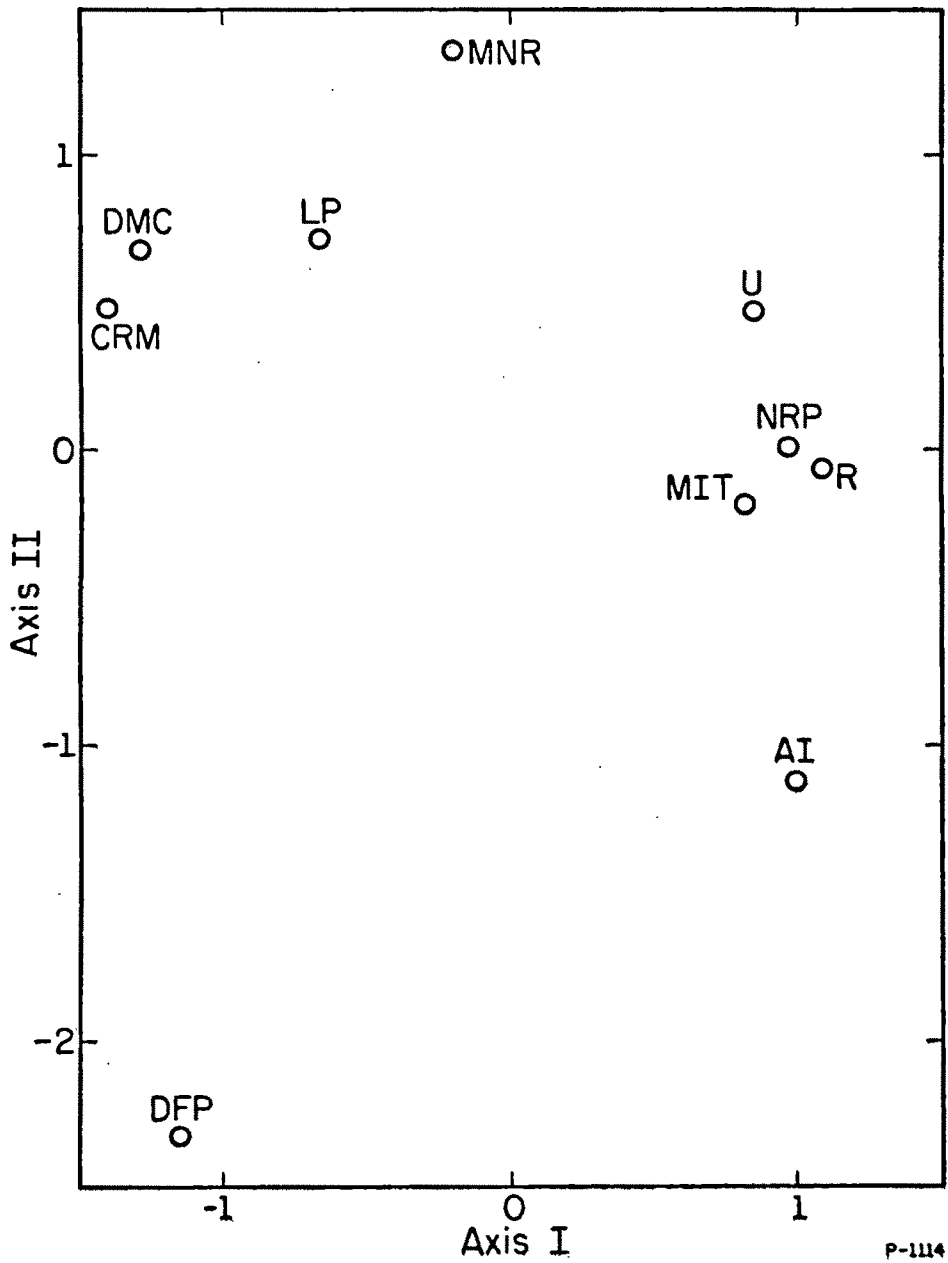


Table 6. Predicted Voting Power by the Shapley-Shubik, Banzhaf, and Deegan-Packel Power Indices

Party	SS Value ϕ	BZ Value β	DP Value ρ
DFP	0.070	0.080	0.106
DMC	0.031	0.040	0.091
CRM	0.016	0.019	0.062
LP	0.264	0.229	0.105
MNR	0.031	0.037	0.091
U	0.300	0.268	0.104
MIT	0.050	0.058	0.103
NRP	0.118	0.134	0.129
AI	0.070	0.080	0.106
R	0.050	0.058	0.103
Total	1.000	1.003	1.000

joined forces to form a minimal winning coalition.

Predicted and observed measures of perceived power were next compared to each other by computing for each subject and each power index the root mean squared deviation

$$\sqrt{\sum_{i=1}^n (z_i - w_i)^2},$$

where $z = z_1, \dots, z_n$ indicates a theoretical vector. The resulting deviation scores, six for each subject, were first used to determine whether the generalizations of the SS, BZ, and DP indices improved the goodness-of-fit. For 49 of the 52 subjects the SS index yielded smaller deviation scores than GSS. A comparison of BZ and GBZ, however, resulted in smaller deviation scores associated with the GBZ index for 39 of the 52 subjects, and in the case of the DP index, the generalization improved the fit in 51 of 52 cases. We next marked for each subject the power index with the smallest deviation score and compared

the best models across all the subjects. The GBZ index provided the best fit for 32 of the 52 subjects (61.5%). Figure 3, which plots the mean perceived power for all subjects in Table 4 against the mean predicted power by the GBZ index in Table 7, provides a general idea of how good the fit is. The results of 10 more subjects (30.8%) were best accounted for by the SS index. The two generalized indices GSS and GDP were the most appropriate models for only one and two subjects, respectively.

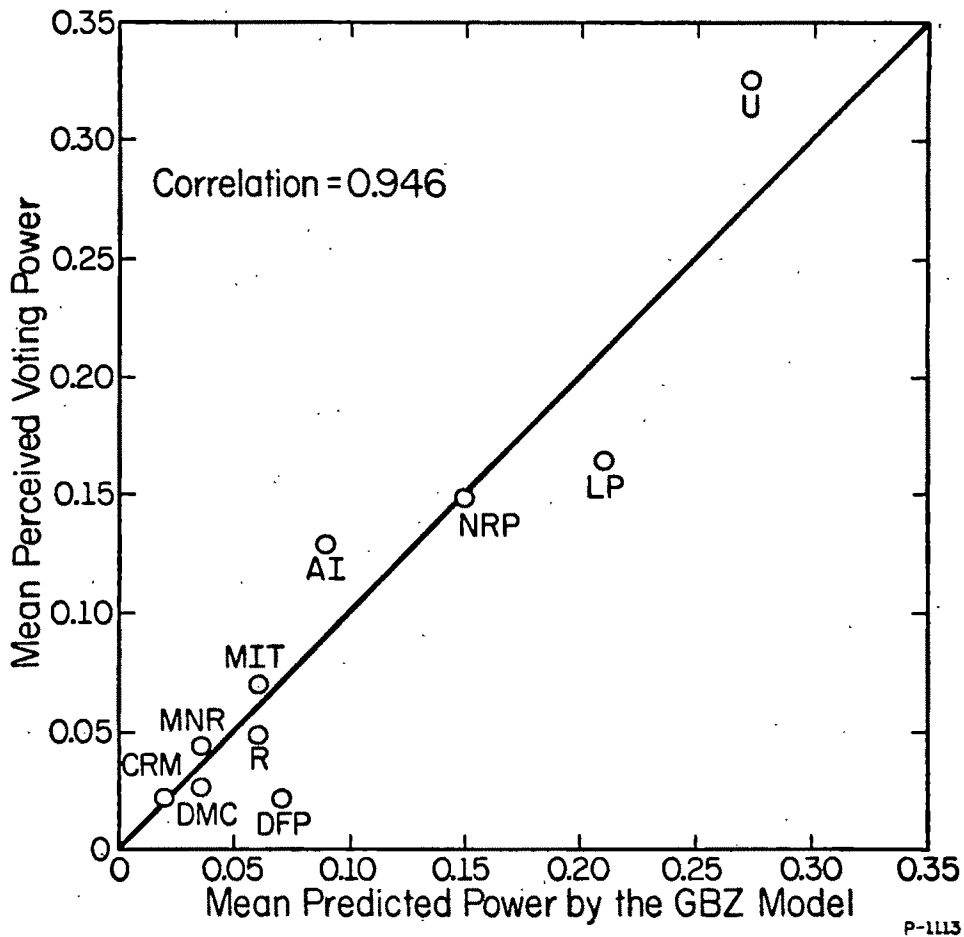
Discussion

Political power, like beauty, is in the eye of the beholder. It depends on, but is probably not solely determined by, the distribution of parliamentary seats, the ideological space, the personalities involved, party cohesiveness, resources controlled by the party, or the history of conflict and cooperation among the parties involving promises, threats, pressures, horsetrading, betrayal, and

Table 7. Means and Standard Deviations of Predicted Voting Power by the Generalized Power Indices

Party	GSS		GBZ		GDP	
	Mean	SD	Mean	SD	Mean	SD
DFP	0.008	0.019	0.071	0.003	0.089	0.007
DMC	0.010	0.018	0.036	0.002	0.085	0.005
CRM	0.007	0.014	0.019	0.001	0.056	0.003
LP	0.123	0.087	0.209	0.011	0.102	0.006
MNR	0.015	0.033	0.036	0.002	0.090	0.004
U	0.421	0.246	0.272	0.011	0.111	0.006
MIT	0.054	0.099	0.060	0.003	0.109	0.004
NRP	0.165	0.201	0.148	0.007	0.139	0.005
AI	0.147	0.104	0.088	0.004	0.111	0.006
R	0.049	0.080	0.060	0.003	0.108	0.004
Total	0.999		0.999		1.000	

Figure 3. Mean Perceived Power Plotted against the Mean Predicted Power by the GBZ Power Index



loyalty. Although power is inherently a multi-dimensional attribute, an implicit assumption of the present study is that individuals can and do integrate some or all of these facets, albeit in different ways, to form a unidimensional scale of political power. Tests of scale reliability, which have not been undertaken in the present study, are required to determine the stability of the integrating function for a given individual over time. The six power indices examined in the present study might be considered, then, as preliminary attempts to model this integrating process. There may be two major reasons for the failure of any of these models to account for perceived political power. The person may not use exactly the same factor or factors that the model postulates, or he or she may use the same factors as postulated but combine them in a different way.

The results support the underlying assumption of the six models that perceived political power is

measurable on a ratio scale. Depending on the criterion used to determine deviations from consistency, most of our subjects exhibited an acceptable level of imprecision in their power ratio judgments, which has only a small effect on the derived weights. The analytic hierarchy process was developed by Saaty for the derivation of priorities or weights which reflect the relative importance of the options in a multi-attribute judgment task. There are several advantages to this technique: it is based on pairwise comparisons, which subjects generally find easy to make; it shares with Thurstonian psychophysical scaling technique and signal detectability theory the indirect approach to the analysis of sensory experience and other subjective phenomena; and it yields a numerical measure of response consistency. But the technique also has its shortcomings. The null hypothesis which it rejects in measuring consistency is too weak; there is a need

for statistically defined or justified measures of response uncertainty based on stronger assumptions about the null hypothesis than sheer randomness. It also appears (Belton & Gear, 1983) that under certain circumstances the technique may yield anomalous results arising from a misunderstanding of what is required in specifying the outputs. Because of these shortcomings, additional studies are required, which apply concurrently several scaling techniques for the measurement of voting power.

Another finding was that as the political sophistication of the subjects grew, less power was jointly attributed to the two largest parties U and LP, and more power was assigned to the three religious parties MIT, NRP, and AI. The loss of power inflicted on the two dominant parties was approximately equal to the joint gain of power by the religious parties. The mean combined power of U and LP decreased significantly from 0.525 for group S to 0.401 for group R, a drop of 12.4%, compared to an increase of 14.3% in the mean combined power of the three religious parties from 0.308 for group S to 0.451 for group R. We attribute these two compensatory effects to the political experience of the subjects, their historical orientation, and their understanding of parliamentary politics. More research is required to find out whether the effects of political sophistication or experience are restricted to the three populations of subjects examined in the present study or are more general and apply to other segments of the Israeli population.

It was pointed out above that the power indices SS, BZ, and DP share a simple, experimentally testable property, namely, parties with equal numbers of seats are prescribed equal power. The observed results are inconsistent with this prediction. Table 2 shows three pairs of parties with the same number of seats: DFP and AI, MIT and R, and DMC and MNR. But Table 4 shows that for each group separately and over all groups, the AI was attributed between four to six times more power than DFP. Similarly, the mean perceived power of MIT exceeded the mean perceived power of R by about 50%, and the mean perceived power of MNR was almost twice as large as that of DMC. Also noted is the difference in mean perceived power between U and LP, which far exceeds the one seat difference between these two parties. All of these comparisons show that within each pair more power was attributed to the party believed to be the more likely candidate to enter a coalition. They strongly suggest that seat distribution alone cannot account for perceived power and that additional factors, which determine the likelihood of entering a winning coalition, must be considered.

It was, therefore, natural to test the three gener-

alized power indices GSS, GBZ, and GDP, which take into consideration both the seat distribution and the ideological similarities between the parties but combine them in different ways. When compared to the indices SS, BZ, and DP, the generalized indices behaved as expected, prescribing more power to the right-of-center parties and less power to the remaining parties. The DP index and its generalized version GDP are clearly rejected as descriptive models, probably because the assumption of equal division of spoils is untenable. The GSS and GBZ indices "move" in the same direction as the observed results. But although the direction of movement is right, its magnitude is wrong, with the GSS attributing too much power to each of the five right-of-center parties (except MNR), compared with the mean perceived power, and the GBZ index assigning each of them too little power.

Of the different approaches that may be considered in revising the GSS and GBZ indices, one warrants special attention. It may be recalled that both of these indices incorporate the assumption that all issue directions in the ideological space are equally likely. Despite its obvious advantages of simplicity and mathematical tractability, this assumption makes very little political sense. Inspection of voting records in the Knesset shows that the political winds do not blow across the ideological space in a perfectly random way. Rather, the directional distribution of political issues introduced to the Knesset is largely, although not exclusively, determined by the coalition and tends to concentrate around issues which are of major interest to the government. An empirical classification of issues actually voted on by the parliament may help to estimate the distribution of issues in R^m , which would then replace the uniform distribution in the computation of the two generalized power indices.

Appendix: Saaty's Ratio Scaling Procedure

Because most social scientists are unlikely to be familiar with Saaty's Analytic Hierarchy Process, a brief discussion is in order. Suppose we wish to compare a set of n objects or activities in pairs according to their relative weights (assumed to belong to a ratio scale). The weights may designate levels of different attributes or dimensions such as importance, desirability, brightness, pleasantness, or power. Denote the objects (political parties in our study) by C_1, C_2, \dots, C_n , and their relative weights (political power) by w_1, w_2, \dots, w_n . The $n \times n$ pairwise comparisons between the n objects may be represented by a matrix A of order n , known as a reciprocal matrix, in which $a_{ij} = w_i/w_j$ denotes the ratio of

the weights of objects C_i and C_j , $a_{ji} = w_j/w_i$ is enforced, and 1's are placed along the main diagonal. Thus, only the $n(n-1)/2$ pairwise comparisons a_{ij} are required to construct A .

Suppose A is known but $w = (w_1, \dots, w_n)$ is not, and we wish to recover w . To do so, we have to solve the system of equations (in matrix form) $(A - nI)w = 0$ in the n unknowns w_1, w_2, \dots, w_n , where I is the identity matrix and 0 is a (column) vector of 0 's. This system has a nonzero solution if and only if n is an eigenvalue (characteristic root) of A . Moreover, all the n eigenvalues λ_i , $i = 1, \dots, n$, of A are zero, except one, called λ_{\max} , which is equal to n . The solution w is any column of A . The various solutions differ from one another by a multiplicative constant. However, it is desirable to have the solution normalized so that $w_1 + w_2 + \dots + w_n = 1$.

When the matrix A is generated from the known weights (without error), it satisfies the property

$$a_{ik} = a_{ij} \times a_{jk}, \text{ for all } i, j, k = 1, \dots, n.$$

A matrix A that satisfies this property is called *consistent*.

In realistic situations the ratio judgments a_{ij} will be inconsistent to some degree. One subject may judge $a_{ij} = 2$, $a_{jk} = 3$, and $a_{ik} = 5$, whereas another may respond with $a_{ij} = 2$, $a_{jk} = 3$, and $a_{ik} = 4$. We say that the second subject is less consistent than the first. We wish to account for the inconsistency in ratio judgments by deriving from A a single numerical index of the degree of inconsistency in the subject's ratio judgments. Mathematically, this is achieved through the solution of the system of n equations

$$A w = \lambda_{\max} w, \quad (A1)$$

where both the scalar λ_{\max} —the maximal eigenvalue of A —and the corresponding eigenvector w are unknown. It can be shown that a reciprocal matrix with positive entries is consistent if and only if $\lambda_{\max} = n$. With inconsistency, $\lambda_{\max} > n$ always. Saaty has observed that in any square matrix, small perturbations in its coefficients imply small perturbations in the eigenvalues. Hence, the higher the degree of inconsistency in the pairwise comparisons, the higher the difference $\lambda_{\max} - n$.

This observation leads to the next and final step of the scaling procedure, which determines whether the degree of inconsistency observed in the $n(n-1)/2$ pairwise comparisons of the subject is sufficiently small to justify the derivation of a ratio scale. No analytical solution to this ill-defined problem is known, but two different answers have been proposed by Saaty. The first is

based on equating inconsistency with randomness. Reverting to Monte Carlo techniques, Saaty (1980, p. 61) computed the sampling distribution of λ_{\max} from 500 randomly generated matrices. As in the present study, each matrix was of order 10×10 with 1s in the main diagonal entries and with reciprocity enforced. The entries were filled in at random from the scale 1 to 9 (integers only), which is very close to the response scale actually adopted by the subjects of the present study (see below). Saaty reported a truncated normal frequency distribution for λ_{\max} . For a 1 to 9 response scale and significance level of 0.01, the critical level for rejecting the null hypothesis (by a one-tailed test) is 19.66. The decision rule of this approach is to reject the matrix A of pairwise ratio judgments as inconsistent if $\lambda_{\max} > 19.66$ and accept it otherwise. If $\lambda_{\max} < 19.66$, the associated eigenvector—the vector of weights w —is computed from Eq. (A1).

Saaty's second answer is to compute the consistency index

$$\mu = (\lambda_{\max} - n)/(n-1), \quad (A2)$$

and to reject A as inconsistent if $\mu > \mu_{\text{crit}}$ and accept it otherwise. There is presently no statistical theory that underlies the measure μ . Based on his rich and versatile experience with the procedure, Saaty proposed to fix μ_{crit} at 0.1 (1977, 1980) or 0.2 (1983).

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Multiparty Equilibria under Proportional Representation

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The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation, as well as partially to characterize it. Specifically, let m be the quota that represents the minimal number of voters necessary for a candidate to be elected. We show that there is a set of elected candidates, each choosing an alternative and each receiving at least m votes, such that no other potential candidate, by offering an additional alternative, can secure at least m votes for himself. We then investigate the structure, at equilibrium, of the set of individuals who support a given candidate, as well as study stability properties of the equilibrium. We also provide necessary and sufficient conditions for the equilibrium to consist of a single candidate, thus generalizing Black's median-voter result.

The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation, as well as partially to characterize it. More specifically, we show that whenever the alternative set is finite and the preference relations of the voters are complete, transitive and single peaked, for any society and any minimum number, m , of votes required for a seat, there exists an equilibrium; that is, there is a set of candidates each choosing an alternative and each receiving at least m votes, and moreover, no other potential candidate can secure m votes for himself. In the special case when m is chosen to be greater than half the size of the population, our result yields the existence of a simple majority equilibrium.

In addition, we provide necessary and sufficient conditions for the equilibrium to consist of a single candidate. This result generalizes Black's median voter. For the more general case, when the equilibrium consists of at least two candidates, we prove that there always exists an equilibrium in which there are two candidates whose chosen

positions coincide with the peaks of two individuals. (We fully characterize these two individuals.)

We also investigate the structure, at equilibrium, of the set of individuals who support a given candidate. First, by means of an example we show that this set need not be consecutive, that is, it is possible that individuals i and k will support candidate a , whereas individual j , whose peak lies between the peaks of i and k , will vote for candidate b . This example also illustrates Black's point that indeed, in contrast to the majority rule where only the peaks of the individuals' preferences matter (the well-known median-voter result), under proportional representation the equilibrium depends on the entire preference relations. We then consider some special cases in which, at equilibrium, the support of each candidate is consecutive. Such is the situation when, for example, each individual's preference relation can be represented by the distance from his peak.

The possibility that, at equilibrium, the support of candidates need not be consecutive might seem, at first sight, somewhat surprising. This phenomenon is, however, quite commonly observed. Indeed, among the individuals who support a conservative party, there might well be voters who are more liberal than some of the supporters of the liberal party. The intuitive reason for this lack of consecutiveness is as follows: All that the phrase "individual i is more conservative than individual j " means is that if the individual could dictate his *most preferred* position (ideology), then the one that would be chosen by i is more conservative than the one chosen by j . However, when the choice is between two other positions (the parties' ideologies), there is no reason for the above-mentioned phenomenon not to exist. To use an

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analogy, consider two individuals who have a preference ordering over the amount of sugar they wish to have in their tea. Assume that individual *i* likes his tea *best* with one spoon of sugar, while individual *j* likes his best with two spoons. It is, nevertheless, conceivable that when faced with the choice between having tea with no sugar at all or with five spoons of sugar, individual *i* prefers the latter alternative, while individual *j* prefers the former. We illustrate below that there might even be situations in which in *all* equilibria, *all* candidates have a *nonconsecutive* support. It is mainly due to this possibility that the proofs of our results are mathematically rather involved.

Because candidates might well have motives and ambitions to increase the set of their voters, it is interesting to investigate the stability properties of our proposed notion of equilibrium under this alternative assumption, which differs from the one originally imposed (that candidates are equally content with any number of votes above *m*). We therefore define an equilibrium to be stable if there does not exist an elected candidate (i.e., one who currently receives at least *m* votes), who, by deviating from it, is certain to increase the number of his supporters in any new equilibrium that might result. We prove that although not all equilibria are stable, there do exist stable equilibria.

The article is organized as follows. We discuss first the background of the problem, then we present the model and state the main results. The stability analysis and the discussion of "true" or "sincere" versus "manipulative" or "policy oriented" voting follows. The example in the sixth section demonstrates that there are situations in which, at each equilibrium, the support of no candidate is consecutive. Some basic observations on the nature of the equilibrium are given in the seventh section and are then used in the proofs of the results in the concluding sections. Although mathematically quite elementary, these proofs are nevertheless somewhat involved. This is particularly true for the proof of the main result of the study, namely, the existence of an equilibrium under proportional representation.

Background

Proportional representation in some form is now used in most democracies as the only method of allocating seats for the legislative bodies, municipalities, unions, and other public organizations. Inquiry in regard to scientific methods of electing representative bodies began as early as the latter part of the eighteenth century. (The earliest statement of the proportional ideal apparently was made by Mirabeau in a speech before the Assembly of Provence on January 30, 1789. The

earliest published proposal of a proportional system was made by Gergonne in 1820.) Many political philosophers and statesmen advocated its use, believing that

The only civilized method of democracy is proportional representation in large constituencies returning many members; there is no other method which gives the individual voter a reasonable opportunity of expressing his real preference. It is the right way, and all other ways are wrong and bad. (Wells, 1923)

Multiparty systems—those with three or more major parties—are likely to occur under proportional representation. It is therefore somewhat surprising that in contrast to the vast quantity of literature on the existence and characterization of a (simple) majority equilibrium, where a single party forms the government, very little work has been done on stable multiparty configurations. One possible explanation for this lacuna in the literature might be the difficulties involved in the formal analysis of the proportional representation model, where the solution concept is a *set* of (elected) candidates, rather than a single one, as in the case of majority rule. As noted by Black (1958, p. 76):

When we were concerned with the election of a single member to represent a constituency, the problem was to find a candidate who stood highest on the average on the preference scales of the voters—and this permitted of several answers being given. In dealing with the statics of P.R. we have the same sort of difficulty in a more acute form; and this is enough to show that no answer that is given could be completely satisfactory.

The proportional representation scheme is also addressed in the classical work of Downs (1957, p. 124), who writes:

Where proportional representation exists, a party which wins only a small percentage of the total vote may place some of its members in the government, since coalition governments often rule. Thus the minimum amount of support necessary to keep a party going is much smaller than in a plurality system; so a multiparty system is encouraged. Nevertheless, each party must still obtain a certain minimum number of votes in order to elect members of the legislature who might possibly enter a coalition. For this reason a given distribution of voters can support only a limited number of parties even under proportional representation. Therefore the conditions for equilibrium exist in both two and multiparty systems.

Downs does not, however, provide a formal proof

of the existence of such an equilibrium, but argues intuitively:

There is a limit to the number of parties which can be supported by any one distribution. When that limit is reached, no more new parties can be successfully introduced. The parties extant at that point arrange themselves through competition so that no party can gain more votes by moving to the right than it loses on the left by doing so, and vice versa. The political system thus reaches a state of long-run equilibrium in so far as the number and positions of its parties are concerned, assuming no change in the distribution of voters along the scale." (p. 123)

Thus, all that Downs proves is that *if* there exists an equilibrium, it can support at most an *a priori* given number of parties, that is, that "parties cannot form ad infinitum" (p. 124, footnote 8). But Downs does not settle the existence issue which, as Black notes, is by no means trivial. As Black (1958, p. 8) states:

The scheme of P.R. which we have outlined is too primitive mathematically to be satisfactory—it depends only on the position of the peaks of the curves concerned and does not take into account the entire constellation of the curves. In this respect it contravenes one of the requirements which we have considered necessary in a solution. Our purpose in considering it, indeed, has been to illustrate the difficulties of getting any satisfactory scheme of P.R. of a mathematical nature, even in the simplest of cases.

The purpose of this work is to define formally and to analyze the notion of an equilibrium under proportional representation. (To the best of our knowledge, no rigorous treatment of multiparty equilibria has been offered before.) There are, however, many methods of securing proportionality that can be and have been used. (For example, see Hoag & Hallett, 1926, pp. 412-456.) The main difference among the various systems of proportional representation is in the way the remainder of the vote is allocated. For this work we chose to analyze the Uniform Quota system, which was used in Germany during the Weimar Republic, and is similar in principle to d'Hondt rule, which is currently used for parliamentary elections in several European countries and in Israel.

The Uniform Quota

Hoag and Hallett (1926, pp. 412-414) describe and discuss the uniform quota method as follows:

The simplest method of apportioning the seats to the lists is that used for the election of the Reichstag under the new republican constitution

of Germany. An arbitrary quota is fixed for all districts. In German Reichstag elections it is fixed permanently at 60,000. In each district each party is assigned as many seats as there are quotas in its vote.

The "uniform quota" method has some obvious advantages. It tends to interest all elements in securing full participation in elections, since the representation of each district as well as of each point of view within each district depends entirely on the number of quotas of votes it polls. It puts elected members from all parties and all parts of the country on an equal footing, since the election quota is everywhere the same. And it gives every voter the unique privilege of not helping to secure additional representation for his district if he cannot secure the kind of representation he wants. If applied to congressional elections in our southern states, where only Democrats are now elected, for example, it would relieve the Republicans who are not allowed to vote of the necessity of being counted to give additional seats to their states in the apportionment and thus really of providing more seats for the Democrats. In states like North Carolina, where considerable numbers of Republicans vote, it would of course—like any plan of proportional representation—enable them to elect members of their own.

The main objection to the uniform quota principle is that it causes fluctuations not only in the size of the district delegation but in the size of the whole body elected. And these fluctuations may depend not only on fluctuations in interest but on such fortuitous circumstances as whether it rains on election day. Furthermore, if it rains in some parts of the country but not in others, the former may be relatively underrepresented. Such considerations may be of minor importance, but nearly all proportional representation countries have adhered to the long-established practice of assigning a definite number of members to each district in advance.

Another example in which the quota is fixed in advance instead of the number of members to be elected is the Israeli law for candidates running for the Knesset (the Israeli parliament). According to that law, any person who wishes to run for the Knesset has to produce a given number (quota) of signatures of citizens who are eligible to vote and who support his candidacy. In the recent 1984 elections, this quota was 5,000 signatures.

Although we know of no other examples in which the uniform quota is, or was, used, we wish to point out that the appointing method devised by the Belgian mathematician d'Hondt is very closely related to the uniform quota. As Hoag and Hallett note (1926, pp. 421-422):

In any of its forms the d'Hondt method is similar in principle to the uniform quota method. It gives a uniform quota assignment for each dis-

strict to which it is separately applied, the only difference being that the quota is determined so as to give a desired total membership instead of being fixed arbitrarily.

In Europe it appears to be generally considered the fairest of the methods of apportionment used for list system elections for a definite number of members, as is evidenced by its widespread use. It is the most strictly proportional method possible for such elections in the somewhat restricted sense that if any list wins a seat for a given number of votes, every other list is sure to win at least as many seats as the number of times its vote contains that number. No other method used or proposed for such elections satisfies that criterion.

(The one objection frequently urged against the d'Hondt principle is that it is likely to give a large party a fraction of a seat more than its exact proportion and a small party a fraction of a seat less, thus tending to discourage independent groups from nominating separate lists.)

Finally, it is interesting to note that the first published proposal of proportional representation apparently advocated the uniform quota system. In 1820 the French mathematician Geronne published in the *Annales de Mathematiques* an article entitled "Arithmetique politique. Sur les elections et le systeme representatif," in which he said: "At the elections the voters would group themselves freely according to their opinions, their interests, or their desires, and any citizen would become a Deputy from the department in the elective chamber who bore a mandate from two hundred voters."

The Model

Society is represented by the finite set $N = \{1, 2, \dots, n\}$ of n voters. Voter $i \in N$ has a preference relation, denoted by \geq_i , over a finite set of alternatives, Ω , where for every $a, b, \in \Omega$, $a \geq_i b$ is interpreted to mean that voter i (weakly) prefers a to b , and $a >_i b$ means that voter i strictly prefers a to b . Since Ω is finite, the alternatives in Ω can be ordered (assigned numbers) so that for every two alternatives $a, b \in \Omega$ either $a > b$ or $b > a$. Given this ordering of the alternatives in Ω , we assume that the preference relation of each individual $i \in N$ is:

ASSUMPTION 1. Complete. That is, for every two alternatives $a, b \in \Omega$, either $a >_i b$ or $b >_i a$.

ASSUMPTION 2. Transitive. That is, for every three alternatives $a, b, c, \in \Omega$: If $a >_i b$, and $b >_i c$, then $a >_i c$.

ASSUMPTION 3. Single peaked. That is, for each $i \in N$, there exists a unique alternative $q(i) \in \Omega$,

called i 's peak, such that for every two alternatives $a, b \in \Omega$, if either $q(i) > a > b$ or $q(i) < a < b$, then $a >_i b$.

Without loss of generality, let the voters be numbered (named), so that $q(1) \leq q(2) \leq \dots \leq q(n)$. That is, individual 1's most preferred alternative is to the left of that of Mr. 2's which is, in turn, to the left of Mr. 3's, and so on. The rightmost peak is that of Mr. n .

The exogenously given positive integer m , $m \leq n$, represents the least amount of votes required for one seat in the parliament. Given society N , the set Ω and the number m , we define an m -equilibrium as a set of alternatives such that when the voters are faced with the choice among these alternatives, and when each voter votes for the alternative he prefers best in this set, each alternative receives at least m votes and, moreover, no new (potential) candidate can attract m voters by offering another alternative, in addition to those offered in the m -equilibrium.

In order to define formally an m -equilibrium we introduce the following notation and definitions which will be used throughout the article:

$$N = \{1, 2, \dots, n\},$$

$$A = \{a_1, \dots, a_T\} \text{ where } a_i \in \Omega$$

$$\text{for all } i = 1, \dots, T, a_1 < a_2 < \dots < a_T$$

$$m = \text{a positive integer not exceeding } n.$$

DEFINITION 1. The support of alternative $a \in \Omega$ given N and the subset of alternatives A , denoted $S(a; N, A)$, is the set of all individuals who strictly prefer alternative a over any other alternative in A . That is, $S(a; N, A) = \{i \in N | a >_i a_t \text{ for all } a_t \in A, a_t \neq a\}$. The cardinality of $S(a; N, A)$ is denoted $s(a; N, A)$.

DEFINITION 2. The support $S(a; N, A)$ is called consecutive if for any three individuals i, j, k with $i < j < k$, $i, k \in S(a; N, A)$ implies that $j \in S(a; N, A)$.

That is, $S(a; N, A)$ is consecutive if whenever Mr. i and Mr. k prefer alternative a to any other alternative in A , so will all other individuals whose peaks lie between those of individuals i and k . (As we shall see below, perhaps somewhat unexpectedly, the support need not be consecutive.)

Using the above terminology we have that the collection A is an m -equilibrium for N if the support of each alternative in A consists of at least m individuals (thus, each elected candidate receives at least m votes), whereas no other alternative is supported (given A) by m or more voters; thus, no potential candidate can guarantee himself a seat. Formally,

DEFINITION 3. A set of alternatives A is called an *m-equilibrium* for N (or, simply, an *m-equilibrium*) if: for all $a \in \Omega$, $s(a; N, A) \geq m$ if and only if $a \in A$.

The set of all *m-equilibria* for N is denoted by $E(N, m)$.

DEFINITION 4. A set A is called a *consecutive m-equilibrium* for N (or, simply a *consecutive m-equilibrium*), if $A \in E(N, m)$ and for every $a \in A$, $S(a; N, A)$ is consecutive.

The set of all consecutive *m-equilibria* for N is denoted by $E_c(N, m)$.

We can now state our main results. Let N be any finite society, which consists of n individuals, let Ω be any finite set of alternatives, and let m be any positive integer not exceeding n . Moreover, we shall assume throughout that Assumptions 1-3 hold.

The first result states that there are cases in which an *m-equilibrium* exists but is not consecutive. It is primarily this possibility that makes the proofs of our results rather involved. Formally,

Result 1. It is possible that there exists no consecutive *m-equilibrium*, that is, $E_c(N, m)$ might be empty, and moreover, that for any $A \in E(N, m)$ and any $a \in A$ the support $S(a; N, A)$ is not consecutive.

The second result is the main conclusion of the article, namely, that an *m-equilibrium* always exists.

Result 2. For any m , $0 < m \leq n$, there exists an *m-equilibrium*.

The next three results serve as auxiliary lemmata for the proof of Result 2, but they are of independent interest as they provide a partial characterization of the *m-equilibria*. Result 3 describes fully the set of *m-equilibria* in the special case in which it consists of a single element (i.e., one candidate wins the election). In order that one alternative constitute an *m-equilibrium* the peak of individual m must lie to the right of that of individual $n - m + 1$. Moreover, any alternative between (and including) these two peaks constitutes an *m-equilibrium*. Formally,

Result 3. The following three statements are equivalent:

- (i) There exists an *m-equilibrium* which consists of a single alternative;
- (ii) $q(m) \geq q(n - m + 1)$;

- (iii) For every α , $q(m) \geq \alpha \geq q(n - m + 1)$, $\{\alpha\}$ is an *m-equilibrium*.

Note that Result 3 generalizes Black's median voter result for the (strict) majority rule. Indeed, if the quota for a seat exceeds half the population, then $m > n/2$, implying that $m \geq n - m + 1$ and hence $q(m) \geq q(n - m + 1)$. By (iii), the median voter's peak is a majority equilibrium. Observe also that (i) does not exclude the possibility that there are *m-equilibria* that consist of two alternatives, even when $q(m) \geq q(n - m + 1)$.

Consider an individual that joins a society N which has an *m-equilibrium* that consists of a single alternative. Suppose that the peak of that individual is (weakly) to the right of all the peaks of the individuals in N . Then, Result 4 asserts that either $q(m)$ or $\{q(m), q(n - m + 2)\}$ constitutes an *m-equilibrium* for the enlarged society. If it is the latter, by (i) of Result 3 there is no peak that lies between $q(m)$ and $q(n - m + 2)$. Hence, in either case the *m-equilibrium* is consecutive. Formally,

Result 4. Let $\tilde{N} = \{1, 2, \dots, n+1\}$. If $q(m) \geq q(n - m + 1)$, then either there exists an *m-equilibrium* for \tilde{N} which consists of a single alternative or else $\{q(m), q(n - m + 2)\}$ is an *m-equilibrium* for \tilde{N} . Hence, the set of consecutive *m-equilibria* for \tilde{N} is nonempty.

Note that Result 4 does not rule out the existence of a nonconsecutive *m-equilibrium* for \tilde{N} .

Result 5 gives a partial characterization of the set of *m-equilibria* that consist of more than a single alternative. Specifically, in that case there always exists at least one *m-equilibrium* that contains the following two peaks: that of individual m and that of individual $n - m + 1$. Moreover, these alternatives are, respectively, the first and last elements of that *m-equilibrium*.

Result 5. If $q(m) < q(n - m + 1)$ then there exists an *m-equilibrium* $A = \{\alpha_1, \dots, \alpha_T\}$ with $\alpha_1 = q(m)$ and $\alpha_T = q(n - m + 1)$.

In the special case that disutility is determined by distance, we have:

Result 6. If the preference ordering of every individual can be represented by the distance of his peak to the alternative, i.e., for each $i \in N$ and every $a, b \in \Omega$, $a \succ_i b$ if and only if $|a - q(i)| < |b - q(i)|$, then there exists a consecutive *m-equilibrium*. Moreover, in this case every *m-equilibrium* is a consecutive *m-equilibrium*.

Discussion and Stability

The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation. In view of "the difficulties of getting any satisfactory scheme of proportional representation of a mathematical nature, even in the simplest of cases" (Black, 1958, p. 80), as is indeed evidenced by the complexity of the proofs of our results, it seems that Definition 3 and Results 1-6, in particular Result 2, have accomplished that goal (at least as a first step).

It might, therefore, be useful to pause and to examine the proposed equilibrium concept more closely, before proceeding with the proofs of the results. Recall that an m -equilibrium consists of a set of alternatives, each offered by an elected candidate, so that each receives at least m votes and there is no other candidate that can attract at least m voters by offering an additional alternative. There are two points that deserve, perhaps, an explanation or a defense.

The first point is that it is assumed that individuals vote sincerely and not manipulatively in the sense of policy-oriented voting; that is, each individual votes for that candidate who best represents his preferences, and voters do not ask about the effect of their votes on the ultimate policy choice of the government. As is well known, this criticism is justified provided that only some of the elected candidates form a coalitional government, and when the number of voters that support an elected candidate influences his probability to belong to the government. Even in that case our proposed notion of an equilibrium serves as a reference point from which voters might deviate. However, in order to circumvent this difficulty completely, assume that all of the elected candidates form a committee (or the cabinet) with each having one vote and decisions are made according to some majority rule. In that case, faced with a set of candidates that forms an m -equilibrium, each voter will indeed find sincere voting to be his optimal strategy. Moreover, as we shall see below, even when the elected candidates "are weighted" by the number of their constituents, it is still best for each voter, when faced with a set of alternatives that is a (stable) m -equilibrium, to vote candidly, namely, for the alternative that ranks highest, within this set, according to his true preference relation.

The second point is that candidates are assumed to be satisficers rather than maximizers; that is, all that a candidate wishes is to receive the quota of m votes, but he is not trying to maximize the number of his supporters. Indeed, even at an m -equilibrium, an elected candidate might, by offering an alternative that differs from the one he

offers at the equilibrium, increase the number of individuals who vote for him. In that sense, therefore, the equilibrium notion is not stable since elected candidates might wish to deviate from the positions they currently hold.

As candidates do indeed have motives and ambitions to increase the set of individuals that support them, we find this point worth pursuing. Therefore, rather than defend the satisficing criterion, we will define and establish the existence of a stable equilibrium, where no candidate can benefit by deviating from it.

Since at an m -equilibrium no potential candidate can receive m votes, nonelected candidates cannot upset it. It follows that the only possible source for the instability of the equilibrium is that an elected candidate might wish to deviate from the position he currently holds. Clearly, every elected candidate is aware that if he deviates, other candidates (elected as well as potential) can do the same. His deviation might cause a chain reaction which the elected candidate must take into consideration. Hence, an elected candidate will have an incentive to deviate from the equilibrium only if he is certain that no matter which new equilibrium will result from his deviation, his set of voters will increase. A stable equilibrium is one from which no candidate has an incentive to deviate.

More specifically, let $A = \{a_1, \dots, a_T\}$ be an m -equilibrium and assume that the elected candidate t considers offering an alternative $w \in \Omega$ instead of a_t . He realizes that such a move will bring about a new m -equilibrium, denoted by B , in which he will offer w . (The new equilibrium B might, for example, be $B = \{a_1, \dots, a_{t-1}, w, a_{t+1}, \dots, a_T\}$. In other words, in the spirit of a Nash equilibrium, an elected candidate who contemplates a deviation assumes that all the elected candidates will not change their required strategies in response to his deviation. But because our result is sufficiently general, no particular assumptions on the form of B need be imposed.) Therefore, offering w rather than a_t will benefit the elected candidate t only if the number of individuals who support him at B is greater than the number of his supporters at A . Formally,

DEFINITION 5. Let $A = \{a_1, \dots, a_T\}$ be an m -equilibrium. An elected candidate t induces B from A via w if B is the m -equilibrium that results from A when the elected candidate t offers alternative w rather than a_t , and $s(w; N, B) > s(a_t; N, A)$.

We shall say that B is induced from A if there exists an elected candidate t and an alternative w such that t induces B from A via w .

But candidate t , when inducing B from A , must

take into account that at B another candidate might contemplate the inducement of yet another new equilibrium, C , and so on. This chain reaction of a sequence of induced equilibria is formalized by:

DEFINITION 6. Let $A = \{a_1, \dots, a_J\}$ be an m -equilibrium. An elected candidate t generates B from A via w if there exists a finite sequence A_1, \dots, A_J with the following properties:

- (i) t induces A_1 from A via w .
- (ii) $A_J = B$.
- (iii) For all $j = 2, \dots, J$, A_j is induced from A_{j-1} .

Now, given an equilibrium A , candidate t will offer w instead of a_i only if in any equilibrium that might result from his deviation, that is, in every m -equilibrium that he generates, the number of his supporters is greater than the number of individuals that vote for him at the original equilibrium A . Formally,

DEFINITION 7. Let $A = \{a_1, \dots, a_T\}$ be an m -equilibrium. An elected candidate t has an *incentive to deviate* from A via w if in all B that t generates from A via w , the number of individuals who vote for him is greater than $s(a_i; N, A)$.

Finally,

DEFINITION 8. An m -equilibrium A is *stable* if no elected candidate has an incentive to deviate from it via some w in Ω .

With the above definitions we can now formally state our two results on stable equilibria.

Result 7. Not every m -equilibrium is stable.

Result 8. For any $m \leq n$ there exists a stable m -equilibrium. Moreover, Results 1-6 are also true when restricted to the set of stable m -equilibria.

Proof of Result 1

It is sufficient to consider the following example:

$N = \{1, 2, \dots, 8\}$ (i.e., $n = 8$)
 $\Omega = \{a, b, c, d\}$ where $a < b < c < d$
 $m = 3$. The preferences of the individuals are:

$a >_i b >_i c >_i d$ for $i = 1, 2, 3$;
 $b >_i c >_i d >_i a$ for $i = 4$;
 $c >_i b >_i a >_i d$ for $i = 5$;

$d >_i c >_i b >_i a$ for $i = 6, 7, 8$.

It is easy to check that these preferences satisfy assumptions 1-3 and that individuals are named so that $q(1) = q(2) = q(3) < q(4) < q(5) < q(6) = q(7) = q(8)$. Now every m -equilibrium, if it exists, must contain alternatives a and d , since each of these two alternatives is always supported by at least three individuals, and $m = 3$. Moreover, because $n = 8$ and $m = 3$, no subset of Ω which contains more than two alternatives can be an m -equilibrium. Thus, the only candidate for an m -equilibrium is the set $A = \{a, d\}$. To conclude the proof of Result 1, it is sufficient to show that both $S(a; N, A)$ and $S(d; N, A)$ contain at least three individuals but neither set is consecutive. Indeed, since $a >_i d$ and $d >_i a$ we have that $S(a; N, A) = \{1, 2, 3, 5\}$ and $S(d; N, A) = \{4, 6, 7, 8\}$.

Q.E.D

Basic Observations

The following seven observations are straightforward and will be used in the proofs of the main results. They might be of independent interest and increase the familiarity with the concepts of m -equilibrium and the support of an alternative. Recall that A is an ordered subset of Ω (although not necessarily an m -equilibrium); that is, $A = \{a_1, \dots, a_T\}$ with $a_1 < a_2 < \dots < a_T$ and $a_i \in \Omega$ for all $i = 1, \dots, T$.

The first observation states that an individual i whose peak $q(i)$ is to the right of an alternative a_i in A will prefer a_i to any alternative that lies to its left. Thus, such an individual will never support a candidate who proposes an alternative $b < a_i$ if the set A is also offered to him. Formally,

OBSERVATION 1. Let $b \in \Omega$. If for some $a_i \in A$, $b < a_i$, then $a_i >_i b$ implies $i \notin S(b; N, A)$. In particular, if $a_i < q(m)$, then $s(b; N, A) < m$ for all $b < a_i$. Indeed, let $q(i) > a_i > b$. Then the single peakedness implies that $a_i >_i b$, thus $i \notin S(b; N, A)$. In particular, therefore, if $a_i < q(m)$ then $j \geq m$ implies that $j \notin S(b; N, A)$ for all $b < a_i$.

Observation 2 is the dual of Observation 1. It asserts that if individual i 's peak is to the left of an alternative $a_i \in A$, then he will not support any candidate that proposes an alternative $b > a_i$, whenever it is feasible for i to choose also from the set A . Formally,

OBSERVATION 2. Let $b \in \Omega$. If for some $a_i \in A$, $b > a_i$, then $q(i) < a_i$ implies $i \notin S(b; N, A)$. In particular if $a_T > q(n - m + 1)$, then $s(b; N, A) < m$ for all $b > a_T$. Indeed, let $b >$

$a_i \succ q(i)$. Then the single peakedness implies that $a_i \succ_i b$, thus $i \notin S(b; N, A)$. In particular, therefore, if $a_T \succ q(n - m + 1)$ then $j \leq n - m + 1$ implies that $j \notin S(b; N, A)$ for all $b \succ a_T$.

An immediate application of Observations 1 and 2 yields:

OBSERVATION 3. Let $b \in \Omega$ and $i \in S(b; N, A)$. Then, $q(i) < a_i$ for all $a_i \in A$, $a_i \succ b$, and $q(i) > a_i$ for all $a_i \in A$ that satisfies $a_i < b$.

OBSERVATION 4. $S(a_i; N, A) \supset \{i \in N | q(i) \leq a_i\}$ and $S(a_T; N, A) \supset \{i \in N | q(i) \geq a_T\}$. In particular, if $a_i = q(m)$, then $s(a_i; N, A) \geq m$, and if $a_T = q(n - m + 1)$, then $s(a_T; N, A) \geq m$.

Let a be an alternative in Ω that differs from individual i 's peak, $q(i)$. Then, any alternative that lies between $q(i)$ and a which differs from a will be preferred by individual i over a . It follows that if alternative a is individual i 's best choice among the elements in A , and b is an alternative that lies between $q(i)$ and a , $b \neq a$, then individual i 's best alternative in the set $A \cup \{b\}$ is alternative b . Formally,

OBSERVATION 5. If $i \in S(a; N, A)$ and $q(i) < a$, then $i \in S(b; N, A)$ for all b with $q(i) \leq b < a$. Indeed, let $q(i) \leq b < a$. Because $i \in S(a; N, A)$, $a \succ_i a_i$ for each $a_i \in A$, $a_i \neq a$. By the single peakedness, $b \succ_i a_i$, thus the transitivity implies that $b \succ_i a_i$ for each $a_i \in A$, that is, $i \in S(b; N, A)$.

Let a_b, a_{t+1}, b_h , and b_{h+1} be four alternatives in Ω that satisfy: $b_h \leq a_t < a_{t+1} \leq b_{h+1}$. Let $c \in \Omega$ be an alternative that lies strictly between a_t and a_{t+1} . Observation 6 asserts that if individual i prefers c to both a_t and a_{t+1} , then he will also prefer c to both b_h and b_{h+1} . Formally,

OBSERVATION 6. Let $B = \{b_1, \dots, b_H\}$ where $b_h \in \Omega$ for each $h = 1, 2, \dots, H$, and $b_1 < b_2 < \dots < b_H$. Assume that for some t and h , $b_h \leq a_t$ and $b_{h+1} \geq a_{t+1}$. Then for all c with $a_t < c < a_{t+1}$, $S(c; N, A) \subset S(c; N, B)$. In particular, if $b_h = a_t$ and $b_{h+1} = a_{t+1}$, $S(c; N, A) = S(c; N, B)$ for each c satisfying $a_t < c < a_{t+1}$. Indeed, let $a_t < c < a_{t+1}$ and let $i \in S(c; N, A)$. Then by Observation 3, $a_t < q(i) < a_{t+1}$. There are two possible cases: $a_t < c \leq q(i)$ and $a_t < q(i) \leq c < a_{t+1}$. Because $c \succ_i a_t$ and $c \succ_i a_{t+1}$ the single peakedness implies that in both cases $c \succ_i b_h$ and $c \succ_i b_{h+1}$. Thus, $i \in S(c; N, \{b_h, b_{h+1}\})$ which coincides, by Observation 3, with $S(c; N, B)$, i.e., $i \in S(c; N, B)$.

Observation 7 states that in any m -equilibrium the (first) leftmost alternative lies (weakly) to the left of individual m 's peak and the (last) rightmost alternative lies (weakly) to the right of $q(n - m + 1)$. That is,

OBSERVATION 7. Let $A \in E(N, m)$. Then, $a_1 \leq q(m)$ and $a_T \geq q(n - m + 1)$: Indeed, if $a_1 > q(m)$, then by Observation 4 $s(q(m); N, A) \geq m$, thus by Definition 3 $A \notin E(N, m)$. If $a_T < q(n - m + 1)$ then by Observation 4 $s(q(n - m + 1); N, A) \geq m$, thus by Definition 3, $A \notin E(N, m)$.

Proofs of Results 3 and 4

Given a society $N = \{1, 2, \dots, n\}$ we shall denote, throughout, $\bar{N} = \{1, 2, \dots, n + 1\}$, $= N \cup \{n + 1\}$ where $q(n + 1) \geq q(n)$.

Proof of Result 3

We shall show that (i) implies (ii), (ii) implies (iii), and (iii) implies (i).

(i) \rightarrow (ii): Indeed, let $\{\alpha\} \in E(N, m)$. Then, by Observation 7, $\alpha \leq q(m)$ and $\alpha \geq q(n - m + 1)$, thus $q(m) \geq q(n - m + 1)$.

(ii) \rightarrow (iii): Indeed, assume that $q(m) \geq q(n - m + 1)$, and let $q(m) \geq \alpha \geq q(n - m + 1)$. Then, by Observation 1, $s(b; N, \{\alpha\}) < m$ for all $b < \alpha \leq q(m)$, and by Observation 2, $s(b; N, \{\alpha\}) < m$ for all $b > \alpha \geq q(n - m + 1)$. Since $s(\alpha; N, \{\alpha\}) = n \geq m$, by Definition 3, $\{\alpha\} \in E(N, m)$.

(iii) \rightarrow (i): Trivial.

Q.E.D.

Proof of Result 4

Assume that $q(m) \geq q(n - m + 1)$.

If, moreover, $q(m) \geq q(n - m + 1)$, then by Result 3, $\{b\} \in E(N, m)$ for all b that satisfies $q(m) \geq b \geq q(n - m + 1)$. Since $S(a; N, \{a\}) = N$ for all $a \in \Omega$, $E_c(N, m) \neq \emptyset$, and the Result holds.

Suppose, therefore, that $q(n - m + 2) > q(m) \geq q(n - m + 1)$, and let $D = \{q(m), q(n - m + 2)\}$. Since $q(m) \geq q(n - m + 1)$, $\{i \in N | q(m) < q(i) < q(n - m + 2)\} = \{i \in N | q(m) < q(i) \leq q(n - m + 1)\} = \emptyset$. Hence, by Observation 3, $s(c; N, D) = 0 < m$ for all c that satisfies $q(m) < c < q(n - m + 2)$. By Observations 1 and 2 we have that $s(c; N, D) < m$ for both $c < q(m)$ and $c > q(n - m + 2)$. Since by Observation 4 $s(q(m); N, D) \geq m$ and $s(q(n - m + 2); N, D) \geq m$, Definition 3 implies that $D \in E(N, m)$. Moreover, by Observation 4, $S(q(m); N, D) \supset$

$\{1, \dots, m\} \supset \{1, \dots, n - m + 1\}$ and $S(q(n - m + 2); N, D) \supset \{n - m + 2, \dots, n + 1\}$. Thus, $S(q(m); N, D) = \{1, \dots, n - m + 1\}$ and $S(q(n - m + 2); N, D) = \{n - m + 2, \dots, n + 1\}$, that is, D is a consecutive m -equilibrium for N .

Q.E.D.

Proofs of Results 2, 5 and 6

Both Results 2 and 5 are corollaries of the following theorem.

THEOREM. For two positive integers n, m with $n > m$ either there exists an m -equilibrium which consists of a single alternative or else there exists an m -equilibrium $\{\gamma_1, \dots, \gamma_T\}$ where $\gamma_1 = q(m)$ and $\gamma_T = q(n - m + 1)$.

We present the proof of the Theorem after establishing the following fact. Let A be an m -equilibrium for N that consists of at least two alternatives, the first being $q(m)$ and the last being $q(n - m + 1)$. Then, if by replacing $q(n - m + 1)$ in A by $q(n - m + 2)$ the resulting subset is not an m -equilibrium for N , then it must be the case that a potential candidate can attract at least m votes by offering an alternative that lies between a_{T-1} and $q(n - m + 1)$ which differs from a_{T-1} . Formally,

LEMMA 1. Let $A \in E(N, m)$ where $T \geq 2$, $a_1 = q(m)$, $a_T = q(n - m + 1)$ and let $\tilde{a}_T = q(n - m + 2)$. Then $\tilde{A} = \{a_1, \dots, a_{T-1}, \tilde{a}_T\} \notin E(N, m)$ if and only if there exists b , $a_{T-1} < b < a_T$ such that $s(b; N, \tilde{A}) \geq m$.

Proof. By Observation 6, for all $a < a_{T-1}$, $S(a; N, \tilde{A}) = S(a; N, A)$ and $S(a_{T-1}; N, \tilde{A}) \supset S(a_{T-1}; N, A) \supset S(a_{T-1}; N, A)$. Since $A \in E(N, m)$, by Definition 3 for all $a < a_{T-1}$, $s(a; N, \tilde{A}) \geq m$ if and only if $a \in A$. Therefore, Observations 2 and 4 imply that $\tilde{A} \in E(N, m)$ if and only if for all a , $a_{T-1} < a < \tilde{a}_T$, $s(a; N, \tilde{A}) < m$. By Observation 2, for all a , $a_{T-1} < a < \tilde{a}_T$, $S(a; N, \tilde{A}) \subset \{1, \dots, n - m + 1\}$. Thus, by Observation 5, for all a , $a_{T-1} < a < \tilde{a}_T$, $S(a; N, \tilde{A}) = S(a; N, A)$. Hence, $\tilde{A} \in E(N, m)$ if and only if for all a , $a_{T-1} < a < a_T$, $s(a; N, \tilde{A}) < m$.

Q.E.D.

Proof of Theorem. The proof is by induction on m . For $n = 1$ we must have $m = 1$ and, clearly, $\{q(1)\} \in E(\{1\}, 1)$. Assume that the theorem holds for any society with no more than n individuals. We shall show that it also holds for a society $N = \{1, 2, \dots, n + 1\}$ with $n + 1$ individuals. If $m = n + 1$, then by Result 3, every α , $q(n + 1 - m + 1) = q(1) < \alpha < q(n + 1) = q(m)$ satisfies $\{\alpha\} \in E(N, m)$. Thus, the theorem holds.

Suppose, therefore, that $m \leq n$ and denote $N' = \{1, 2, \dots, n\}$ (i.e., $N = N' \cup \{n + 1\}$). If $q(n - m + 1) \leq q(m)$, then by Result 3 there exists $\{\alpha\} \in E(N, m)$ and Result 4 yields the theorem for this particular case.

It remains, therefore, only to consider the case where $m \leq n$ and $q(m) < q(n - m + 1)$. Then, the induction hypothesis implies that: $E(N, m) \neq \emptyset$, every m -equilibrium for N contains at least two alternatives, and there is an m -equilibrium that contains both $q(m)$ and $q(n - m + 1)$. Choose $A^* = \{\alpha_1, \dots, \alpha_T\} \in E(N, m)$ to be such that $\alpha_1 = q(m)$, $\alpha_T = q(n - m + 1)$ and every $\{\gamma_1, \dots, \gamma_k\} \in E(N, m)$, with $\gamma_1 = q(m)$, $\gamma_k = q(n - m + 1)$ satisfies $\gamma_{k-1} \leq \alpha_{T-1}$. (Such an A^* exists because the set of alternatives, Ω , and hence $E(N, m)$, are finite sets). Define $\tilde{A} = \{\alpha_1, \dots, \alpha_{T-1}\}$, where $\tilde{\alpha}_T \equiv q(n - m + 2)$. If $\tilde{A} \in E(N, m)$, we are done. Otherwise, by Lemma 1, the set $Z = \{b \in \Omega | b < \alpha_T \text{ and } s(b; N, \tilde{A}) \geq m\}$ is nonempty. Define $\beta^* = \max\{b | b \in Z\}$. (Such a β^* exists since Ω , and hence Z , are finite sets.) Clearly, $q(m) \leq \alpha_{T-1} < \beta^* \leq \alpha_T = q(n - m + 1)$. Denote $P = \{i \in N' | q(i) < \beta^*\}$, and let M be a set of m identical individuals whose preferences are single peaked at β^* . (Note that it is possible that the peak of no individual in N is β^* , i.e., that $M \cap N = \emptyset$.) Since $\beta^* < \alpha_T = q(n - m + 1)$, $P \cap \{n - m + 1, \dots, n + 1\} = \emptyset$. Thus the cardinality of P does not exceed $n - m$. Hence, the cardinality of $N' \equiv P \cup M$ does not exceed n . Moreover, since β^* (the peak of the last m individuals of the society N'), satisfies $\beta^* > \alpha_{T-1} > q(m)$, by the induction hypothesis there exists $B^* = \{\beta_1, \dots, \beta_H\} \in E(N', m)$ with $\beta_1 = \alpha_1 = q(m)$ and $\beta_H = \beta^*$. We shall conclude the proof of the theorem by proving:

PROPOSITION. $\tilde{B} \equiv B^* \cup \{\tilde{\alpha}_T\} = \{\beta_1, \dots, \beta_{H-1}, \beta_H, \tilde{\alpha}_T\} \in E(N, m)$.

In order to establish the proposition we need the following:

LEMMA 2. $\beta_{H-1} \leq \alpha_{T-1}$.

Proof. We shall show that if $\beta_{H-1} > \alpha_{T-1}$, then $C \equiv \{\beta_1, \dots, \beta_{H-1}, \alpha_T\} \in E(N, m)$, which contradicts the choice of $A^* = \{\alpha_1, \dots, \alpha_{T-1}, \alpha_H\} \in E(N, m)$. (Recall that if $\{\gamma_1, \dots, \gamma_k\} \in E(N, m)$ with $\gamma_1 = q(m)$ and $\gamma_k = q(n - m + 1)$, then $\gamma_{k-1} \leq \alpha_{T-1}$.)

Assume, therefore, that $\beta_{H-1} > \alpha_{T-1}$. Then,

- (i) For all $b < \beta_{H-1}$, $s(b; N, C) \geq m$ if and only if $b \in C$: Indeed, by Observation 3, for all $b < \beta_{H-1}$, $s(b; N, C) = s(b; P, B^*) = s(b; N', B^*)$. Since $B^* \in E(N', m)$, Definition 3 yields (1).
- (ii) $s(\beta_{H-1}; N, C) \geq m$: Indeed, since $\beta_{H-1} < \beta_H$

$= \beta^* \leq \alpha_T$, we have from observation 6 that $S(\beta_{H-1}; N, C) \supset S(\beta_{H-1}; N, B^*) = S(\beta_{H-1}; P, B^*) = S(\beta_{H-1}; N', B^*)$. Since $B^* \in E(N', m)$, Definition 3 yields (ii).

- (iii) For all $b > \beta_{H-1}$, $s(b; N, C) \geq m$ if and only if $b \in C$; that is, $b = \alpha_T$. Indeed, since we assumed that $\beta_{H-1} > \alpha_{T-1}$, Observation 6 and Definition 2 imply that $s(b; N, C) \leq s(b; N, A^*) < m$ for all $b > \beta_{H-1}$, $b \neq \alpha_T$. Moreover, Observation 4 implies that $s(\alpha_T; N, C) \geq m$.

Thus, (i), (ii), (iii), and Definition 3 imply that $C \in E(N, m)$, which concludes the proof of Lemma 2.

Q.E.D.

Proof of the Proposition

- (iv) For all $b < \beta^* = \beta_{H^*}$, $s(b; \tilde{N}, \tilde{B}) \geq m$ if and only if $b \in \tilde{B}$: Indeed, by Observation 3 for all $b < \beta^*$, $S(b; \tilde{N}, \tilde{B}) = S(b; P, B^*) = S(b; N', B^*)$. Since $B^* \in E(N', m)$, Definition 3 yields (iv).
- (v) $s(\beta_{H^*}; \tilde{N}, \tilde{B}) \geq m$: Indeed, since by Lemma 2, $\beta_{H-1} \leq \alpha_{T-1}$ then Observation 6 implies $S(\beta_{H^*}; \tilde{N}, \tilde{B}) \supset S(\beta_{H^*}; N, A)$. But $\beta_H = \beta^* \in Z$ hence $s(\beta_{H^*}; \tilde{N}, A) \geq m$.
- (vi) For all b , $\beta^* = \beta_H < b \leq \alpha_T$, $s(b; \tilde{N}, \tilde{B}) < m$: Indeed, since $\beta^* > \alpha_{T-1}$, by Observation 6, $S(b; \tilde{N}, \tilde{B}) \subset S(b; \tilde{N}, A)$ for all b with $\beta^* < b \leq \alpha_T$. But, by the choice of β^* , $s(b; \tilde{N}, A) < m$ for all b with $\beta^* < b \leq \alpha_T$.
- (vii) For all b , $\alpha_T < b < \tilde{\alpha}_T$, $s(b; \tilde{N}, \tilde{B}) < m$: Indeed, by Observation 1 $S(b; \tilde{N}, \tilde{B}) \subset \{1, \dots, n - m + 1\}$ for all $b < \tilde{\alpha}_T$. Therefore, by Observation 5 $S(b; \tilde{N}, \tilde{B}) \subset S(\tilde{\alpha}_T; \tilde{N}, \tilde{B})$ for all b , $\alpha_T < b < \tilde{\alpha}_T$. Thus, (vi) yields (vii).
- (viii) For all b , $b > \tilde{\alpha}_T$, $s(b; \tilde{N}, \tilde{B}) \geq m$ if and only if $b \in \tilde{B}$, that is, $b = \tilde{\alpha}_T$: follows directly from observations 4 and 2.

By Definition 3, (iv)-(viii) imply that $\tilde{B} \in E(\tilde{N}, m)$ which completes the proof of the proposition and hence of the theorem.

Q.E.D.

Now, Result 2 is, of course, an immediate corollary of the theorem. Moreover, if $q(m) < q(n - m + 1)$, then by Result 3, there is no m -equilibrium which consists of a single alternative, and the theorem yields Result 5.

Proof of Result 6. Because by Result 2, $E(N, m) \neq \emptyset$ and clearly, $E_c(N, m) \subset E(N, m)$, we have only to prove that for any $A = \{a_1, \dots, a_T\} \in$

$E(N, m)$, A is a consecutive m -equilibrium, that is, that $S(a_i; N, A)$ is consecutive for each $a_i \in A$. Let $A = \{a_1, \dots, a_T\} \in E(N, m)$, $i, j, k \in N$ with $i < j < k$ and suppose that for some τ , $\{i, k\} \subset S(a_\tau; N, A)$. We have to show that $j \in S(a_\tau; N, A)$.

Since $\{i, k\} \subset S(a_\tau; N, A)$, then for all $a_i \in A$, $i \neq \tau$

$$|q(i) - a_i| > |q(i) - a_\tau| \text{ and}$$

$$|q(k) - a_i| > |q(k) - a_\tau|.$$

Therefore, for all q , $q(i) \leq q \leq q(k)$, for all $i \neq \tau$

$$|q - a_i| > |q - a_\tau|.$$

In particular, this inequality holds for $q = q(j)$, that is, $j \in S(a_\tau; N, A)$.

Q.E.D.

Proofs of Results 7 and 8

Proof of Result 7. Consider the society $N = \{1, 2, \dots, 10\}$, with 10 voters, in which the set of alternatives consists of 10 alternatives given by $\Omega = \{a_1, a_2, \dots, a_{10}\}$, where $a_1 < a_2 < \dots < a_{10}$.

All that is required of the preference ordering, \succ_i , of individual $i \in N$ over Ω is that it be single peaked, with i 's peak being a_i , and also satisfy the following conditions.

For $i = 4$, $a_4 \succ_i a_3 \succ_i a_2 \succ_i a_1$ and $a_4 \succ_i a_5 \succ_i a_6 \succ_i a_7 \succ_i a_8 \succ_i a_9 \succ_i a_{10}$; for $i = 5$, $a_5 \succ_i a_4 \succ_i a_3 \succ_i a_2 \succ_i a_1$ and $a_5 \succ_i a_6 \succ_i a_7 \succ_i a_8 \succ_i a_9 \succ_i a_{10}$; for $i = 6$, $a_6 \succ_i a_5 \succ_i a_4 \succ_i a_3 \succ_i a_2 \succ_i a_1$ and $a_6 \succ_i a_7 \succ_i a_8 \succ_i a_9 \succ_i a_{10}$; for $i = 7$, $a_7 \succ_i a_6 \succ_i a_5 \succ_i a_4 \succ_i a_3 \succ_i a_2 \succ_i a_1$ and $a_7 \succ_i a_8 \succ_i a_9 \succ_i a_{10}$; for $i = 8$, $a_8 \succ_i a_7 \succ_i a_6 \succ_i a_5 \succ_i a_4 \succ_i a_3 \succ_i a_2 \succ_i a_1$ and $a_8 \succ_i a_9 \succ_i a_{10}$. Finally, let the quota m be equal to 3. Then, it is easy to check that $A = \{a_3, a_5, a_7\}$ constitutes a 3-equilibrium, where $S(a_3; N, A) = \{1, 2, 3\}$; $S(a_5; N, A) = \{4, 5, 6\}$; $S(a_7; N, A) = \{7, 8, 9, 10\}$.

Suppose for simplicity that when an elected candidate deviates from an equilibrium, the resulting new equilibrium that he induces is the one in which no other elected candidate deviates and also, that his move does not enable a new potential candidate to receive m or more votes. Then, clearly the elected candidates in A that offer a_3 and a_5 will not deviate. Now, if the second elected candidate will offer alternative a_6 instead of a_7 (which is the one he is currently offering), the resulting induced equilibrium will be $B = \{a_3, a_6, a_7\}$, where $S(a_3; N, B) = \{1, 2, 3\}$; $S(a_6; N, B) = \{4, 5, 6, 7\}$; $S(a_7; N, B) = \{8, 9, 10\}$. It is easy to verify that B is a stable equilibrium, and therefore, no elected candidate in B has an incentive to deviate from it. Thus, the only equilibrium that the second elected candidate in A can generate via a_6 is B . But in B , the number of individuals who vote for him ($= 4$) is greater than the number who vote for him in A ($= 3$). Therefore,

the second elected candidate has an incentive to deviate from A via a_6 . Hence A is not a stable 3-equilibrium.

Q.E.D.

In order to prove Result 8 we first need:

LEMMA 3. Let $A = \{a_1, \dots, a_T\}$ be an m -equilibrium. If an elected candidate τ has an incentive to deviate from A via w , then τ cannot generate A from A via w .

Proof. Recall that τ has an incentive to deviate from A via w , if and only if by offering w instead of a_τ , he can guarantee himself a support with at least $\alpha \equiv s(a_\tau, N, A) + 1$ voters in any equilibrium that will result. But this means that whoever offers a_τ in A can guarantee himself (by offering w instead of a_τ) at least α voters. Now, assume in negation, that τ could generate A from A via w . Then, when the generated A obtains, the candidate who is now offering a_τ can also guarantee himself α votes and again "regenerate" A , at which stage yet another candidate (who is the one offering a_τ in this second iteration) will deviate and guarantee himself a support of size no less than α after which, once again, A will be generated and so on. After regenerating A n times, we will have that n different candidates in (the n th iteration of) A have a support of at least α individuals. But this is, of course, impossible, since then, the total number of supporters of candidates in A is at least $n\alpha \geq n(M + 1) > nM$. CONTRADICTION.

Q.E.D.

Proof of Result 8. Let $m \leq n$ be given. Then by Result 2, there exists $A_0 = \{a_1, \dots, a_T\} \in E(N, m)$. If A_0 is stable, we are done. Otherwise, there exists an elected candidate τ_1 and an alternative $w_1 \in \Omega$ such that τ_1 has an incentive to deviate from A_0 via w_1 . In particular, there exists $A_1 \in E(N, m)$ which is induced by τ_1 from A_0 via w_1 . If A_1 is stable, we are done. Otherwise, there is an elected candidate τ_2 (who may be the same as τ_1) and an alternative $w_2 \in \Omega$ such that τ_2 has an incentive to deviate from A_1 via w_2 , thereby inducing $A_2 \in E(N, m)$. Continuing this process yields the sequence $A = \{A_0, A_1, A_2, \dots\}$. We claim that A is finite, and hence there exists a stable m -equilibrium. Suppose to the contrary. Then, since the set of all m -equilibria is finite, if A is infinite there exist two different natural numbers, k and l , such that $A_k = A_l$ and $\{A_k, A_{k+1}, \dots, A_l\} \subset A$. But then the candidate who induces A_{k+1} from A_k also generates A_l from A_k , contradicting Lemma 3. Thus, A is finite, and by construction, the last element of A is a stable m -equilibrium.

Q.E.D.

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Risk and Uncertainty as Factors in the Durability of Political Coalitions

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The central purpose of this article is to shed some light on the roles that risk attitudes and uncertainty may play in determining the durability of political coalitions. Although Axelrod theorized that polarized, or "unconnected," coalitions would be less durable than "connected" coalitions, subsequent empirical research has found little support for Axelrod's hypothesis. In this article we demonstrate theoretically that many of the anomalies associated with Axelrod's polarization hypothesis can be explained by incorporating risk attitudes and uncertainty into a logical model of a coalition maintenance process. The model we have constructed assumes that coalition members are uncertain about their ultimate payoffs and therefore are subject to persuasion and manipulation by a coalition leader. Through computer simulation we demonstrate that polarized coalitions are indeed capable of enduring for relatively long periods of time once members' risk characteristics and uncertainty are taken into consideration.

An essential element of successful political leadership is the ability to maintain a supporting coalition. Yet surprisingly little is known about the ways in which political leaders keep their coalitions intact, and little is known about the properties of coalitions that most affect their longevity. Generally, political scientists have been much more interested in questions about the formation of coalitions than in questions about the maintenance of coalitions. But theories that are useful for explaining which coalitions form often are of little use for explaining which coalitions are most durable.¹ To the extent that formation and

maintenance entail different processes, additional research on the maintenance of coalitions is clearly needed.

The objective of this research is to formalize a dynamic model of a maintenance process and, through computer simulation, to assess the impact on longevity of three properties of coalitions and their members. The three properties examined are: polarization, that is, the divergence in the long-term policy goals of the coalition members; uncertainty, the members' subjective estimates of receiving their preferred payoffs from a coalition; and risk attitudes, the willingness of members to gamble on receiving their preferred outcomes from a coalition.

Of these three variables, only polarization has been the subject of previous investigation. Although a substantial portion of the literature of coalition theory treats actors as maximizers of expected utility (e.g., De Swaan, 1973; Leiserson, 1966; and Riker, 1962), that same body of literature speaks hardly at all to the roles of risk characteristics and uncertainty in the decision processes of those actors.² The work described herein is an risk attitudes and uncertainty into a dynamic model, we seek not only to provide directions for future research, but also to provide a systematic way of conceptualizing the process, or art, of political leadership.

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¹For example, minimal winning status is an important theoretical factor in the formation of coalitions, but there is considerable variance in the durability of minimal winning coalitions, once formed. See Dodd (1976, p. 141).

²One notable exception is the work of Bueno de Mesquita (1975, 1981).

Research on the Durability of Coalitions

Virtually all theoretical and empirical work on coalition durability since Axelrod (1970, p. 167) has included some notion of policy polarization, or divergence in the policy preferences of coalition members. Axelrod proposed that coalitions with low conflict of interest—as measured by the spatial distances among the policy positions of coalition members—“can be expected to last longer once formed than the average coalition, just because disputes within such a coalition will be easier to resolve.” Because Axelrod was not able to measure precisely the spatial distances among parties along some ideological continuum, he introduced the notion of a “connected” coalition in order to capture the amount of dispersion in the policy positions of coalition parties. A connected coalition was defined as a coalition consisting of adjacent parties.

Axelrod's hypothesis was nicely borne out by his own data on the Italian parliament from 1953 through 1969. Data from other European parliaments, however, have proved to be much less supportive. Neither Laver (1974) nor Warwick (1979) found statistically significant evidence that connected coalitions, as defined by Axelrod, lasted longer than unconnected coalitions, and Dodd (1976), using a variable that took into account both the number of conflict dimensions and the dispersion of parties along those dimensions, concluded that polarization had very little effect on cabinet durability.

In part, the lack of strong empirical support for Axelrod's theory can be attributed to differences (and difficulties) in operationalizing the concept of polarization. But regardless of the measurements used, support for the polarization hypothesis is generally weak. Evidence indicates that polarized coalitions frequently do endure, and specific examples are not hard to find. Among relatively polarized coalitions, the Zoli government in Italy lasted for 13 months—just over the average for the duration of all coalitions in the data presented by Axelrod (p. 178). The Bidault government of 1949, in France, lasted for nine months, a fair run for a Fourth Republic government (De Swaan, 1973, p. 184). Finally, in Israel, the Eshkal government of 1966 governed for 16 months (De Swaan, p. 234). All of these were unconnected, or “open,” coalitions. To be sure, they were not among the most long-lived of coalitions, but neither did they vanish in the blink of an eye.

The inability of the polarization hypothesis to account for such anomalies has encouraged a search for new explanations of coalition durability. Dodd (1976), for example, argues that durability depends on the size of cabinet coalitions,

and that the size of cabinet coalitions depends on a complex interaction of variables in the parliamentary party system: the number of parties, the relative strength of parties, the variance in party positions, and the number of salient ideological cleavages.

Although Dodd's empirical results are generally consistent with his theory, his model nevertheless fails to account for most of the variance in the durability of coalitions. His parliamentary party system variables account for only 20% of the variance in durability, and the size of the coalition (i.e., the degree to which the coalition approaches minimum winning status) also explains only about 20%. At best, then, 60% of the variance in the durability of coalitions remains to be accounted for.

The extensive unexplained variance in models of cabinet durability reveals the limitations of existing research strategies. Hence, Dodd (1984, p. 155), for one, has suggested that the challenge for scholars is to “reassess the conceptual and methodological underpinnings of current work and to explore new empirical avenues for research.” One alternative research strategy is to move away from strictly deterministic models and to begin to examine the stochastic components of coalitional maintenance situations (e.g., Cioffi-Revilla, 1984). In the approach taken here, we too introduce a probabilistic element into the maintenance process. We assume that coalition actors practice some Bayes-like probability estimation procedures, so that durability depends not just on party system characteristics, but also on the uncertainty and risk-taking characteristics of the actors.

Theoretical Considerations

Uncertainty as a Central Variable

The bearing of uncertainty upon the durability of coalitions derives from the fact that both leaders and members of coalitions depend upon their perceptions of one another to estimate each other's future behaviors. Insofar as they entail likely future policy behaviors, these perceptions are not likely to consist of point estimates held with absolute certainty. Rather, subjective beliefs about another's behavior may reasonably be characterized as probability distributions (e.g., DeGroot, 1970, chap. 6), and the confidence or lack of confidence one has about someone else's future behavior may be described in terms of the variance in those distributions. The greater the variance, the less confident one would be about another's likely behavior.

This is not to say that real political actors think explicitly in terms of probability distributions and

variances. Instead, their behaviors suggest that these may be reasonable terms in which to describe their decision making under uncertainty. Thus, if one had only a brief history of experience with a particular individual (i.e., only a few data points on which to base a judgment) whose behavior has been quite normal, one is apt to give more weight to a single instance of aberrant behavior (e.g., rage) than if one had a long experience of the person behaving calmly (i.e., many data points, very few of which involve rage). Similarly, with regard to the anticipated behaviors of two individuals with each of whom one has had a brief acquaintance, one is apt to be more confident about the future behavior of one than of the other, if that one—even on brief acquaintance—has behaved more consistently than the other.

Just as with these everyday experiences, the durability of political coalitions ought to be affected substantially by variances in the probability distributions each member associates with the likely long-run behaviors of the other members. Consider the situation from one possible perspective of the leader of party *A*. Let us call that person *L*, and let us assume that in terms of mean estimates, *L* perceives the relative positions of all other parties as depicted in Figure 1. However, let us also assume that *L* is not certain of the position of any of the parties except Party *A*, and that his degree of uncertainty varies from party to party, as represented by the probability distributions in Figure 2.

L's mean estimates of the position of each of the parties, as indicated by the vertical dashed lines in Figure 2, is the same as in Figure 1. However, the probability distributions that *L* associates with those same means open up an interesting possibility. Imagine a utility function for *L* such that association with policy positions to the right of point *X* have an unacceptable cost—perhaps loss of the leadership position or even assassination. Then, consider the wide variance about *L*'s estimate of party *C*'s position. As compared with *L*'s perception of party *D*'s position, the probability that party *C* will adopt a position to the right of *X* is quite high. Thus, one could reasonably expect *L* to prefer the coalition *ABD* rather than *ABC*, because the probability of being associated

with a policy position to the right of *X* is considerably smaller with coalition *ABD*.

Coalition *ABD*, however, is an "open" coalition in Axelrod's terminology and therefore should be less durable than coalition *ABC*, a "connected" coalition. But if we assume that *L* would withdraw from either coalition if his mean estimate of one of the partner's positions fell to the right of *X*, coalition *ABD* is likely to retain *L*'s allegiance longer than coalition *ABC*. Based upon the distributions in Figure 2, it would take relatively few instances of movement to the right on the part of *C* to move *L*'s mean estimate of *C*'s position to the right of *X*. But because of the rather spiked distribution that *L* has with regard to *D*'s position, it would take considerably more instances of rightward movement on the part of *D* to move *L*'s mean estimate of *D*'s position to the right of *X*. Thus, *ceteris paribus*, the coalition *ABD* might well be more durable than coalition *ABC*.

Risk Taking as a Central Variable

Under the maxims of utility theory, the attitude of an individual toward risk on a given issue is captured in the shape of his utility function regarding that issue. An individual is regarded as risk averse on a given issue if his utility function with respect to that issue is concave in relation to the horizontal axis, and as risk acceptant if that function is convex in relation to the horizontal axis, because of the theoretical implications of these shapes for the relationship between a particular outcome with certainty, on the one hand, and a gamble between best and worst outcomes, on the other (Friedman & Savage, 1948).

The bearing of risk characteristics on the durability of coalitions may be demonstrated by a discussion of the relationships presented in Figure 3. Therein, alternate utility functions are presented for some member *M* of coalition *C* for policy outcomes O_b , O_w , and O^* . Let O^* be the outcome *M* is certain of achieving by not being a member of coalition *C* (i.e., *M*'s assured payoff from an alternative coalition). Let outcome O_b be the best outcome *M* could realize by belonging to coalition *C*, and let O_w be the worst outcome that *M* could

Figure 1. Party Positions on a Policy Continuum

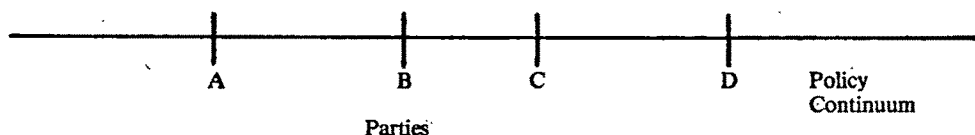
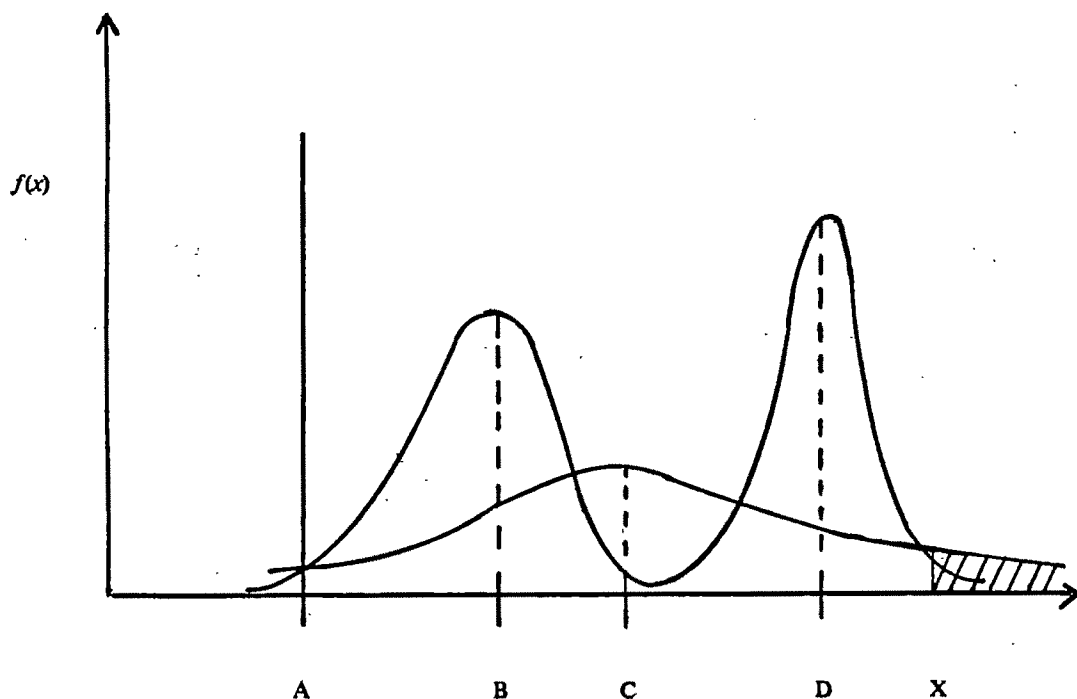


Figure 2. L's Perceptions of Party Positions



realize by being a member of C. Associated with O_w is a utility value of -1 , and associated with O_b is a utility value of $+1$. This is true regardless of the shape of M 's utility curve. However, this is not the case for O^* . If M is a risk taker, then the utility associated with O^* is $-.5$; and if M is a risk averter, the utility associated with O^* is $+.5$.

The decision problem for M is to choose between two alternative courses of action: to remain in coalition C or to leave coalition C. Assuming M is uncertain about the payoff he will receive from coalition C, and assuming M is a rational utility maximizer, M will choose that action which yields him the largest expected utility. To allow M to calculate expected utilities, we assign subjective probabilities to outcomes O_w and O_b . For purposes of illustration in Figure 3, we assume that M anticipates O_w and O_b each with probability one-half.

Because O_w and O_b are equally likely, M 's expected utility from staying in the coalition is $(.5)(1) + (.5)(-1) = 0$. Alternatively, because M is assured of obtaining O^* if he leaves the coalition, his utility from leaving is $-.5$ or $.5$, depending on whether he is a risk taker or a risk averter. Suppose M is a risk taker. Then, being a rational utility maximizer, M will prefer to stay in coalition C, because the expected utility of staying (0) is

greater than the utility from leaving ($-.5$). But just the opposite holds if M is a risk averter. In that case, the utility from leaving, $.5$, will be greater than zero. Thus, *ceteris paribus*, the loyalty of coalition members will depend on the shape of their utility curves over the set of possible policy outcomes. Members who are risk acceptant are more likely to remain loyal to the coalition than members who are risk averse.

The Interpretation of Risk Attitudes

The decision problem depicted in Figure 3 has several basic properties that are helpful in explicating and interpreting the concepts of risk acceptance and risk aversion. In order to develop these properties, let P_{ij} be coalition member i 's subjective probability of obtaining outcomes O_w and O_b from the coalition C, and let U_i^* be the utility associated with outcome O^* for member i ; $i=1,2$ and $j=w,b$. (We now drop the assumption that all probabilities equal one-half.) Let Member 1 have a risk-averse utility curve and Member 2 a risk-acceptant utility curve. (Assume U_1^* is greater than U_2^* .) Then, Member 1 will remain in the coalition only if $P_{1b} - P_{1w} > = U_1^*$, whereas Member 2 will remain in the coalition only if

$P_{2b} - P_{2w} \geq U_2^*$. For just two outcomes, $P_{1w} = 1 - P_{1b}$; and the minimum value of P_{1b} that will keep Member 1 in the coalition is:

$$P_{1b}^* = U_1^* + (1 - P_{1b}) = (U_1^* + 1)/2. \quad (1)$$

Similarly, the minimum value of P_{2b} that will keep Member 2 in the coalition is:

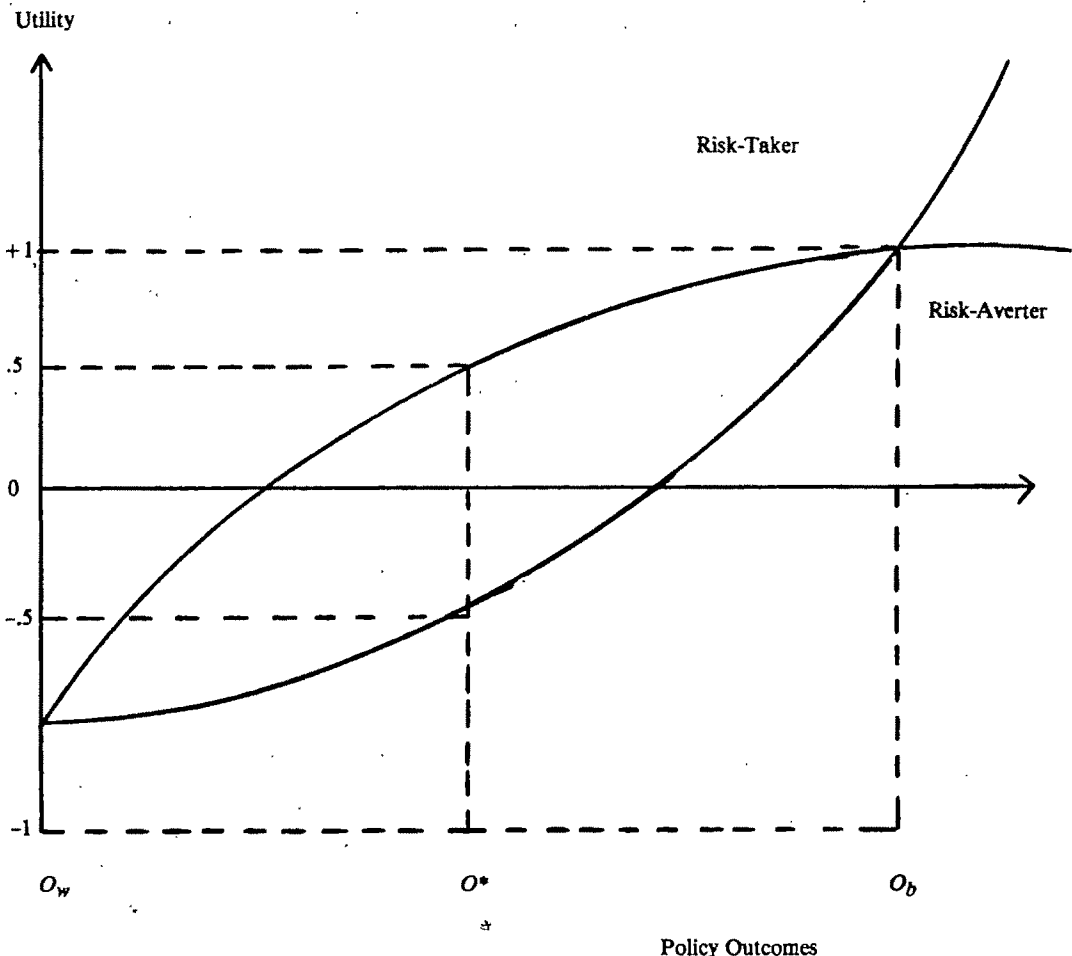
$$P_{2b}^* = U_2^* + (1 - P_{2b}) = (U_2^* + 1)/2. \quad (2)$$

Therefore, if coalition members are rational, they will remain in the coalition only if their subjective probabilities of obtaining their best outcomes are at least as large as the minimum values, P_{ib}^* , that satisfy equations (1) and (2). Very literally, these minima can be thought of as the members' *secu-*

ity levels. They indicate the level of assurance, or probability, that each member must have of achieving his preferred payoff. Any time a follower's subjective probability is at least as large as his security level, the follower believes he is in the best coalition possible. But any time his beliefs change such that the probability of receiving the preferred payoff drops below the security threshold, the rational member will leave the coalition.

From equations (1) and (2) it follows that different security levels, that is, different values of P_{ib}^* , reflect different degrees of convexity or concavity in utility curves. Since $U_1^* > U_2^*$, P_{1b}^* must be greater than P_{2b}^* . Consequently, individuals with risk-averse utility curves will require higher probabilities of achieving their preferred outcome than individuals with risk-acceptant utility curves,

Figure 3. Alternate Utility Functions for Coalition Member M



other things being equal. Security levels, then, are indicators of risk attitudes and will be treated as such in the ensuing analysis.

Security levels, like other attitudinal characteristics, are determined to some extent by factors totally outside of the model (e.g., genetics and social conditioning). However, security levels are also determined by the external coalitional opportunities available to a player. Clearly, the better O^* , the higher utility one can be certain of by belonging to some other coalition besides C . As indicated by equations (1) and (2), higher utilities for O^* will yield higher values of P_{1b}^* and P_{2b}^* . This is perfectly reasonable in that the better one can be assured of doing outside of coalition C , the greater assurance one must have that C will provide the desired payoff. Thus, the more attractive the external coalition opportunities are to member M of coalition C , the higher will be M 's security level.

The theoretical considerations presented thus far suggest that attitudes toward risk taking and uncertainty do indeed have a bearing on the longevity of political coalitions. This theoretical discourse, however, does not, by itself, yield insights into the extent to which security levels and uncertainty affect the longevity of coalitions; nor does it yield insights into the dynamics of the maintenance process. In order to develop the dynamic properties of the model as well as to test the model's sensitivity to risk attitudes and uncertainty, we have developed a computer algorithm that simulates the roles of risk, uncertainty, and polarization in the maintenance process. We turn now to the formal development of that algorithm.

A Model of a Coalition Maintenance Process

Our model of a maintenance process is based on a three-person coalition: two followers, or members, and a leader.³ We let $N = \{1, 2\}$ denote the set of followers and let L be the leader.⁴ L is given complete control over payoffs to N , and these payoffs are assumed to take the form of policy outcomes. L is assumed to make a series of policy decisions, each decision occurring at some point in time, t . Moreover, these policy decisions, or allocations, are assumed to occur at regular

intervals, so that an allocation at $t=6$, for example, is three times as far into the future as an allocation at $t=2$.

We let Z be a discrete random variable that assigns a real number, either 1 or 0, to each of L 's policy decisions. Each allocation, therefore, is dichotomous in nature, such as a decision to spend money on guns or on butter. The number of such allocations L is allowed to make is unrestricted, so that in effect one could think of L as sampling with replacement from an urn of 1s and 0s, where each draw represents a policy outcome.

The members of the coalition are required to have opposite preferences. Follower 1 always prefers 1 to 0 and Follower 2 prefers 0 to 1. In addition, we assume that for each follower there is some fixed proportion of 1s or 0s that represents his minimum acceptable payoff (*MAP*). The *MAP* represents the smallest payoff the follower would accept with certainty in exchange for participation in the coalition. If one thinks in terms of sampling from an urn, each follower's *MAP* would be the minimum (or maximum) proportion of 1s he would be willing to accept if the urn of 1s and 0s were divided between the followers at a single point in time. The difference in the followers' minimum (or maximum) acceptable proportion of 1s indicates the degree of polarization in the model coalition.

Each follower is assumed to require his minimum acceptable payoff with some fixed probability. Follower 1 might, for example, require 60% 1s with probability .75, and Follower 2 might require 60% 0s with probability .80. These fixed probabilities are the members' security levels, and they remain fixed throughout the sequence of allocations.

The Followers' Decision Problem

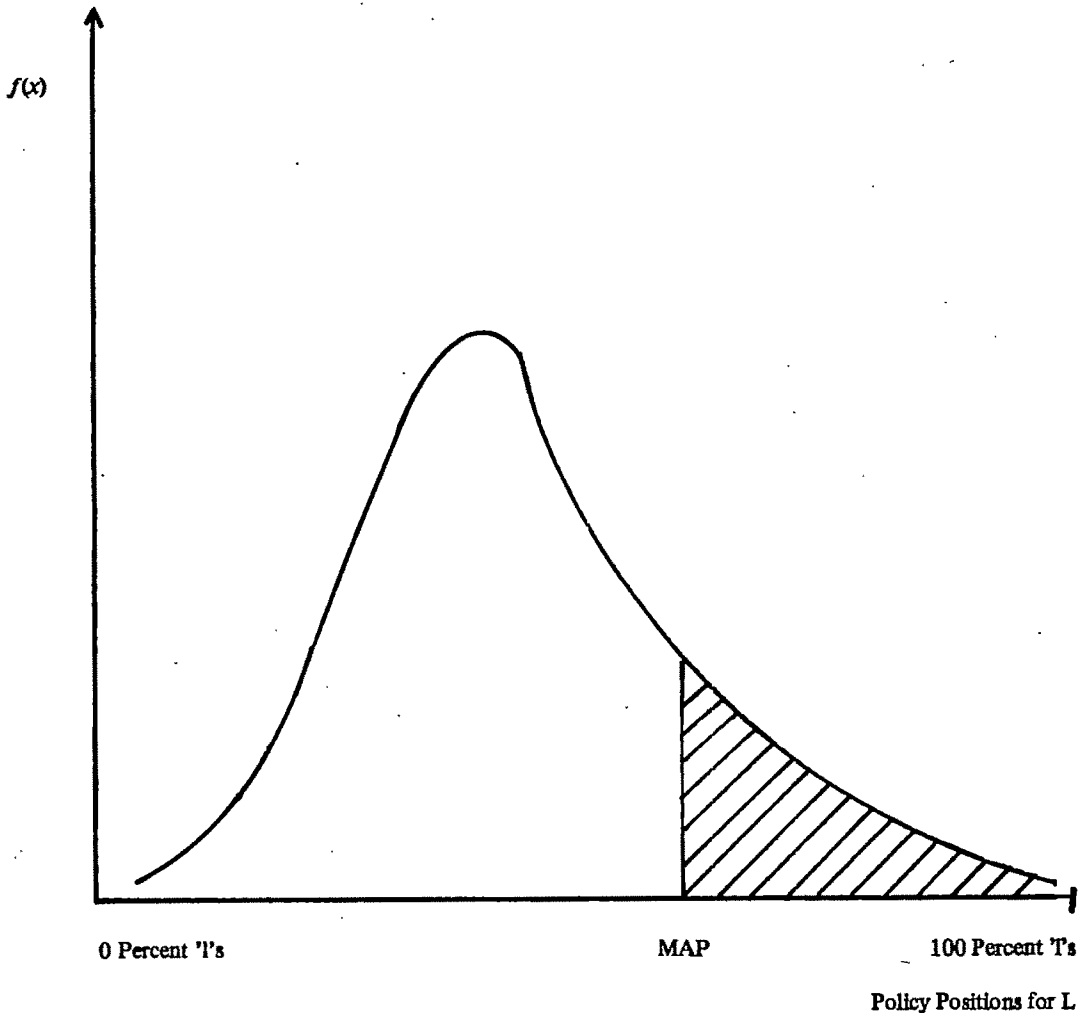
The decision problem for the followers is to estimate the likelihood of receiving their *MAP*s. This is problematic for the followers because we assume that they are not certain about L 's "true" policy position—that is, the proportion of 1s that L ultimately will allocate. As a result, the followers must make probabilistic estimates of receiving their *MAP*s, and in order to allow them to do so, we assume that each follower has a subjective probability distribution over the set of L 's possible policy positions. Figure 4 illustrates one possible distribution for a follower whose *MAP* is 60% 1s. The shaded area is the follower's subjective probability estimate of receiving his *MAP*.

After each allocation by L , we assume that the followers update their subjective probability estimates. Thus, the followers' distributions will shift

³The model is based on just a three-person coalition in order to keep the analysis simple. Even so, a number of important real-world situations can be described in terms of three-person coalitions—for example, the tripartite governments of the early French Fourth Republic.

⁴One might think of the followers as leaders also, but as leaders of political parties. The leader, L , however, is always the leader of a coalition of parties.

Figure 4. Coalition Member's Subjective Probability Distribution



one way or the other depending on the allocations they observe. In Figure 4, for example, the more 1s the follower observes, the larger will be the area under the curve to the right of the *MAP* point.

A formal prescription for updating a subjective probability estimate on the basis of prior information and sequential observations is given by Bayes' Theorem. Under a Bayesian process, an a priori distribution at time 1 becomes an a posteriori distribution at time 2, which becomes a new a priori distribution at time 3, and so forth. In problems of decision-making under uncertainty, any decision rule that prescribes an action that maximizes one's expected utility given some a priori distribution is said to be a *Bayes decision rule* against the a priori distribution. (See Luce & Raiffa, 1957, pp. 309-313.) In the present context, we assume that the followers in the coalition are

rational Bayesian actors in the sense that each employs a Bayes decision rule.

The Bayes decision rule, or the optimal decision rule, for the followers in the present context is to leave the coalition any time their probability estimates drop below their security levels. In Figure 4, for example, a follower who preferred 1s to 0s would compare his security level with the value of the area of the shaded region, and if the shaded area were smaller than his security level, he would leave the coalition.

The formalization of the followers' decision rule requires the following definitions.

DEFINITION 1. Let Θ be an allocation rule by which L chooses some $z \in \{0,1\}$ at each decision point. L 's decision at time t is given by $\Theta(t) = z$.

DEFINITION 2. Let a series of decisions by L be called a sequence, and let a sequence of t elements be denoted by S_t . As an example, one possible sequence of three elements would be denoted by $S_3 = (0, 1, 0)$.

DEFINITION 3. Let r be the number of 1s in t allocations.

DEFINITION 4. Let $X = \{x: 0 \leq x \leq 1\}$, be the set of possible policy positions for L ; where each $x \in X$ represents some proportion of 1s.

DEFINITION 5. Let μ_1 be the minimum proportion of 1s acceptable to Follower 1, and let μ_2 be the maximum proportion of 1s (or minimum proportion of 0s) acceptable to Follower 2.

For some fixed t and r , let $f(x; r, t)$ be a subjective probability function for member i over X corresponding to the beta distribution.⁵

DEFINITION 6. $f(x; r, t) = (1/k)x^{r-1}(1-x)^{t-r-1}$, where $r = \sum_{k=1}^t z_k$, and $(1/k) = (t-1)!/(t-r-1)!$.

The mean of the beta distribution is given by r/t , and the variance by $r(t-r)/t^2(t+1)$. Thus, the followers' beliefs about L 's position at the beginning of the sequence can be specified by adjusting the variance and the mean of their subjective probability distributions through the parameters r and t .⁶

DEFINITION 7. Denote L 's true policy position by x^* , where $x^* \in X$ such that $\lim_{t \rightarrow \infty} \Pr(|r/t - x^*| > \epsilon) = 0$ where ϵ is a small, positive constant.

In other words, x^* is the point about which the followers' probability distributions eventually collapse. Hence, with enough observations, the

followers become certain of L 's actual policy position.

Given the probability density function of Definition 6, we can now define the subjective probability estimate for follower i at time t that L 's policy position is equal to or greater than the follower's minimum or (maximum) acceptable proportion of 1s.

DEFINITION 8.

$$P_{it} = \Pr(X > \mu_i) = \int_{\mu_i}^1 f_i(x; r, t) dx.$$

DEFINITION 9. Let P_1^* be Follower 1's security level, and let P_2^* be Follower 2's security level.

With these last two definitions, the followers' decision rules are: For Follower 1, if $P_{1t} < P_1^*$, leave the coalition; for Follower 2, if $P_{2t} > P_2^*$, leave the coalition. With $\mu_1 > \mu_2$, we assume $P_1^* > 0$ and $P_2^* < 1$, implying that neither member will stay in the coalition with certainty.

The decision problem for the followers can be summarized briefly. Each follower is assumed to have some predetermined minimum long-term acceptable payoff, μ_i , along with some required and constant probability of obtaining that payoff, P_i^* , called the security level. We interpret the difference in the followers' values of μ as the degree of polarization in their policy preferences, and each follower's value of P_i^* is taken as a measure of risk taking. For each follower, the higher the value of P_i^* , the more confident he must be of achieving his required proportion of 1s or 0s, and the less willing he will be to risk staying in a coalition that might not provide μ_i . In addition, each follower has some belief, P_{it} , or subjective probability estimate, that the leader's true policy position is such that, in the long run, the leader will deliver at least the minimum acceptable payoff. These subjective estimates of probability will change as the followers observe L 's behavior, and any time a follower's estimate of the probability that L will deliver his minimum acceptable payoff does not meet his security level, P_i^* , the follower leaves the coalition.

Our view of the maintenance process is similar to that described by Grimsson (1982), who conceives the maintenance process as a series of connected games.

Changes in environmental parameters, the emergence of new problems and tasks, the need for new policies, and continuous payoff allocations—all these keep coalition actors involved in an unbroken bargaining process. Coalition maintenance thus can be viewed as a series of connected games. . . . When a partner becomes more and more dissatisfied with the outcome of in-

⁵The beta distribution fits our needs here because it is capable of representing a wide variety of priors, and information is incorporated easily to update the shape of the distribution. The beta distribution also has the property that if the prior distribution follows a beta distribution, then the posterior distribution will also follow a beta distribution (DeGroot, 1970, chap. 9).

⁶Even though a sequence, S_t , begins only when the coalition is first formed, the members of the coalition are assumed to have had different experiences with L before the formation of their coalition. In other words, each follower enters the coalition with some expectation of L 's position. This expectation, or prior, might be based on observations of previous allocations of 1s and 0s by L , or on verbal commitments by L .

dividual games, the maintenance propensity is likely to fall and the process of dissolution will set in. (p. 177)

The Leader's Decision Problem

The leader's role in the present model is to make sequential allocations of 1s and 0s. What makes the leader's role significant, though, is that we endow him not only with perfect information about his followers' preferences, uncertainty, and security levels; but also that we allow him to choose 1s and 0s *in any sequence he wishes*. Thus, the leader is given the power to manipulate the followers' beliefs of receiving their minimum acceptable payoffs. By allocating a series of 1s, for example, the leader could increase a follower's confidence of receiving his *MAP* (provided the follower preferred 1s to 0s) and, therefore, strengthen his allegiance to the coalition.

We assume that the leader's sole objective is to maximize the longevity of his coalition and therefore the longevity of his leadership. The decision problem for the leader, then, is how to allocate a sequence of 1s and 0s that is optimal in the sense that it keeps the coalition together for as long as possible. In order to accomplish this objective, the leader needs an optimal allocation decision rule, and we turn now to the derivation of such a rule.

Thus far only a particular sequence, S_t , consisting of any number of 1s and 0s has been defined. In order to identify an optimal allocation rule for L , we now define a set of sequences, \bar{S} , the set of *uninterrupted* sequences, which have the property that both members are still in the coalition after the last allocation and have been in the coalition through all previous allocations.

DEFINITION 10. $\bar{S} = \{S_t: P_{1k} \geq P_1^* \text{ and } P_{2k} < P_2^* \text{ for } k = 1, \dots, t\}$.

Also, we define a set of finite sequences all of the same length, τ , within \bar{S} .

DEFINITION 11. $S' = \{S_t \in \bar{S}: t = \tau, \tau < \infty\}$.

Defined next are two specific sequences, a complete sequence and an optimal sequence.

DEFINITION 12. A sequence S_t is a complete sequence if and only if $S_t \in S'$ and $S_{t+1} \notin S'^{+1}$.

DEFINITION 13. A sequence S_t is an optimal sequence if and only if $S_t \in S'$ and $S'^{+1} = \phi$.

By definition, all optimal sequences must be finite sequences. Therefore, in order to establish that an optimal sequence does in fact exist, it must

be proven that finite sequences exist. Theorem 1 establishes that there is a sequence that at some point cannot be extended without destroying the coalition. A proof of Theorem 1 is contained in the appendix.

THEOREM 1. In three-person political coalitions consisting of rational Bayesian actors, dichotomous policy choices, and a leader who seeks to maximize the longevity of his coalition, there exists a finite t such that $S' \neq \phi$ and $S'^{+1} = \phi$.

The idea behind the proof of Theorem 1 is quite simple once two assumptions are made: each follower is assumed to be able to receive his minimum acceptable payoff from some other coalition with a nonzero probability, and the followers are assumed to have conflicting preferences for 1s. Then, as the number of allocations by L becomes large, the variance in each follower's density collapses about x^* , so that in the limit at least one follower's subjective estimate of receiving his *MAP* will approach zero and drop below his security level, driving him out of the coalition and ending the sequence.

Theorem 2 (the proof of which also appears in the appendix) states that all complete sequences will be optimal sequences.

THEOREM 2. In three-person political coalitions consisting of rational Bayesian actors, dichotomous policy choices, and a leader who seeks to maximize the longevity of his coalition, if $S_t \in S'$ and $S_{t+1} \notin S'^{+1}$, then $S'^{+1} = \phi$.

The significance of Theorem 2 is that *any* complete sequence (i.e., a sequence such that both members are still in the coalition after the last allocation, both members have been in the coalition through all previous allocations, and a sequence that cannot be extended without driving one of the members out), will be an optimal sequence. Consequently, in terms of an optimal allocation rule for L , the choice of a 1 or a 0 at each step in the sequence is arbitrary as long as the coalition remains intact. The allocation rule, Θ^* , given in Definition 14, requires only that each allocation is made in such a way that, if possible, it does not drive one of the members out of the coalition. Thus, any sequence that proceeds according to Θ^* will eventually produce a complete and, therefore, optimal sequence.

DEFINITION 14. Let Θ^* be the rule whereby at each decision point L chooses any $z \in \{0, 1\}$ such that $S_t \in S'$.

While allowing for the specification of an optimal allocation rule for the coalition leader, Theorem 2 is also interesting in another respect. Theorem 2 suggests that there may very well be a

considerable degree of arbitrariness in the policy process; that is, a leader is predicted to be able to do pretty much as he pleases unless the viability of his coalition is threatened directly. Thus, the model suggests that the political process is such that leaders are held accountable only at certain critical junctures in their leadership, and that the frequency with which these critical junctures occur is a function of the uncertainty and risk characteristics of the members of the coalition.

The Simulations

In a world where at least one of the coalition members would leave the coalition immediately if the eventual payoff were known with certainty, the important questions are: How long will it take the coalition members to decide that the nature of the world is such that they will not obtain their required payoffs, and why does it take the coalition members as long as it does to come to such a realization? Although our theoretical expectations are that risk attitudes, uncertainty, and polarization all have a bearing on members' decisions to participate in a coalition, we do not know *a priori* just how important these factors are, or how important they are relative to each other. The purpose of the computer simulations is to provide information about these relationships for a selected set of prior distributions. Our primary hypotheses are that coalitions composed of risk-acceptant members are significantly more durable than coalitions composed of risk-averse members, and that a high level of uncertainty among the members significantly reduces the possibility that a coalition will survive.

Generation of the Data

All of the data used in the subsequent analyses were generated from a prior distribution with a mean of .6 for each follower; that is, each follower's initial expectation of the leader's true policy position was assumed to be .6, or 60% favorable outcomes.⁷ The .6 mean was selected as a reasonable approximation to real-world situations. It is not unrealistic to think each follower initially might expect 60% favorable, albeit diametrically opposed, outcomes. However, as long as we assume that the followers have conflicted preferences, it is increasingly less realistic to believe that each follower will initially expect 70 or 80% favorable, but diametrically opposed, outcomes. Few leaders could be expected to be so skillful, or so ambiguous, as to allow for such

divergent beliefs about their true policy preferences. Conversely, one would not expect rational and pragmatic politicians to belong to a coalition in which fewer than 50% favorable outcomes were anticipated.⁸

Given a fixed mean of .6 for the initial prior distributions of the followers, we selected different variances for those prior distributions in order to represent various degrees of uncertainty about the leader's position. The largest variance chosen was based on a prior n of 20, whereas the smallest was based on an n of 100. In total, six different prior distributions were specified, with between approximately 40 and 100 iterations generated for each.⁹

The values of the remaining parameters were fixed as follows. The maximum values for risk and polarization were determined by the points at which only a single additional allocation could be made to each member before the coalition fell apart. The minimum value for the risk attitude was set at .51 under the assumption that most coalition members desire slightly better-than-equal odds of obtaining their minimum long-term required payoffs. In addition, the minimum value for polarization also was set at .51, allowing for just a slight contrast in members' preferences.

Model coalitions were constructed using these initial values for risk and polarization, and the allocation process was allowed to proceed until the coalition ended. Before each allocation, the leader (i.e., the computer algorithm) checked to see if it made a difference whether a 1 or a 0 was allocated. If it made no difference, that is, if an allocation of one type or the other would not drive one of the members out of the coalition, a 1 or a 0 was allocated randomly. Then, the members' prior distributions were adjusted accordingly, and another allocation was made. When the coalition finally ended, the risk and polarization values were incremented one at a time by a value of .01, and the allocation process was restarted for the new coalition.

A separate record of data was constructed for each of the simulated coalitions. Written on each of those records were the values of polarization for each follower (that is, the value of each follower's minimum long-term required payoff), a value for each follower's risk attitude (the required probability of receiving one's minimum long-term required payoff), the initial variance in the prior distribution concerning the leader's posi-

⁷One exception might involve coalitions of small parties, or parties with a very limited agenda.

⁸With a fixed mean of .6, it was found that these priors allowed for a sufficient number of iterations and a sufficient range of iterations. The six initial priors were based on 20, 30, 40, 50, 75, and 100 observations.

⁹The computer program used to generate these data was written in PL1 and is available from the authors.

Table 1. Means, Ranges, and Standard Deviations for Independent and Dependent Variables
($N = 566$)

Variable	Mean	Minimum	Maximum	Standard Deviation
Iterations	36.9	2	148	34.3
Polarization	.563	.51	.59	.019
Security	.652	.51	.94	.099
Uncertainty	.005	.0024	.011	.0025

tion, and the number of iterations or allocations survived by that particular coalition. In order to simplify the data analysis, only symmetric coalitions—coalitions with members having equal but opposite values for risk and polarization—were analyzed.

Analysis of the Data

The allocation process was stimulated in 566 different coalitions, each consisting of some unique combination of polarization, uncertainty, and security. Table 1 shows various summary statistics for these three variables and the number of iterations each coalition lasted. On average, each coalition lasted roughly 37 iterations; the maximum was 148 and the minimum just two.¹⁰ The security variable exhibited much greater variance than either the uncertainty variable or the polarization variable; and coalitions reaching a polarization of .60 generally were not able to survive one iteration, even when security levels were quite low and uncertainty was low.

In order to evaluate the impact of each of the three variables on iterations, the total number of iterations lasted by each coalition was regressed

on polarization (P), level of security (S), and uncertainty (U). The results of this regression analysis, based on all 566 cases, are displayed in Table 2. (The coefficients for polarization and security are based on values of the variables ranging from 0 to 100 rather than 0 to 1.) Together, the three variables explain 78% of the variance in iterations.¹¹ This good fit, however, is not a surprise, because the regression has been applied to data generated by a computer simulation of a logical model. Rather, the good fit is merely a vindication of the algorithm itself.

Especially important is that each of the three variables affects iterations in the theoretically prescribed manner. Higher levels of polarization, security levels, and uncertainty noticeably shorten the longevity of the coalitions. Moreover, both security level and uncertainty exert large enough effects on iterations to mitigate the effects of polarization. Based on the coefficients in Table 2, a polarized coalition ($P = 56.5$) with risk-acceptant members ($S = 52$) and moderate uncertainty ($U = .005$) will last about 26 iterations—just about the same duration as an unpolarized coalition ($P = 52$) with risk-averse members ($S = 80$) and moderate uncertainty. Thus, holding the uncertainty factor constant and varying the security levels of the members is sufficient to make

¹⁰Coalitions that would not last even one iteration or coalitions that lasted for 150 iterations or more were excluded from the data. The upper level of 150 was chosen arbitrarily in order to avoid outlier coalitions, that is, coalitions that lasted far beyond the length of the average coalition. This upper bound eliminated only 13 cases.

¹¹Logarithmic transformations of the variables increased the R^2 value to about .87, indicating that the relationships are not perfectly linear. Nevertheless, for simplicity of interpretation, we present the variables and their coefficients in their original form.

Table 2. Regression Results for Simulation Data

Variable	Unstandardized Coefficient	Standardized Coefficient
Intercept	1370	—
Polarization	~ 19.9	-1.098
Security	~ 3.1	~ .900
Uncertainty	-11689	~ .851

polarized and unpolarized coalitions endure for about equal lengths of time.

Similarly, a very polarized coalition is predicted to last for close to the average iterations of all coalitions when the members are quite confident about the leader's policy position. For example, a polarized coalition ($P=58$) with risk-acceptant members ($S=52$) and very low uncertainty ($U=.002$) is predicted to last for 31 iterations. From a theoretical standpoint, these results are quite useful for understanding when and why polarized coalitions might endure for relatively long periods of time.

Ideally, we would also like to know the *relative* importance of the three variables for explaining coalition durability. The standardized coefficients in Table 2 yield some information about relative importance, but we urge caution in interpreting their values as indications of importance. The independent variables are correlated among themselves, because polarized coalitions must be fairly risk acceptant if the coalitions are to survive even one iteration. Consequently, determining which variables are most important is somewhat problematic. Nevertheless, the beta coefficients in Table 2 suggest that all three variables are of relatively equal importance, with polarization carrying just slightly more weight.

The fact that uncertainty and risk are virtually as important as polarization suggests that they are indeed useful additions to any model of coalition durability. Not only can substantial gains in explanatory power be expected through an examination of the risk attitudes and uncertainty of coalition members, but some of the anomalies resulting from Axelrod's polarization hypothesis can be accounted for as well. The simulations indicate very clearly that polarized coalitions will endure for relatively long periods of time if the members are either risk acceptant or else quite certain about the leader's policy position. Although these theoretical results certainly do not suggest how one might empirically operationalize the concepts of risk acceptance, risk aversion, and uncertainty, they do at least suggest that these concepts are useful starting points for future research.

An Empirical Application

One can always resort to idiosyncratic explanations to account for instances of open and durable coalitions in the real world. By introducing risk and uncertainty into explanations of coalition durability, however, seemingly disparate and idiosyncratic explanations can be subsumed under a single, general theory. One historical example—the Bidault II (1949-1950) government in France, nicely illustrates this point.

Even though the second Bidault government was an open coalition because of the exclusion of the Gaullists, it managed to endure for a total of nine months, a relatively long time for any government of the unstable and turbulent Fourth Republic. In comparison, the Bidault I government lasted only six months, and the Schuman government only seven months. And, unlike the Bidault II coalition, each of these other coalitions was connected.

The exclusion of the Gaullists, and hence the durability of the Bidault II coalition, fits well with predictions from the theory of risk and uncertainty. Under the theory of risk and uncertainty, a party will be excluded from a potentially connected coalition if that party's behavior is uncertain and unreliable, or if the party is extremely inflexible in its demands (i.e., risk averse). In the early years of the French Fourth Republic, the Gaullists fit both of these conditions—they were both intransigent and unreliable coalition partners.

Much of the apparent intransigence of the Gaullists resulted from personal characteristics of General de Gaulle himself. He was perceived by many to be arrogant and unyielding, owing in part to his military background. Williams (1964, p. 133), in one description of de Gaulle in 1946, wrote that, "Conciliation and compromise had never been marked features of his character, and no doubt he made too little allowance for the difficulties of partners who had to meet a totalitarian and demagogic rival on the electoral battlefield." Indeed, de Gaulle had a difficult time making the transition from the battlefield to the political arena. Only three months after the 1945 election, an impatient de Gaulle, frustrated by the difficulties of political negotiation, withdrew from politics and returned to private life.

Contributing to the perceived unreliability of the Gaullists were two factors: de Gaulle's lack of support for the first two constitutional drafts and, later, the inability of his party to discipline its members in parliament. De Gaulle's silence on the first constitutional draft put the MRP (Christian Democrats) in the awkward position of being the chief opponent of the draft, a document largely drawn up by the Marxist parties. Later, when the MRP compromised with the leftist parties in an effort finally to put an end to provisional government, de Gaulle publicly denounced their proposals, thereby once again undercutting and embarrassing the MRP. Naturally, the MRP felt betrayed, because de Gaulle had given them no support either in formulating a constitutional draft or in opposing Marxist influence. When de Gaulle re-entered politics in 1947, launching his Rally of the French People (RPF) in an appeal to reform the constitution and combat communist

influence, he attracted the support of many MRP voters. Most of these disaffected voters were convinced that the MRP was incapable of dealing effectively with the Communists, a perception created, not coincidentally, by de Gaulle's repeated lack of support for the MRP. Thus, despite its ideological similarities with de Gaulle, the MRP leadership found not only that de Gaulle's support was undependable, but also that de Gaulle was in fact a threat to their party.

At its inception in 1947, the RPF was claimed to be an "inter-group," not a party, so that deputies from other parties could belong to the RPF and still remain members of their old parties. But it was this heterogeneity of the RPF that contributed to its unreliability as a coalition partner. Even though the RPF captured 150 out of 320 senate seats in the 1948 election, most of those elected were interested only in the electoral support afforded by the popular de Gaulle. When the RPF leadership attempted to enforce discipline among these senators, over two-thirds defected from the RPF cause and returned to their original parties.

Clearly, then, by 1949 the Gaullists had established a reputation of intransigence and unreliability. They appeared to be a party that was not only unwilling to negotiate and compromise, but also a party with uncertain political orientations that could not be relied upon for support on the important referenda questions of the day. It is little wonder, therefore, that Georges Bidault, as leader of the MRP, chose to exclude the Gaullists when forming his cabinet in 1949.

On the surface, this story about the Bidault II government is nothing more than that—just a story, a discrete account of historical events. When viewed in the context of a theory of coalition durability based on risk and uncertainty, however, the story becomes a coherent and general explanation of an important political event. Similar stories can be told about other coalitions. The Liberal Party, for example, appears to have been excluded from the Zoli government (1957-1958) in Italy because of its aversion to risk taking. Axelrod (1970, p. 181) has indicated that the Liberals had "unrealistically high aspirations for cabinet positions." And the Rafi apparently were excluded from Levi Eshkol's (1966) government in Israel because of their unreliability and unpredictability. The Rafi had previously defected from Maipai, Israel's main labor party (Aronoff, 1978). Thus, to the extent that the theory of risk and uncertainty allows one to integrate seemingly isolated stories into a larger, general, and coherent picture of coalition durability, the theory is indeed useful, and because the broad logic of the model offers an intuitively plausible account of real world

events, the model holds promise for empirical application.

Conclusions

Only very recently have political scientists recognized the need to develop models of coalitional processes that are both dynamic and probabilistic. Cioffi-Revilla (1984), for example, has formulated a model of Italian cabinets in which longevity is a random variable, so that the duration of cabinets is viewed stochastically rather than deterministically. Browne, Frendreis, and Gleiber (1984, p. 191) have suggested that "the stability of cabinets is appropriately modelled as a problem of individual decision making under uncertainty." We concur wholeheartedly. Our results suggest quite clearly that uncertainty, as well as considerations of risk, are indeed important additions to models of coalition durability.

We believe that the model developed here captures the essence of much of what politics is about. To our minds, politics is a dynamic process filled with uncertainty. From the standpoint of political leadership, uncertainty provides opportunities for manipulation and deception; or as Downs (1957, p. 83) has eloquently pointed out, "Uncertainty gives rise to persuasion." It is the process of persuasion, springing from uncertainty, and its implications for the durability of coalitions that we have sought to capture.

Additional levels of complexity certainly can, and should, be built into the model as developed here. One useful modification might be to assign policy preferences to the leader, rather than have the leader concerned only about remaining in power. This could be accomplished within the present framework simply by requiring that the leader satisfy some minimal policy expectations of his own in addition to those of the other members of the coalition. The policy preferences of the leader would then enter into the model as one more factor affecting longevity.

A second useful extension, one that would also move the model closer to the real world, would be to weight payoffs differently. Two approaches are possible here. One would be to weight recent payoffs more heavily than earlier payoffs under the assumption that politicians, like voters, are concerned with the question, "What have you done for me lately?" A second approach would be to weight payoffs in essentially a random fashion under the assumption that external events impinge upon coalition members in rather unpredictable ways, but nevertheless ways that make certain outcomes more important than others. This latter modification would also move the model in the

direction of an "events approach" as proposed by Browne et al. (1984).

Ultimately, the robustness of the simulation results will be demonstrated only if the results hold up under further development and modifications in the assumptions. In the meantime, however, the results suggest strongly that if one is addressing a world of rational maximizers of expected utility, who do practice some Bayes-like probability estimation procedures, there is substantial explanatory power to be gained by investment in the operational assessment of risk and uncertainty in coalition maintenance situations.

Appendix

THEOREM 1. There exists a finite t such that $S^t \neq \phi$ and $S^{t+1} = \phi$.

Proof. We have the following assumptions:

(i) $P_1^* > 0$ and $P_2^* < 1$,

(ii) $\mu_1 > \mu_2$.

Then, by the two decision rules, if $\lim_{t \rightarrow \infty} P_{1t} = 0$

or $\lim_{t \rightarrow \infty} P_{2t} = 1$, the coalition will be terminated. Thus, we show that in the limit, $P_{1t} = 0$ or $P_{2t} = 1$. We consider just two cases: $x^* > \mu_1$ and $x^* < \mu_1$. (Notationally, we now shorten $f_i(x; r, t)$ to $f(x)$.)

Case 1. For $x^* < \mu_1$, let $\epsilon = \frac{1}{2}(\mu_1 - x^*)$, so that

$$\int_0^{\mu_1} f(x) dx > \int_{x^* - \epsilon}^{x^* + \epsilon} f(x) dx.$$

And in the limit,

$$\begin{aligned} 1 &> \lim_{t \rightarrow \infty} \int_0^{\mu_1} f(x) dx \\ &> \lim_{t \rightarrow \infty} \int_{x^* - \epsilon}^{x^* + \epsilon} f(x) dx. \end{aligned}$$

The right-most term in the above inequality, however, must be equal to one, because the mean of each follower's subjective distribution converges in probability to x^* . Consequently, the value for the middle term in the above inequality must also be one in order to make the inequality true. Since

$$\int_0^{\mu_1} f(x) dx = 1 - P_{1t}, \quad \lim_{t \rightarrow \infty} P_{1t} = 0,$$

and the sequence ends, because $P_1^* > 0$ by (i) above.

Case 2. For $x^* \geq \mu_1$, we know that $x^* > \mu_2$ by (ii) above. Let $\epsilon = \frac{1}{2}(x^* - \mu_2)$, so that

$$\int_{\mu_2}^1 f(x) dx > \int_{x^* - \epsilon}^{x^* + \epsilon} f(x) dx.$$

In the limit

$$\begin{aligned} 1 &> \lim_{t \rightarrow \infty} \int_{\mu_2}^1 f(x) dx \\ &> \lim_{t \rightarrow \infty} \int_{x^* - \epsilon}^{x^* + \epsilon} f(x) dx. \end{aligned}$$

Therefore, $\lim_{t \rightarrow \infty} P_{2t} = 1$, and the sequence ends since $P_2^* < 1$ by (i) above.

THEOREM 2. If $S_t \in S^t$ and $S_{t+1} \notin S^{t+1}$, $S^{t+1} = \phi$.

Proof. As before, assume Member 1 prefers 1s, and Member 2 prefers 0s. Let a sequence, S_t , of r 1s and $t-r$ 0s be denoted by $S_t = (r, t-r)$.

If $\mu_1 > \mu_2$ (as in Theorem 1), then $S_t \in S^t$ and $S_{t+1} \notin S^{t+1}$ implies

(i) $P_{2t+1} > P_2^*$ if $S_{t+1} = (r+1, t-r)$.

Similarly, $S_t \in S^t$ and $S_{t+1} \notin S^{t+1}$ implies

(ii) $P_{1t+1} < P_1^*$ if $S_{t+1} = (r, t+1-r)$.

First, let S'_t be any sequence such that $S'_t \in S^t$, where r' is the number of 1s in S'_t . If $r' > r$, then $r'+1 > r+1$ and $S'_{t+1} \notin S^{t+1}$ by (i) above. And if $r' < r$, then $r'+1 \leq r$ and $S'_{t+1} \notin S^{t+1}$ by (ii) above.

Consequently, $S'_{t+1} \in S^{t+1}$ requires $r < r'+1 < r+1$ which is impossible. Hence, if $S_{t+1} \notin S^{t+1}$, $S'_{t+1} \notin S^{t+1}$.

Next, let S'_t be any sequence such that $S'_t \in S^t$ and $r'=r$, but let the order of 1s and 0s in S'_t differ from the order in S_t . Then $P'_{1t} = P_{1t}$ since by Definitions 6 and 8 P_{1t} depends only on the parameters r and t , not on the order of the t elements. Hence, if $S_{t+1} \notin S^{t+1}$, then $S'_{t+1} \notin S^{t+1}$ and $S^{t+1} = \phi$.

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The Decline of Class Revisited: Class and Party in England, 1964-1979

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Class has long been the preeminent source of political conflict in industrial society, but its electoral influence has declined in recent years. The sources of the decline are not yet firmly established, and moreover the implications for political parties remain unclear. The decline-of-class hypothesis states that parties on the left will decline as the working class becomes more affluent and adopts middle-class styles of conduct. By contrast, the party-appeals hypothesis suggests that as the electorate becomes more middle class, parties of the left will alter their appeals to encompass the growing middle class and so offset the shrinkage of their traditional working-class constituency. This article applies multivariate analysis to survey data collected in England between 1964 and 1979 to test four specific hypotheses derived from the two scenarios. The results support the decline-of-class theory's prediction that economic development erodes the working-class bases of left-wing parties, but not its claim that the left-wing party's vote declines proportionately. Rather, the results suggest that parties are apparently able to change their appeals to reduce their losses, as argued by the party-appeals theory, but not to eliminate them. It seems that there are restraints on parties' ability to change their appeals, limitations not envisioned by the party appeals theory.

Historically, social class emerged as the primary basis of political conflict in many western societies after the Industrial Revolution. With the growth of a large, urban working class in the late nineteenth century, parties of the left emerged to make explicit appeals based on working-class interests (LaPalombara & Weiner, 1966; Lipset & Rokkan, 1967). In many countries these appeals were eventually successful, sweeping the left into power for long periods. But at the apogee of these parties' power in the decades after the Second World War, sustained economic development began to erode their social base. With improving material standards of living, growing white collar employment, and the pervasive diffusion of

middle-class styles of behavior, it seemed that many "natural" working-class supporters of the left were becoming middle-class and eventually would defect to parties of the right. This decline-of-class theory gained wide currency in the 1960s and was used to underpin many works speculating on the transformation of party systems and the nature of politics in general in advanced industrial democracies (see, for example, Abramson, 1971; Alford, 1963; Bell, 1960; Goldthorpe, 1969; Korpi, 1978; Lipset, 1964; Lockwood, 1960; Myers, 1970; and Runciman, 1964).

Underlying all of these works were two fundamental theses:¹

HYPOTHESIS 1. Economic growth reduces the

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¹An alternative argument is that class cleavages in politics have declined because objective differences between classes decline with economic development. Although this is not an implausible argument, in fact class cleavages seem to have been stable, at least since the Second World War. The best evidence on this question is from studies of the life-chances of children born into different classes, that is, studies of social mobility, since there are baseline data from the early postwar period and, in addition, the topic lends itself to retrospective inquiries. Evidence from the United States, Britain, Canada, and industrial societies generally all suggest that there has been little or no consistent change (Blau & Duncan, 1967, pp. 177-182; Goldthorpe et al., 1980; Hauser et al., 1975; Hazelrigg & Garnier, 1976; Hope, 1981; McRoberts & Selbee, 1981).

size of social groups that traditionally support left-wing parties.

According to this hypothesis, the vote for the left should decline proportionately because no modification of the "normal," long-established link between class and party is anticipated by it:

HYPOTHESIS 2. The electoral strength of left-wing parties declines in direct proportion to the shrinkage of the social groups that traditionally support them.

There is, however, a contrary view based on the argument that the appeals political parties make, and hence their sources of electoral support, are not fixed and immutable but rather are the result of conscious, rational choices by party elites. In a classic analysis, Downs (1957, p. 28) argued that parties rationally choose to maximize their vote by appealing to the greatest number of electors: "Parties formulate policies in order to win elections, rather than win elections in order to formulate policies." Rather than being mere passive recipients of support from existing social groups, parties are active agents, consciously choosing what political stance they will adopt (Sartori, 1969, 1976). This argument implies that parties of the left will not stand passively by as their traditional supporters become less numerous. Instead they will try to make good the loss by increasing their support in the growing middle class, either through an implicit appeal to middle-class interests or, with less risk of alienating traditional supporters, by eschewing class altogether and emphasizing issues (such as the environment, nuclear energy, or women), which cut across class. As a result, the political distance between the middle and working classes will narrow as parties of the left shift their ground.

The logic of Downs's argument goes further, implying that, in spite of the changes imposed by economic growth, the vote for the left will not in fact decline. Drawing on economic theories about the optimal geographical location of competing firms, Downs shows that the equilibrium position for two parties contesting the vote along a single left-right dimension leads to an approximately equal division of the vote, with one party offering an appeal somewhat to the left of the center and the other offering one somewhat to the right. If the population shifts to the right, the left party must, for fear of losing votes, shift its appeal to the right, and, crucially, the conservative party must also shift its appeal to the right for fear of losing more from its normal right-of-center constituency (now closer to the left's new position) than it could gain by not moving. Hence, the left is able to make new converts on its side of the new

center and the equal division of the vote is re-established. In short, this party-appeals theory, as we shall call it, argues that:

HYPOTHESIS 3. Economic growth leads parties of the left to mute their appeal to the working class and hence reduces class polarization in politics.

and

HYPOTHESIS 4. The division of the vote between left- and right-wing parties is unaffected by economic growth.

The decline-of-class theory and the party-appeals theory thus lead to several important and empirically testable propositions about the effects of economic growth on politics. Two of these propositions are different but perfectly compatible (Hypotheses 1 and 3), whereas the other two are contradictory (Hypotheses 2 and 4). In this article we test these four hypotheses by analyzing the changing relationship between class and party in England over the period 1964 to 1979.

The English Case

England is an important test case for these theories because the British system is the prototypical example of class-based politics, as well as one of the oldest democratic systems and the source of many of the political traditions and institutions of the Anglo-American world. The dominant class alignment emerged between the First and Second World Wars (Wald, 1983); it is best documented in a series of analyses published in the 1960s, all of which contradicted the decline-of-class theory and validated the frequently quoted aphorism: "Class is the basis of British party politics; All else is embellishment and detail" (Pulzer, 1967, p. 98; see also Abrams & Rose, 1960; Alford, 1963). So strong was the electoral influence of class that whatever minor shifts of support took place, its impact seemed relatively consistent from election to election. Despite the alternation of parties in government, class remained by far the most significant determinant of voting choice in the British electorate, thereby underpinning the general stability of the party system.

Over the past decade, however, this static view of the relationship between class and party, and its political consequences, has changed profoundly as the relationship itself has weakened. Butler and Stokes (1974, p. 203), for example, see "the declining strength of the association between class and party (as) one of the most important aspects of political change in this decade." Others have

reached similar conclusions (Alt, 1984; Cain, 1978; Crewe, Sarlvik & Alt, 1977, p. 167; Franklin, 1982; Franklin & Mughan, 1978; Miller, 1978, pp. 263-264; Rogowski, 1980; Rose, 1980, p. 149). Even though there may now be widespread support for the first part of the decline-of-class thesis, the second, that is, that Labour must inevitably lose support, remains less clear. Several studies of attitudinal changes in the mass public are consistent with the view that the Labour party's support must decline (Crewe, Sarlvik, & Alt, 1977; Goldthorpe et al., 1971; McKenzie & Silver, 1968; Nordlinger, 1967), but it has also been argued that both parties have suffered equally in the face of economic and social change (Crewe, 1977). It is apparent, therefore, that there has been no rigorous empirical test of the overall thesis, much less any plausible estimate of the magnitude of the effects of this change. One aim of this study is to fill both these gaps.

Another major question concerns the specification of which aspects of class are changing, and with what impact upon voting choice. It is now generally agreed that several dimensions of class are politically salient in Britain, notably occupation, union membership, education, class self-image, and home ownership.² Previous analyses, such as Butler and Stokes's (1974) classic, *Political Change in Britain*, typically look only at one class variable (most commonly class self-image or occupation dichotomized into manual versus nonmanual), and trace changes in its relation to the vote over time. This approach is inadequate in that it neglects other important aspects of class and, insofar as there are changes in other correlated aspects of class, estimates based on such a simplistic model will necessarily be biased. For example, an increase in education or middle-class self-image among people who are objectively manual workers will make it seem that manual workers are no longer so politically different from nonmanual workers, even if in fact there is no such change (because the manual group is increasingly "contaminated" by well-educated workers who identify with the middle class). Our analysis circumvents this problem by

using powerful multivariate techniques to deal simultaneously with the several politically relevant aspects of class.

Previous estimates have also been flawed by a failure to deal with several other politically crucial variables which are deeply entwined both with class and with recent historical changes. First, and particularly central to the decline-of-class thesis, is the very important dampening effect on change produced by political socialization. As parties of the left came to power after the Second World War, an increasing proportion of children, being raised in left-wing homes, became strongly disposed to support the left (Butler & Stokes, 1974, pp. 229-230; Jennings & Niemi, 1978; Rose, 1974). This change continues unabated, so that new entrants to the British electorate come increasingly from established Labour families. We take this potentially confounding change explicitly into account in our analysis and provide precise estimates of its effects. Second, there are other important changes in society that may affect the relationship between class and party. In particular, trade union membership mediates this relationship and, importantly, it has been spreading from the manual workforce into white-collar occupation (Bain, 1970). This growth in union membership should redound to the overall benefit of the left, so it too should be controlled in the analysis. Again, we provide estimates of the effect of this change on the Labour vote.

After describing the variables in our model of class voting and our methods of analysis, we first present the model estimated over the pooled 1964, 1970, 1974, and 1979 surveys in order to provide a general picture of the enduring character of the class-party relationship in England during this 15-year period. We then present an analysis of the changes over the four elections and, finally, discuss the implications the findings have for the decline-of-class and party-appeals theories.

Data, Measurement and Method

Data

The analysis is restricted to England because popular support for Scottish and Welsh nationalism in 1974 and 1979 means that the party systems in these countries are no longer strictly comparable: "The growth of nationalist voting (in Scotland and Wales) and national trends in support for non-nationalist parties imply that a Britain-wide analysis would simply mix and confuse a variety of divergent trends" (Miller, 1973, p. 257; see also Drucker, 1979). The time span covered is 1964 to 1979, the maximum time period for which suitable data are currently available. For the sake of brevity, we analyze change over

²In this article, we unfortunately have not been able to consider class in either Marx's sense of ownership of the means of production or Dahrendorf's sense of authority (e.g., Robinson & Kelley, 1979). The required questions were not asked in all of the four surveys we analyze so, to maintain comparability over time, we were forced to omit them throughout, although we have dealt with them at some length elsewhere using only the 1979 data (Kelley & McAllister, in press). The present results are therefore restricted to class in a limited sense of the word, albeit the sense in which it is traditionally used in British political sociology.

only four of the six general elections that have taken place over this period: 1964, 1970, October 1974, and 1979. Although this is a relatively short period in which to study the changes resulting from economic growth, it is nonetheless long enough for real changes in standard indicators of it to manifest themselves, and our estimates of the effects of these changes provide the best evidence yet available concerning the likely impact over a longer period of change (see Table 1).

The data come from four separate surveys: the 1964 and 1970 electorate cross-sections of Butler and Stokes (1974) ($Ns = 1,769$, and $1,845$); the October 1974 British Election Study ($N = 2,365$); and the 1979 British Election Study ($N = 1,893$). Data for the 1983 election are not available at the time of writing. Each survey involved a randomly drawn sample of the British electorate and was put into the field immediately after the general elections of those years. For comparability, we have used the 1964 post-election study in preference to the 1963 pre-election study. Completion

rates for the surveys were 68%, 70%, 75%, and 72%.

The analysis was restricted to those who voted (approximately three-quarters of the total) and reported their class identification and their (or their spouse's) occupation. With these restrictions, the Ns are 1,208, 1,186, 1,503, and 1,207.

Measurement

The dependent variable is the recalled vote in the general election that had just been held. It is coded "1" if voted Labour and "0" if voted Conservative. Liberal voters were coded ".5," thereby in effect placing them halfway between Labour and Conservative, which is reasonable in terms of their attitudes and social characteristics (Alt, Crewe, & Sarlvik, 1977; Himmelweit et al., 1981, chap. 10; Lemieux, 1977). Alternative coding schemes (e.g., combining them with Conservatives or omitting them from the analysis altogether) led to the same substantive conclu-

Table 1. Variables, Scoring, and Means

Variable	Scoring	Means			
		1964	1970	Oct. 1974	1979
Vote					
Labour vote	1 = Labour; .5 = Liberal; 0 = Conservative	.54	.49	.52	.43
Family background					
Father white collar worker	1 = white collar worker 0 = otherwise	.20 (.21)	.23 (.21)	.25 (.25)	.28 (.28)
Father Labour partisan	1 = Labour; .5 = Liberal 0 = Conservative	.51	.56	.56	.56
Class position					
Head of household white collar worker	1 = white collar worker 0 = otherwise	.37 (.34)	.39 (.34)	.40 (.40)	.42 (.41)
Owner-occupier house tenure	1 = owner-occupier 0 = otherwise	.39	.52	.57	.61
Class self-image^a	1 = middle class self-image; 0 = working class self-image	.32 (.30)	.33 (.36)	.34 (.34)	.35 (.34)
Education^a	Years of full-time schooling	9.81 (9.64)	10.03 (10.41)	10.18 (10.00)	10.36 (10.32)
Other^b					
Trade union member	1 = member 0 = otherwise	.22	.26	.25	.31

^aThe means for these variables are estimates, for the reasons noted in the text. See note 3 for details of their calculation. The actual means are shown in parentheses.

^bFor simplicity, age, sex, and region are omitted. Their effects are uniformly small and generally statistically insignificant. Including them has no noticeable effect on the results for the other variables.

Sources. Sources for all tables are: 1964 and 1970 electorate cross-sections of Political Change in Britain Survey ($Ns = 1,769$ and $1,845$); October 1974 and 1979 British Election Studies ($Ns = 2,365$ and $1,893$).

sions (details available on request). Nonvoters and those who did not answer the voting questions, a little over 20% of the total, are omitted from the analysis.

The variables used in the analysis, together with their means, are given in Table 1. The independent variables were selected because of their proven or potential impact on party choice and can be divided into three categories. The first, family background, is measured by father's occupation and partisanship. Mother's partisanship was not available in all the surveys and was therefore excluded. The second, the respondent's class position, is measured by four variables. Occupation is measured by the categorical distinction of whether or not the head of the household is a white-collar worker; a continuous measure of occupational status has no effect on voting (Kelley & McAllister, *in press*). The other dimensions of social class are tapped by type of housing tenure, class self-image, and education. Type of housing tenure is included because the division between owner occupiers and council tenants has proved to be an important and enduring electoral influence in Britain. Butler and Stokes (1974, p. 107), for example, suggest that "housing has more to do with defining the sub-cultures of social class than all else but occupation itself." Income is excluded because it has no appreciable effect on the vote once other variables are considered. The final variable is trade union membership, which is included for the reasons noted above. For simplicity, age, sex, and region, which turn out to be of little or no political relevance, are excluded from the analysis. The correlations and standard deviations for these variables are given in Table A1.

The analysis is based on ordinary least squares regression methods, which assume that relations are, to a reasonable approximation, linear and additive (Hanushek & Jackson, 1977; Kmenta, 1971). There are well-known reservations about using regression when one dependent variable is dichotomous, but these difficulties are in practice minimal unless the marginals are highly skewed, which is not so in this case even when Liberal votes are omitted to make it dichotomous (Gillespie, 1977; Kmenta, 1971, pp. 425-427). Missing data (save on vote, occupation, and class self-image) are treated by the "pairwise present" procedure, which is statistically preferable to the usual alternatives (Hertel, 1976). Alternative analyses using listwise deletion lead to identical substantive conclusions (details available on request). For the analysis of Figure 1, a single model is used only for heuristic purposes because, as we show, there are differences in slopes in different time periods. The central results (given in Figures 2 through 6 and Table A2), are based on models

estimated separately for each survey, in effect allowing for all possible interactions between date and other variables.

Multicollinearity is not a serious problem here, because the highest correlations between independent variables are approximately .4, which is well below the very conservative figure of .5 suggested by Hanushek and Jackson (1977, p. 89). Nor do minor changes in model specification or measurement lead to any appreciable changes in the estimates, another clue to the absence of serious multicollinearity (Hanushek & Jackson, 1977, p. 90).

Although England has undoubtedly become steadily more middle class, there remains some uncertainty in estimating the magnitude of these small changes from survey data because of sampling error, differences in sample coverage, and differences of detail in the way the questions were asked and coded. We therefore assume that the actual data for the four surveys represent imperfect estimates of the underlying trend and attribute the minor fluctuations observed in them to problems inherent in the data. Specifically, we argue that this will affect four variables—father's occupation, head-of-household's occupation, class self-image, and education—and that changes in each will reflect long-term shifts in the economy, whereas any short-term fluctuations or reversals of the trend are unlikely. We therefore estimate the underlying trend by fitting an ordinary-least-squares regression line through the observed means, and use these predicted means in all substantive analyses.³ By contrast, small fluctuations in trade-union membership and house tenure are more likely to be genuine, because they can be influenced by short-term political and socioeconomic change, so we have not smoothed trends in these variables. The use of actual instead of fitted means leads to some differences in our results but leaves the major conclusions unchanged (details available on request).

³The predicted means were calculated as follows. For example, the observed mean years of schooling are 9.64 in 1964, 10.41 in 1970, 10.00 in October 1974, and 10.32 in 1979. We treated these as four data points and estimated a linear regression predicting education on the basis of date obtaining:

$$\text{Estimated Schooling} = -61.63$$

$$+ .03638 (\text{Date of Survey}).$$

This implies an annual growth in the average level of schooling in the British population of slightly under 4% of a year, or something over a third of a year per decade. Evaluating the regression equation for various dates yields predicted mean years of schooling of 9.81 years in 1964, 10.03 in 1970, 10.17 in 1974 and 10.36 in 1979.

Class Voting in England

The Model

Before the question of change can be addressed, we need to specify how the various dimensions of social class stand in relation to both each other and to the dependent variable, party choice. It is also useful to identify which of them have a significant impact on party choice in the period from 1964 to 1979. Our model (Figure 1) is fitted to the 1964, 1970, October 1974, and 1979 surveys simultaneously and so (for the moment) ignores changes over time. The model shows the equivalent of standardized partial regression coefficients (betas, or paths) that are directly comparable so that the relative importance of the various dimensions of class can be assessed. For clarity, only effects of the magnitude of .10 or more are shown here (although all are included in subsequent analyses). Figure 1 adds little to the conventional wisdom about electoral behavior in general and British electoral behavior in particular. The single most important determinant of voting choice is respondents' political socializa-

tion, as measured by father's partisanship, whereas father's occupation influences the vote only indirectly through its impact on respondents' education and occupation. Class self-image, home ownership, and occupational class are of roughly equal direct influence. Education has only an indirect influence on the dependent variable. This model is the basis for our analysis of the changes in the class-party relationship between 1964 and 1979.

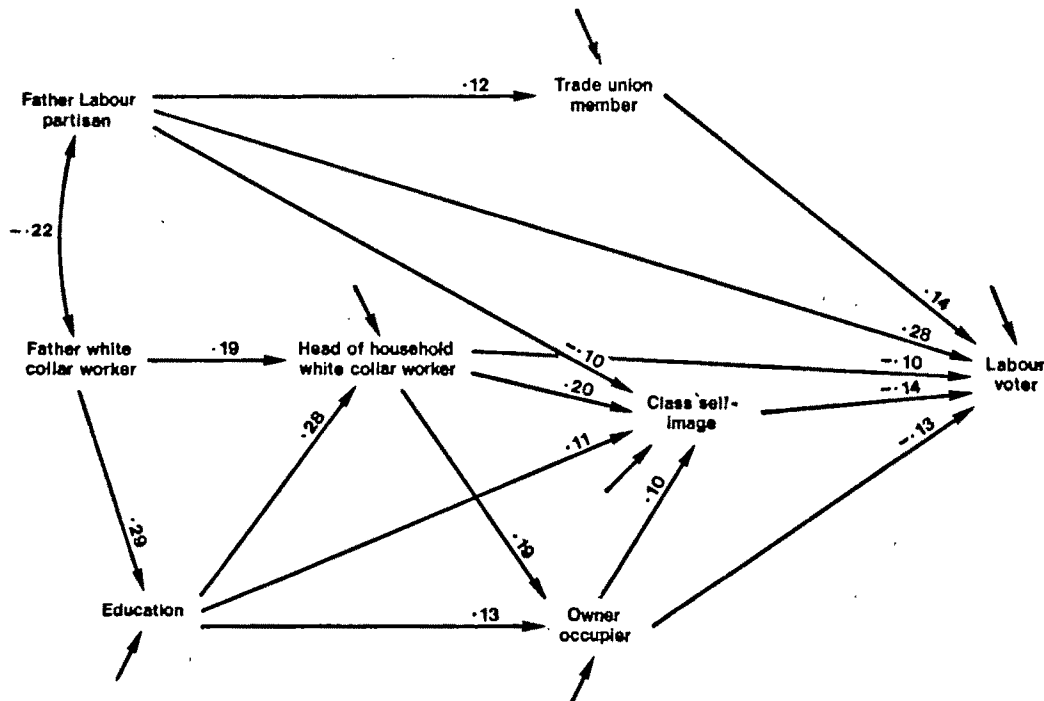
The Weakening Relationship between Class and Party

We can see the extent to which social structure has changed in England between 1964 and 1979 by comparing the means of the class variables for the four surveys.⁴ Figure 2 shows there has been a

⁴We take 1964 as the baseline, which seems a reasonable choice conceptually—there was apparently no substantial change in the relation between class and party in the decades preceding it (Alford, 1963)—and a necessary choice practically, because there were no suitable national surveys in the 1950s or earlier.

Figure 1. A Basic Model of British Electoral Behavior, with Standardized Partial Regression Coefficients for 1964, 1970, October 1974 and 1979 Pooled Sample.

Only major influences are shown ($\beta > .10$).
All coefficients shown are significant at $p < .01$; $N = 15,104$.

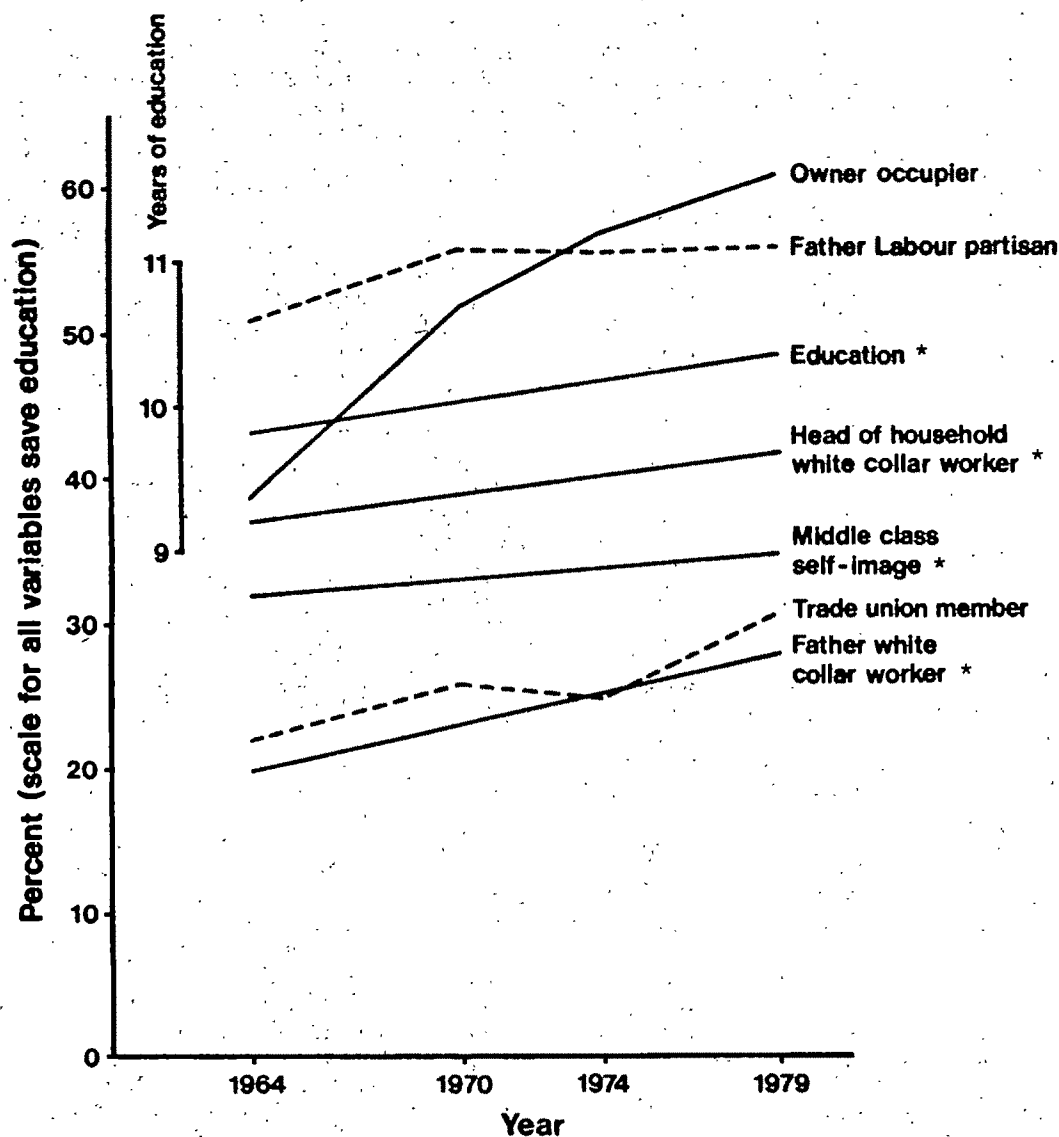


slow but steady embourgeoisement of the English electorate (see also Halsey, 1972; Rose & McAllister, 1982). For example, in 1964, 37% of the electorate were in white-collar occupations; by 1979 this number had increased to 42%. Similarly, in 1964 the average voter had 9.8 years of full-time education, compared to 10.3 years for his 1979 counterpart. The largest increase has occurred in home ownership: in 1964 just under 40% of voters were owner occupiers, compared to 60% in 1979.

In addition to these changes in class variables, there is a small but politically crucial increase in the proportion of the electorate coming from Labour homes (up 5%). Trade union membership appears to be marginally more susceptible to government policy, since it declined slightly during the 1970 to 1974 Heath Conservative government and resumed its slow upward movement when Labour regained power.

The decline-of-class theory was correct, then, in holding that the working class slowly shrank in

Figure 2. Changes in the Mean for Various Socioeconomic Variables (Solid Lines) and Other Variables (Broken Lines) in England, 1964-1979



size between 1964 and 1979. But what were the political implications, if any, of this shrinkage? Let us begin by asking (hypothetically) what would have happened if the 1964 relation between class and the vote had subsequently remained unchanged in strength. To take education as an example, in 1964 each year of education reduced an elector's probability of voting Labour by about 7%. As we have just seen, educational levels were increasing and by 1970 the average voter had an additional half a year of schooling. So if the 7% educational differential remained unchanged, then the Labour vote should, hypothetically, have dropped by 3.5% (7 times the one half, shown by the solid line in Figure 3). That is a very substantial drop in electoral terms, representing more than the winning margin in two-thirds of the general elections in this period. Other things being equal, it alone would make it very difficult for Labour to gain power again and would dramatically support the decline-of-class theory's Hypothesis 2.

Labour did regain power, however, and it did so in part because the relationship between education and voting changed dramatically, the 7% differential in 1964 falling to approximately 2% in subsequent elections (see Figure 4). So the loss to Labour was much less than it would have been, amounting to about 1% in 1979 (namely, the differential of 2% times the half-year increase in education, with the results shown by the broken line in Figure 3). This is as much as the class-appeals hypothesis would lead us to expect, and the most likely explanation is that it is the pay-off of the Labour Party's conscious decision to tone down its earlier appeal to less-educated voters (Kavanagh, 1982, pp. 104-107). Whether Labour managed to defuse the issue entirely is, however, another question. The estimated effects for 1970, 1974, and 1979 (shown in Figure 4) are approximately 2%, which implies a slight loss to Labour. Although small, all three are significantly different from zero at $p < .05$. In other words, the evidence suggests that the growth in educational levels still worked somewhat to Labour's disadvantage despite the party's attempts to neutralize it.

Changes in other aspects of class, with the exception of home ownership, had much less of an impact than changes in education; none would have led to a change of as much as 1% in the Labour vote had 1964 patterns prevailed. This proviso notwithstanding, however, the pattern is similar to that for education. Both the growth in the white-collar workforce and the increase in the proportion of the population identifying with the middle class would have harmed Labour had (hypothetically) the link between class and vote remained unchanged after 1964. Changes in home

ownership, by contrast, were more harmful in their impact than changes in education. Figure 2 shows that the increase in home ownership was dramatic, and the difference in voting behavior between owner occupiers and council house tenants increasingly large (Figure 4). But, unlike on other class questions, the Labour Party seems not to have been able so easily to mute its class-based appeal, so that although differences based on home ownership declined in 1970, they rose dramatically toward the end of the 1970s. Indeed, by 1979, Labour lost more votes among home owners than it would have done had 1964 patterns persisted (see Figure 3).

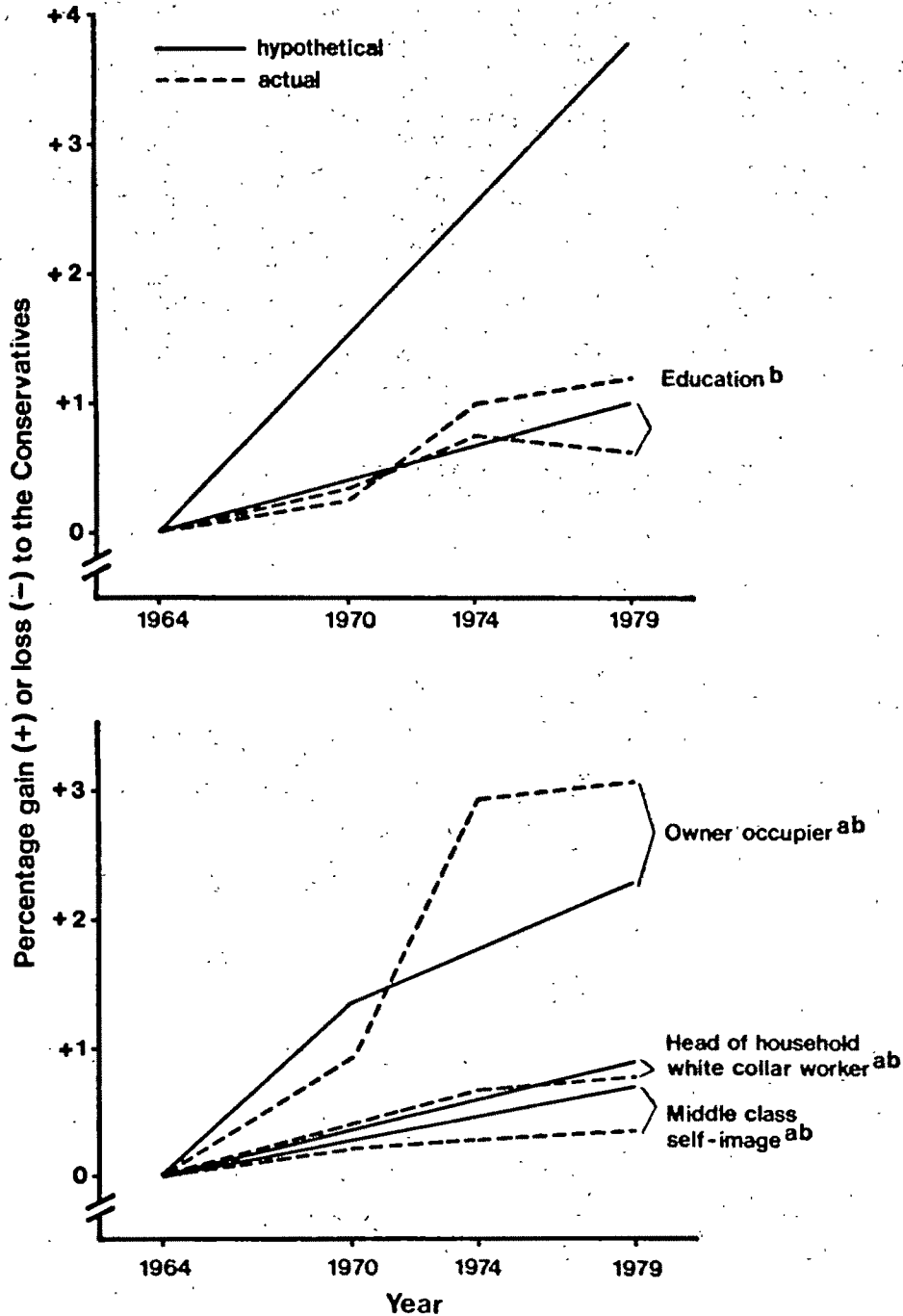
This is clear evidence against the basic premise of the party appeals theory set out in Hypothesis 1. But it is an informative exception to the general pattern. On most class issues it seems that the Labour Party makes the running, which is perhaps not surprising because the party originated from a working-class base and has consistently exploited class issues. The Conservatives, on the other hand, have for generations studiously ignored such issues and raised other themes, such as leadership, patriotism, and religion because they would lose any election fought on strictly class lines. Consequently, when the Labour Party mutes its previously strident appeals to class interests, the Conservatives cannot easily retaliate by stressing class concerns themselves. The Labour Party, in contrast, is able to change its emphasis relatively quickly and thereby reduce the impact of unfavorable changes in the class structure.

But housing is different from the traditional aspects of class. In Britain there are two dominant types of housing: owner-occupied and rented council housing, the latter heavily subsidized by the state. Owners are traditionally Conservative in partisanship, and council house renters traditionally Labour (Butler & Stokes, 1974, p. 107; Rose, 1982, Table 2). However, home ownership is widely valued in the working as well as in the middle class, and the Conservatives have exploited the issue's cross-class appeal to dominate the housing question. First, they have strongly encouraged home ownership and were the main architects of the dramatic growth in home ownership in the 1960s and the early 1970s. Second, in the early 1970s they introduced the policy of selling council houses to their tenants at reduced prices, often over the vocal opposition of Labour-controlled local councils (Berry, 1974). As a result, the Conservatives have increased the electoral salience of housing to their own advantage (Dunleavy, 1979; McAllister, 1984).

By contrast, Labour has traditionally supported council housing, originally as a social welfare measure, and has long been identified with opposition to Conservative initiatives to reduce the size

Figure 3. Conservative Gains Due to Socioeconomic Changes in England, 1964-1979.

Solid lines are hypothetical gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.



^{a,b}1979 hypothetical value (a) or actual value (b) significantly different from 1964 at $p < .05$, one tailed.

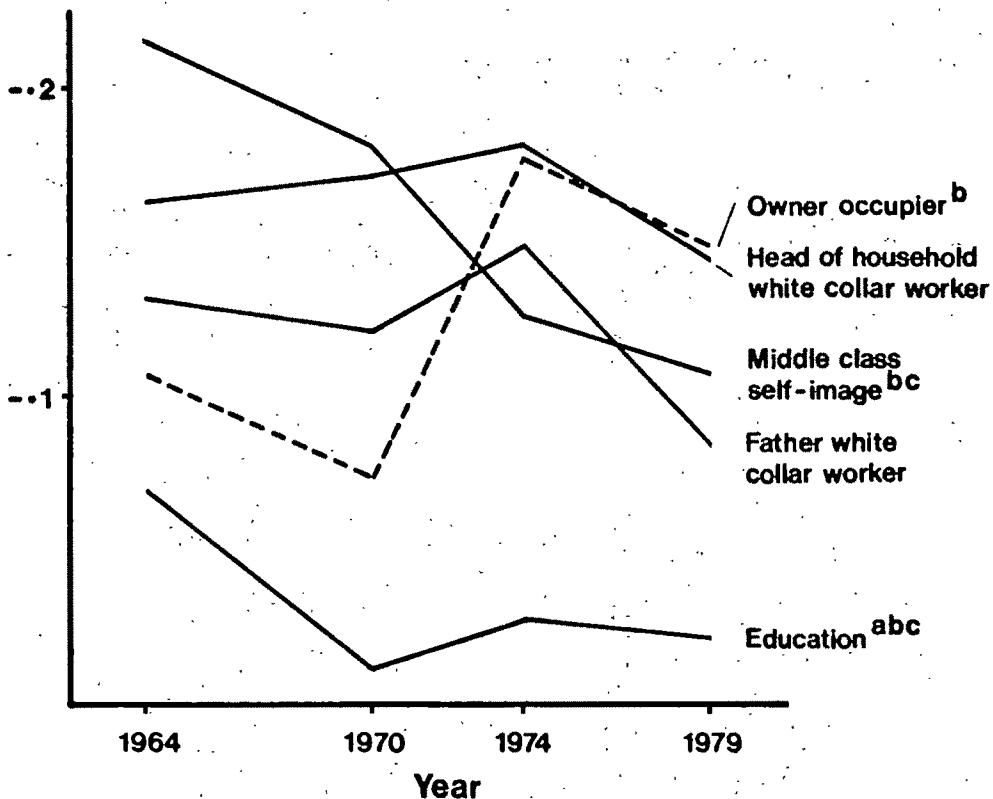
of the council rental market. So Labour cannot reverse its stance on housing without alienating many of its supporters. The Conservatives therefore hold the initiative on the housing issue, while Labour has few options, the reverse of the situation on the other class issues. Further, for good vote-maximizing reasons the Conservatives have stressed their appeal to home owners by advocating lower interest rates in home loans and introducing a policy of allowing council tenants to purchase their homes at advantageous prices. The overall result has been that the Labour Party lost even more home-owner votes than would have been predicted by structural change alone.

The same general picture emerges when we examine the electoral impact of the two changes that might reasonably have been expected to increase the Labour vote; these are the increase in the proportion of trade union members in the electorate and the increase in the proportion

coming from a Labour household. Figure 5 shows that the Labour Party in fact benefited by approximately 1% of the vote in 1979 from the increasing unionization of white-collar workers, who have tended to be more militant than their blue-collar counterparts (Bain, 1970). The increasing number of people born into Labour households can also be seen to have benefited Labour by approximately 2% of the vote (broken line), but there has been a slight decrease in the influence of family political socialization, so that the party's actual gain is less than would have been expected had the link between father's party and voting choice remained at the 1964 level (solid line).

It is obvious that, except in the case of home ownership, actual Labour losses have been substantially less than they might have been, which leads us to reject the second hypothesis to the effect that economic growth leads to a proportionate decline in the vote share of left-wing par-

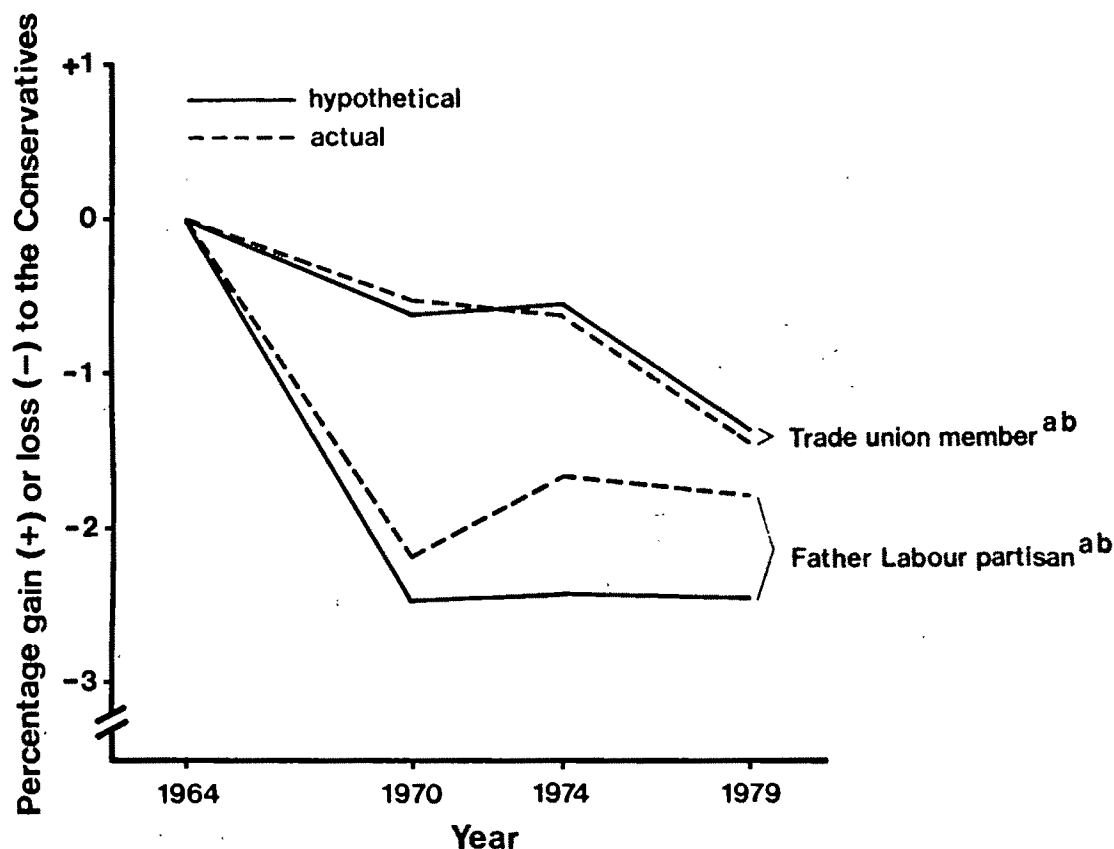
Figure 4. Metric Partial Regression Coefficients Giving the Total Effect on the Vote of Various Socioeconomic Variables for England, 1964-1979



Source: Appendix Table A2, lines 5 to 8.

a,b,c 1964 coefficient significantly different from 1970 (a), 1974 (b), or 1979 (c) at $p < .05$ (two-tailed).

Figure 5. Conservative Gains (+) or Losses (-) Due to Various Structural Changes in England, 1964-1979



Solid lines are hypothesized gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.

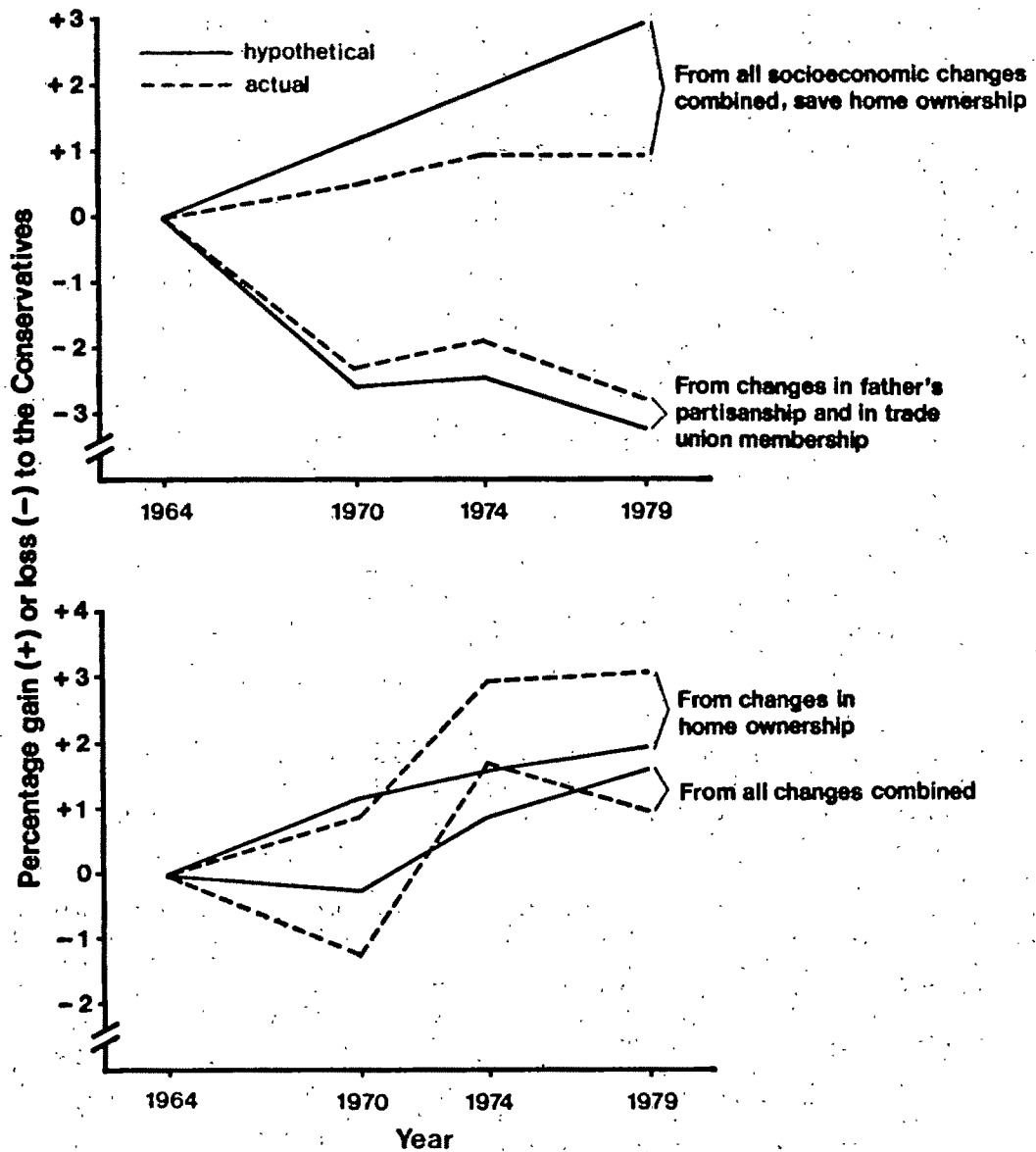
^{a,b}1979 hypothetical value (a) or actual value (b) significantly different from 1964 at $p < .05$, two-tailed.

ties. We can gain a further perspective on the influence of socioeconomic change by showing how it affects party support in the aggregate. Figure 6 presents two separate sets of results: in the upper graph the consequences of socioeconomic change (save home ownership) and of changes in father's partisanship and trade union membership, and in the lower graph the consequences of change in home ownership and of all changes combined. From all socioeconomic changes combined (save home ownership), Labour could have expected to lose approximately 3% of the vote if the relationship between class and party had remained at the 1964 level (solid line). But in the event, this relationship did change, and Labour had a smaller net loss of 1% by 1979. Labour did, however, reap an actual net

gain of approximately 2.5% in 1979 as a result of father's partisanship and increasing trade unionism. From the bottom graph in Figure 6 we can see that the net result of all of these changes combined was an actual gain to the Conservatives of 1%.

Clearly the electoral decline of Labour is not the inevitable concomitant of economic growth, as the decline-of-class theory (and Hypothesis 2) predicts. Moreover, it is possible to go further and argue that as social change continues to erode their traditional social bases, parties of the left will seek new support within the middle class. These results probably help to explain Labour's success in the years between 1964 and 1979 in gaining substantially more middle-class support than previously (Kavanagh, 1982, pp. 107-108).

Figure 6. Conservative Gains (+) or Losses (-) from All Sources of Change Combined in England, 1964-1979
 Solid lines are hypothetical gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.



Indeed, it appears that Labour was able to make most of its inroads into the skilled or supervisory nonmanual grades, rather than the management or professional grades (Rallings, 1975). Our evidence therefore endorses Hypotheses 3 and the party appeals theory, which postulates that economic growth will lead parties of the left to mute their appeal to the working class and strengthen their appeal to the middle class, hence depolarizing the class cleavage.

The final Hypothesis 4 states that the division of the vote between left- and right-wing parties will be unaffected by economic development. This is, in effect, a logical extension of the party appeals theory, insofar as the parties may ultimately find themselves appealing with equal success to middle- and working-class groups. Our findings for England over a decade and a half produce partial, but not complete, support for this hypothesis. Although the Labour Party has been able substantially to overcome the losses from the gradual erosion of its traditional working-class base, it has not totally overcome the problem, and from the results in Figure 6, we see that Labour was still registering a small net loss in 1979 as the result of socioeconomic change within the electorate.

The notion of party adaptability should not be taken too far, however. There are, in fact, three practical factors that limit the ability of parties to transform their appeals along the lines of the Downsian model. First, if an appeal is aimed at the center, then the party risks losing support from its traditional supporters. To obviate this, the party may attempt to disguise the appeal and make it gradual, so as to retain the allegiance of its "natural" supporters. Second, party members and institutional interests may also oppose such a change of appeal because these may well remain committed to the party's basic principles on grounds of faith (or, in the case of groups such as trade unions or business, on grounds of self-interest). We can see this type of conflict in the almost perennial conflict between the more ideologically oriented left-wing Labour Party activists and their more moderate parliamentary leadership (Howell, 1979; Minkin, 1978). Third, voters tend not to choose a party *de novo*, and political socialization and the feelings of affective loyalties that voters have for one or other party impose a pervasive constraint on party successfully altering its appeal.

Discussion

Although there can be little dispute that the importance of class in structuring the vote is declining in many advanced industrial democracies, it has been unclear how far this process has

gone, what its nature is, and what its implications are for the electoral fortunes of the major political parties. In this article we have examined two theories, the decline-of-class theory and the party appeals theory, and postulated two pairs of hypotheses that speak directly to them. To test them we have used survey data from England spanning 15 years. Each theory was only partially supported. The decline-of-class theory rightly predicts that economic development will erode the working-class bases of left-wing parties (Hypothesis 1), but fails to see that parties may change their appeals to offset their consequent electoral disadvantage (Hypothesis 2). Similarly, the party-appeals theory isolates the importance of shifting appeals (Hypothesis 3), but fails to take into account that parties cannot change their appeals too rapidly or completely without risking the alienation of their traditional supporters (Hypothesis 4). In short, both theories accurately predict the *mechanisms* of change, but are less than accurate in recognizing the constraints that limit it.

Our findings show that had the class and party relationship in England remained as strong as it was in the early 1960s, the potential shifts resulting from economic growth were large and consistent enough to have greatly favored the Conservatives.⁵ Indeed, changes in the level of education alone would have increased the Conservative's vote by more than 3% and changes in other class variables (class self-image, white-collar occupation, council housing, father in a white-collar occupation) by smaller amounts. Taken together, these shifts would have transformed British politics, burdening Labour with a built-in disadvantage that would have been sufficient to exclude it from power under all but the most exceptional circumstances. For example, the hypothetical Conservative socioeconomic gain of 3.5% is greater than the percentage gap between the two parties in four of the six general elections between 1964 and 1979.⁶ From a theoretical perspective, scholars have been right to focus attention on the erosion of the left's natural class base (Lipset, 1981). Our span of only 15 years shows a 3.5% loss, but in, say, 30 years, that loss could be 7% and over two generations from the 1920s to the present it could

⁵Britain's much-publicized economic difficulties in the late 1970s and early 1980s are mainly a matter of *slower* long-term growth than in other European countries, not (save for a time during the worst of the recent depression) of stagnation or decline. They have fallen back relative to other countries but not in absolute terms, and it is the absolute, not the relative, changes that are central to the decline-of-class theory.

⁶The exceptions were 1966, when the gap was 6.2%, and 1979, when the gap was 7.0%.

be 14%. The clear implication is that economic growth can fundamentally alter the balance of power between left and right within even the time span of a generation.

As it turned out, however, Labour was able to alter the bases of its appeal so that the 1964 pattern did not continue into the late 1970s. By 1979 it was facing only a 1% actual disadvantage owing to socioeconomic changes other than housing, not the 3% hypothetical disadvantage. Moreover, the changes in father's partisanship and (to a lesser extent) trade unionization advantaged Labour by about 3% in 1979. The simple fact of the matter is that these are factors whose influence the Conservatives could not easily negate; father's partisanship is an aspect of political socialization and not therefore subject to alteration through changing appeals or policies, while Labour's relationship with the unions is an enduring historical connection. But equally, Labour was unable to defuse the housing issue and lost heavily (nearly 3%) on that. The net result is that the Conservatives gained only 1% in 1979 from all the changes combined. We must, therefore, look elsewhere for the explanation of Labour's poor showing in the 1979 election and, by implication, of their disastrous performance in the 1983 election. As has been shown elsewhere, Labour's decline has more to do with territorial factors than with fundamental changes in the electorate's political attitudes (McAllister & Mughan, 1985; McAllister & Rose, 1984).

One final point is worthy of discussion. If, in line with the party-appeals theory, a party is theoretically able to shift its appeal to suit the nature and type of social groups present within the electorate, then it follows that parties will be seeking to appeal to the same clientele once the proportion of middle class and working class voters in the electorate become equal in size. Hence, class polarization will decrease, if not disappear altogether. But we can also take the logic of this further and suggest that if the middle class actually becomes numerically dominant, then it is in the interests of the *party of the right* to implement a direct class appeal. In both Australia and the United States the proportions of white- and blue-collar workers are almost equal, and it would appear that their respective parties of the right have, in recent years, been more prepared to aim their appeals more narrowly at the middle class than hitherto.

It is possible to make two further speculations should such a situation arise. First, there would be a change in the character of issues promoted by the party of the left; they would move away from class-related concerns and toward nonclass issues such as nuclear power, the environment, or consumer affairs. To some extent this change to issue

rather than class appeals appears to have already begun in the United States (Nie, Verba & Petrocik, 1976; Pomper, 1972) and Britain (Budge, 1982; Crewe & Sarlvik, 1980; Robertson, 1984; Sarlvik & Crewe, 1983; Studlar, 1978; Studlar & Welch, 1981). Second, broadening the appeal of the party of the left (necessitated because the working class now constitutes a minority of electors) would provoke factional conflict, with the party activists on the left being deserted by the now moderate leadership. Equally, however, the moderate wing of the party of the right could desert the party as it intensifies its appeal to the middle class. Overall, we would predict that the consequences of economic growth would lead not only to a decline in class polarization, but to a shift in the kind of issues that are important to the left and to factional conflict as the left wings of both parties lose out to their respective center and right.

These predictions are, however, speculative. Our key argument is that the original "decline of class" theories, which predicted that economic growth would undermine parties of the left, were misplaced. Instead, through a variety of strategies these parties have been able to alter the basis of their appeal to the electorate on many, although not all, issues. While decreasing class polarization is the result of this process of adaptation, it does not signal the inevitable demise of working-class parties.

Methodological Appendix

Hypothetical Changes

Our basic model is presented in Figure 1, which gives the causal order among the variables. The results in Figures 3 and 4 are of two sorts, hypothetical and actual, and are obtained in different ways. The hypothetical changes are all based on regression analyses of voting behavior in the 1964 data. To get the effect of changes in the proportion of the electorate whose fathers were white-collar workers, for example, the following equation was estimated:

$$1964Vote = a + b_1 FaWhiteC + b_2 FaParty \quad (1)$$

where a is the regression constant, and b_1 and b_2 are metric partial regression coefficients. Note that this equation gives the total effect of having a white-collar father (including the parts that come about indirectly through the effects that it has on such factors as the respondent's own education, occupation, and union membership); variables reflecting respondent's own education, therefore

do not appear in the equation (see Alwin & Hauser, 1975).

The predicted vote in 1964 can be obtained from equation (1) by multiplying the means for the independent variables by the regression coefficients and adding the products (e.g., Duncan, 1968). For example, in 1964 the proportion of the electorate who came from white-collar families was .20 and b_1 was -.13 (indicating that people from white-collar families were 13% less likely to vote Labour), so that part of the calculation would be:

$$1970Vote = a - (.13 \times .20) + b_2 FaParty \quad (2)$$

But the proportion of the electorate from white-collar families is slowly increasing, rising to .23 by 1970. On the (hypothetical) assumption that the relation between these variables and the vote did not change between 1964 and 1970, the effect of this change on the vote can also be estimated from equation (1) by replacing the old proportion, .20, by the new, .23:

$$1964Vote = a - (.13 \times .23) + b_2 FaParty \quad (3)$$

The difference between the predicted vote in equation (2) and that in equation (3) shows the change in the vote that would have come about because of the increase in voters from white-collar backgrounds. So, subtracting equation (3) from equation (2):

$$\begin{aligned} &\text{Hypothetical change} \\ &= \text{equation (2) from equation (3)} \\ &= b_1 (1970FaWhiteC - 1964FaWhiteC) \\ &= -.13 \times (.23 - .20) \\ &= -.0039. \end{aligned} \quad (4)$$

This result implies a drop in the Labour vote of nearly 0.4% between 1964 and 1970. Figures for 1974 and 1979 are obtained similarly, using the proportion of voters from white-collar backgrounds in those years.

Estimates for other variables are obtained similarly, replacing equation (1) with the appropriate equation giving their total effects. For example, the effect of home ownership is estimated from:

$$\begin{aligned} 1964Vote &= a' + b_1' FaWhiteC \\ &+ b_2' FaParty + b_3' Educ + b_4' WhiteC \\ &+ b_5' HomeOwner. \end{aligned} \quad (5)$$

The results from these calculations are presented in Figures 3 and 4: The actual figures are in Table

A2, where the calculations are line 2 times line 5 (for 1970), line 3 times line 5 (1974), and line 4 times line 5 (1979).

Actual Changes

The estimates just described show what would have happened if there were changes in the sort of people in the electorate (e.g., an increase in the proportion coming from white collar families) but no changes in the *relation* between these variables and the vote since 1964 (e.g., no change in the 13% differential between white- and blue-collar voters). But in fact the relations changed. For example, by 1970 those born into white-collar families were not 13% but only 12% less likely to vote Labour. Thus the actual growth in the white-collar class was not so disadvantageous to Labour as it would have been if the 1964 relation between class and vote had remained unchanged.

The actual vote in 1970 is therefore not to be found from equation (1) but from its 1970 analog, estimated using 1970 (rather than 1964) data:

$$\begin{aligned} 1970Vote &= a'' + b_1'' FaWhiteC \\ &+ b_2'' FaParty. \end{aligned} \quad (6)$$

The actual change in voting results from the increase in voters from white-collar families is analogous to equation (4):

$$\begin{aligned} &\text{Actual change} \\ &= b_1'' (1970FaWhiteC - 1964FaWhiteC) \\ &= -.12 \times (.23 - .20) \\ &= -.0036. \end{aligned} \quad (7)$$

Estimates for other years are computed similarly. Estimates for other variables are computed from the analogs to equation (5) estimated for 1970 and later years. These results are reported in Figures 3 and 4. The actual data are in Table A2; the calculation is line 2 times line 6 (for 1970), line 3 times line 7 (for 1974), and line 4 times line 8 (for 1979).

Changes from All Sources Combined

The actual result of all these changes combined is shown in Figure 5. It is not, however, simply the sum of the effects computed from equation (7) because there is some overlap in them. For example, the increase in the proportion of the electorate born into white-collar families affects voting mainly indirectly: children born to white-collar families are likely to get more education and better jobs, and to own their homes, and only because of those things to vote differently. Our

estimates of the effects of home ownership include some of these changes (since the increase in home ownership is in part a consequence of the increase in white-collar families, for example). In order to avoid this kind of double counting, the estimate for the combined effect of all variables is

taken from the regression equation giving the direct rather than the total effect of these variables (Table A2, lines 9 to 12). The effects for 1970, for example, are from the analog to equation (6), which includes all independent variables simultaneously (i.e., *FaWhiteC*, *FaParty*, *Educ*,

Appendix

Table A1. Correlations between Variables, 1964-1979^a

	Labour voter (1)	Father white collar (2)	Father Labour partisan (3)	White collar worker (4)	Trade union member (5)	Owner-occupied (6)	Class self-image (7)	Education (8)	Standard deviation (9)
1. Labour voter									
1964	1.00	-.196	.433	-.294	.211	-.221	-.373	-.259	.471
1970	1.00	-.173	.421	-.254	.216	-.168	-.289	-.108	.479
1974	1.00	-.211	.355	-.274	.219	-.289	-.282	-.163	.448
1979	1.00	-.178	.367	-.226	.206	-.247	-.259	-.152	.457
2. Father white collar									
1964	-.196	1.00	-.211	.278	-.066	.122	.258	.202	.408
1970	-.173	1.00	-.184	.340	-.062	.106	.245	.273	.405
1974	-.211	1.00	-.213	.299	-.057	.205	.246	.339	.435
1979	-.178	1.00	-.285	.290	-.039	.168	.238	.361	.451
3. Father Labour partisan									
1964	.433	-.211	1.00	-.168	.132	-.127	-.194	-.109	.381
1970	.421	-.184	1.00	-.183	.195	-.144	-.122	-.099	.434
1974	.355	-.213	1.00	-.151	.118	-.134	-.207	-.127	.420
1979	.367	-.285	1.00	-.176	.107	-.177	-.236	-.183	.426
4. White collar worker									
1964	-.294	.278	-.168	1.00	-.098	.240	.388	.388	.475
1970	-.255	.340	-.183	1.00	-.121	.205	.347	.255	.474
1974	-.274	.299	-.151	1.00	-.073	.299	.280	.374	.490
1979	-.226	.290	-.176	1.00	-.051	.287	.367	.371	.492
5. Trade union member									
1964	.211	-.066	.132	-.098	1.00	-.052	-.120	-.068	.414
1970	.216	-.062	.195	-.121	1.00	-.058	-.189	-.036	.438
1974	.219	-.057	.118	-.073	1.00	-.021	-.107	.012	.436
1979	.206	-.039	.107	-.051	1.00	-.039	-.100	.005	.462
6. Owner-occupier									
1964	-.221	.122	-.127	.240	-.052	1.00	.201	.183	.489
1970	-.168	.106	-.144	.205	-.058	1.00	.247	.142	.500
1974	-.289	.205	-.134	.299	-.021	1.00	.223	.229	.496
1979	-.247	.168	-.177	.287	-.039	1.00	.258	.228	.487
7. Class self-image									
1964	-.373	.258	-.194	.388	-.120	.201	1.00	.387	.459
1970	-.289	.245	-.122	.347	-.189	.247	1.00	.231	.482
1974	-.282	.246	-.207	.280	-.107	.223	1.00	.266	.474
1979	-.259	.238	-.236	.367	-.100	.258	1.00	.309	.475
8. Education									
1964	-.259	.202	-.109	.388	-.068	.183	.387	1.00	1.37
1970	-.108	.273	-.099	.255	-.036	.142	.231	1.00	1.73
1974	-.163	.339	-.127	.374	.012	.229	.266	1.00	1.35
1979	-.152	.361	-.183	.371	.005	.228	.309	1.00	1.47

^aMeans are given in Table 1.

Table A2. Means and Regressions, 1964-1979

	Class Variables							Variance explained (R ²)
	Father white collar ^a	Father Labour partisan	Trade union member	Educa-tion ^a	White collar worker ^a	Home owner	Class self-image ^a	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Means								
1. 1964 means	.199	.514	.219	9.81	.367	.396	.319	—
2. Change, 1964-1970	.031	.048	.040	0.22	.023	.129	.014	—
3. Change, 1964-1974	.052	.048	.036	0.36	.038	.170	.024	—
4. Change, 1964-1979	.078	.048	.090	0.55	.057	.219	.036	—
Estimated Total Effects (Metric partial regression coefficients)								
5. 1964 data	-.127*	.506*	.152*	-.169*	-.160*	-.105*	-.212*	—
6. 1970 data	-.118*	.445*	.135*	-.012*	-.168*	-.072*	-.177*	—
7. 1974 data	-.146*	.347*	.174*	-.028*	-.178*	-.174*	-.121*	—
8. 1979 data	-.081*	.370*	.161*	-.022*	-.139*	-.141*	-.103*	—
Estimated Direct Effects (Metric partial regression coefficients)								
9. 1964 data	-.013	.419*	.138*	-.028*	-.087*	-.090*	-.212*	(32%)
10. 1970 data	-.026	.389*	.107*	.003	-.103*	-.044	-.177*	(26%)
11. 1974 data	-.045	.276*	.164*	.001	-.112*	-.161*	-.121*	(26%)
12. 1979 data	-.019	.301*	.153*	.001	-.079*	-.129*	-.103*	(22%)

^aMeans for these variables smoothed by linear interpolation for reasons given in the text.

*Significantly different from zero at $p < .05$, two-tailed.

WhiteC, Homeowner, and Union), estimated using 1970 data. The estimates for later years are from the same equation applied to data for the later year. The actual data are in Table A2. The calculation is line 2 times line 10, summed (for 1970); line 3 times line 11, summed (for 1974); and line 4 times line 12, summed (for 1979).

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Party Goals and Government Performance in Parliamentary Democracies

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From assumptions of parties as rational actors, this study develops four measures of government performance: duration, mode of resignation, subsequent alternation, and electoral success. These measures are used in a test of competing hypotheses concerning minority government performance in parliamentary democracies. Minority governments are conventionally portrayed as poor performers, but tests of this proposition have been seriously limited. An alternative hypothesis depicts minority governments as rational cabinet solutions without significant performance liabilities. These hypotheses are tested against an extensive cross-national data set including 323 postwar governments in 15 parliamentary democracies. The conventional wisdom about minority governments is not supported by the evidence. In some respects, minority governments are clearly superior to majority coalitions. Moreover, minority government formation may enhance systemic responsiveness and accountability. The findings support the explanation of minority governments as rational cabinet solutions.

Parliamentary Government and Political Performance

The performance of democratic regimes and governments is a central concern in political science. Yet systematic efforts to study political performance cross-nationally have been rare until recently (Almond & Powell, 1978; Gurr & McClelland, 1971; Powell, 1982). In this analysis I will investigate the performance of various types of parliamentary governments. In so doing, I have two main objectives. The first is to bridge the traditional gap between the study of political performance and the more sophisticated analysis of government and coalition formation. Secondly, I derive performance measures that permit a test of competing hypotheses concerning government formation. More specifically, I compare the performance of minority governments (or cabinets) to that of single-party and coalition governments of majority size.

A minority government is any government formed by a set of parties that controls fewer than

half the seats in the national legislature (or in bicameral legislatures: the lower house). The conventional wisdom portrays such governments as highly unviable and ineffective cabinet solutions. The mere absence of a governmental majority is commonly taken as an indication of cabinet ineffectiveness (Powell, 1981, p. 869; 1982). Johnson's (1975) summary of the disadvantages of minority governments aptly represents many of the misgivings frequently expressed. In Johnson's (1975, p. 87) view, minority governments tend to be:

1. Weak and exposed to the risk of parliamentary defeat,
2. Opportunistic and in search of ways to escape their minority position, and
3. Lacking in the authority and support necessary to handle serious problems.

Without addressing these specific propositions, this article will test the conventional view of minority cabinet performance. Contrary to the conventional wisdom, I hypothesize that minority governments generally perform as well as their natural competitors, that is, majority coalitions. I have argued elsewhere that minority governments emerge as rational cabinet solutions (Strom, 1984b). According to this explanation minority cabinets form when the benefits of holding office are outweighed by the costs for a majority segment of the party system. The benefits of governing are inversely related to the opportunities for opposition parties to influence parliamentary legislation. The costs of holding office are greater the more competitive and decisive future elections are likely to be. Consequently, minority governments should form when both *oppositional influ-*

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ence and electoral competitiveness (or decisiveness) are high. If minority governments are indeed rational cabinet solutions, then their performance should not be systematically inferior to that of competing cabinet types. On the other hand, if minority governments are in fact feeble performers, then the rationalist explanation is weakened. In the latter case one would have to assume that party leaders attach significantly greater utility to participation in majority governments than to inclusion in minority cabinets. The contention that these leaders might rationally prefer minority governments under certain conditions would then be much less credible. Thus, the analysis that follows will serve as an additional and independent test of the latter theory.

But the analysis of government performance can only serve as such a test if it meets three conditions. First, the performance measures will have to be relevant to the calculations of party leaders in the game of government formation. Invoking rationality in this context implies that government performance affects the expected utilities of party leaders, and the performance measures have to render this assumption plausible. Second, the two competing hypotheses concerning government performance must be tested extensively against a large set of cross-national data. Finally, one must control for whether or not potential performance differentials are due to underlying structural conditions. For example, minority governments might perform poorly because they form in situations where no government could expect to be successful. If so, it would be wrong to take their occurrence as evidence against an explanation based on rationality. In brief, the three conditions above spell out the tasks of the empirical analysis.

Political Performance: Functional Approaches

Valid and theoretically interesting measures of government performance are in short supply. Eckstein (1971) has pointed out some of the problems inherent in the conceptualization and measurement of political performance. It is difficult to find criteria of evaluation that are neither overly broad and vague (e.g., system maintenance) nor excessively specific and arbitrary (e.g., allocations of government spending). In the former case, causal relationships between the relevant variables may be rather complex and indirect. In the latter case, there may be considerable controversy within and between political systems over the values on which we base our judgments.

Most authoritative studies of political performance have been explicitly or implicitly functionalist in orientation. However, few have claimed to have formulated grand theory or

specified requisite functions. Within the functionalist framework, seminal works have focused on such general variables as civil order, participation, legitimacy, and regime stability (Eckstein, 1971; Powell, 1982). The concern has been with performance as systemic outcome, but these outcomes may have as much to do with environmental conditions as with internal conversion processes. Thus, the causal link between the properties of political institutions (e.g., governments) and such outcomes as legitimacy and civil order may be highly tenuous. Attempts to conceptualize government performance have yielded similarly broad and unspecific categories (Blondel, 1968; Finer, 1975; Spiro, 1959).

The opposite extreme in functional analysis of government performance is a focus on sub-systemic output. One might evaluate cabinets on the basis of parliamentary appropriations for public services, on the level of tax extraction, or on the volume of parliamentary legislation (Almond & Powell, 1978). The validity of such performance measures is open to serious challenge (Di Palma, 1977, pp. 7-27). If output measures are selected subjectively, we almost inevitably involve ourselves in value judgments over which political leaders may be fundamentally at odds. Some politicians strive to increase the size of the public sector; for others, the exact opposite is a primary objective. It is hardly the task for empirical political scientists to settle such disputes *a priori*.

Government Performance: A Rational Choice Approach

As an alternative to the functionalist approach, I link the analysis of government performance to the objectives of government leaders. This strategy has its own problems. For one thing, governments may vary widely in their ambitions. A typical caretaker administration may seek only routine administration for a few months until a new election is held. Other governments desire thoroughgoing changes in the political and economic structures of their countries (Dahl, 1966, pp. 341-344). It would obviously be misleading to consider these goals equivalent in an evaluation of cabinet performance. Moreover, cabinets may be more or less explicit in their formulation of goals, and their goals may be conditioned by the political environment. Consequently, comparisons between and even within countries become problematic.

Yet a useful specification of performance dimensions need not elude us. Based on rational choice assumptions, performance measures can be derived deductively from party goals. In so doing, I assume that parliamentary parties (or their

leaders) act as cohesive units, and that these parties pursue a variety of goals in the cabinet formation game. Furthermore, whether in government or opposition, party leaders are assumed to be motivated primarily by partisan rather than systemic considerations. Under these assumptions, government performance can be identified as the performance of the governing parties.

Substantively, I distinguish between policy influence and political office as party goals (see Wittman, 1983). In this context, "political office" refers to participation in government. A major reason that parties wish to be in office lies in their interest in parliamentary influence, but this is hardly the only reason. The spoils of office count for something in their own right, and government participation may also have a certain symbolic value for political parties and their leaders. Conversely, one need not participate in the cabinet in order to enjoy some degree of legislative influence, although officeholding is certainly conducive to political effectiveness (Strom, 1984b).

Party objectives are also complicated by temporal perspectives. Political parties have immediate as well as longer-term goals, and tradeoffs may be necessary along the temporal dimension as well as between office and influence. I distinguish between short-range and medium-range party goals. In practical terms, the distinction between the short and the medium term corresponds to the difference between pre-electoral and postelectoral considerations. The next election is the first occasion when resources (votes) can be substantially reallocated between the parties. To the extent that elections are competitive and decisive for cabinet formation, conflicts may arise between any party's pre-electoral and postelectoral goals. This is because the voters will react to the government participation choices of the various parties. Governing now may reduce the chances of office as well as influence after the next election (Downs, 1957; Rose & Mackie, 1983).

Clearly, most parties have goals that go beyond the next electoral period. However, in the following analysis I disregard the longer term for three reasons. One reason is the great uncertainty attached to predictions of the parliamentary situa-

tion several elections ahead. The second is the limited time horizons of party leaders as individuals. To the extent that personal interest enters into the calculations of party leaders, they are unlikely to be moved by opportunities in the distant future. The political durability of party leaders is short, and their ambitions must typically be realized soon or never. Thirdly, party leaders are constrained by the urgency and irreversibility of many policy decisions, which can be made only now and once. For these reasons, party leaders are likely to have steep discount rates for longer-term benefits.

In sum, parties will value officeholding before as well as after the next election. Apart from that, they will also wish to be influential in the future as well as today. Combining the substantive and temporal dimensions produces a typology of party goals in the cabinet formation game, as presented in Table 1. This typology provides us with a framework for analysis of government performance. I shall proceed to construct performance measures, which have been included in Table 1. Their purpose is to enable us to evaluate the degree to which various types of governments further the goals of the participating parties.

The performance measures have been designed to meet the conditions of relevance to the calculations of party leaders. I have also sought to capture dimensions of cabinet performance that are related to important constitutional functions of governments. This study therefore bears some indirect relation to functional analyses of government performance. The variables I present at least tap Eckstein's criteria of durability and decisional efficacy (Eckstein, 1971). Thus the performance variables indirectly indicate how well various types of parliamentary governments perform their constitutional functions.

The Data Base

Government performance will be tested against a set of data including all recent governments in a number of parliamentary democracies with a significant record of minority governments. For the sake of the plausibility of the assumptions about party objectives, the scope of this study is

Table 1. Typology of Party Goals

	Short range	Medium range
Office	Pre-electoral office (Durability)	Post-electoral office (Alternation)
Influence	Pre-electoral influence (Resignation)	Post-electoral influence (Electoral success)

Table 2. Governments by Country and Format, 1945-1982

	Majority	Minority		Nonpartisan	Total	Mean parliamentary basis
		N	% ^a			%
Belgium	25	4	14	0	29	62.0
Canada	7	8	53	0	15	53.1
Denmark	3	20	87	0	23	40.1
Finland	19	11	30	7	37	54.6
France	18	12	40	0	30	51.0
Iceland	13	4	24	0	17	51.5
Ireland	10	5	33	0	15	50.5
Israel	23	2	8	0	25	62.0
Italy	26	19	42	0	45	51.8
Netherlands	15	3	17	0	18	61.6
Norway	8	10	56	0	18	47.4
Portugal	7	1	8	4	12	64.5
Spain	1	3	75	0	4	50.0
Sweden	9	10	53	0	19	47.2
United Kingdom	14	2	13	0	16	52.7
Total	198	114	35.3	11	323	53.2

^aMinority governments as a percentage of total governments.

Sources. Keesing's Contemporary Archives, Mackie and Rose (1982), Cook and Paxton (1975), *Facts on File*.

restricted to stable, liberal, and developed nations. The sample includes all advanced Western parliamentary democracies with a history of at least one minority cabinet per 20 years since 1945. For each country, all governments formed from May 1945 through 1982 are included.¹ I have counted governments according to the generally accepted practice for each country,² except that any of the following conditions is sufficient to constitute a change of government:

1. Change of prime minister, or
2. Change of parties represented in the cabinet, or
3. General elections, or
4. By-elections resulting in a change in the government's parliamentary basis from majority to minority status, or vice versa (see Lijphart, 1982).

¹Many political systems experienced all-party governments during and immediately after World War II. I have not included such governments in this analysis, even in cases where they were formed in the aftermath of the war. In other words, for each country the record begins with the first partisan parliamentary cabinet formed after the cessation of hostilities in Europe.

²The one significant problem in this respect involves governments, particularly in France and Italy, which fall their vote of investiture. In such cases, the most common practice is to include them, which has also been my choice here.

These parameters yield a total of 323 governments in the following 15 countries: Belgium, Canada, Denmark, Finland, France, Iceland, the Republic of Ireland, Israel, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and the United Kingdom. For all intents and purposes, this sample constitutes the entire universe of post-1945 minority governments.³ Table 2 presents a breakdown of the governments included. Note that the overall frequency of minority governments is as high as 114 out of 323, or 35.3%, which corresponds to close to eight minority cabinets per country. Whether in absolute or relative terms, this record can only be described as very substantial. In Canada, Denmark, Norway, Spain, and Sweden, more than half of all governments have been of minority status.

Among majority administrations, I distinguish between coalitions and single-party (majority party) governments. The latter tend to form

³In Europe, the only minoritarian case excluded is the Austrian Kreisky government of 1970-1971. Obviously, not all 15 nations have had continuous histories of parliamentary democracy since 1945. In such cases, the time span covered is shorter. Thus, the Israeli record begins in 1949, and the French ends with the demise of the Fourth Republic in 1958. The first Portuguese government included is the first Goncalves cabinet of 1975, and the first Spanish government the one formed by Suarez in July 1977.

whenever possible, that is, in every majority situation. In minority situations, when no majority party government is possible, the choice will normally be between a majority coalition and a minority government. The sample contains 42 majority party and 156 majority coalition governments. Finally, there is a small number of non-partisan administrations: seven in Finland and four in Portugal. These are governments without any identifiable partisan support, mostly caretaker business administrations. Owing to their lack of partisanship, these governments cannot be assigned any value for parliamentary basis, that is, the percentage of legislative seats controlled by the governing parties.

External Support

Some minority governments may be majority governments in disguise: governments with an equally secure, but less visible, basis in the assembly (Daalder, 1971, p. 288; De Swaan, 1973). Minority governments often have formal or informal support agreements with parties not represented in the cabinet. If the commitment of these external supporters is as strong as that of the governing parties, there would be no reason to expect the performance of minority governments to differ from majority coalitions. I have attempted to identify externally supported cabinets in this stricter and more interesting sense. Thus, a government is classified as externally supported if:

1. Its support is negotiated before the formation of the government, and
2. That support is an explicit, comprehensive, and more than short-term commitment to the policies as well as the survival of the government.

By these criteria, 35 of the governments in this study are externally supported. However, some of the externally supported cabinets are of majority size to begin with, and in other cases the inclusion of external support is not sufficient to cross the majority threshold. For only 13 governments does external support make the difference between minority and majority status. I call these formal minority governments. The remaining 101 minority cabinets will be referred to as substantive minority governments (Strom, 1984b).

Pre-Electoral Office Performance

When parties decide to participate in government formation, they are presumably interested in some measure of government longevity. The point follows directly from my assumptions. If parties,

ceteris paribus, seek to maximize pre-electoral officeholding, then they in effect wish to hold office for as long as possible before elections. Governing parties will seek to maximize government durability, that is, the longevity of their own rule. To use durability as a measure of government performance is not a novel idea (e.g., Blondel, 1980; Warwick, 1979), but in this analysis, durability taps one specific and theoretically derived dimension of government performance. Durability is measured as the tenure of each government in months.⁴ It is common to associate minority governments with low levels of durability (Dodd, 1976, pp. 170-181; Lijphart, 1982; Warwick, 1979). Likewise, coalition governments have traditionally been perceived as less durable than single-party administrations, although this proposition has been challenged by Dodd (1976). Table 3 presents mean durability figures for the various cabinet types.

At first sight, the findings are fairly consistent with the conventional view of minority governments. Although majority governments enjoy an average duration of approximately 19.3 months, undersized cabinets tend to last only 13.8 months, and nonpartisan administrations no more than 4.1. When the majority and minority categories are subdivided, however, a more interesting pattern emerges. Table 3 shows us that there are two outlying categories. Majority party governments tend to be unusually durable, whereas nonpartisan administrations are very short-lived. The difference between majority coalitions and minority governments remains in favor of the former, but the contrast is far from dramatic. In fact,

⁴Duration of tenure has been measured from the date of formal investiture to the date when the prime minister submits his or her (subsequently accepted) resignation. In cases where the government continues in office after general elections, the date of elections has been used as a termination point. The duration figures have been rounded off to the nearest whole number of months.

Table 3. Cabinet Duration by Cabinet Type

Cabinet Type	Mean Duration (months)
Majority party	28.8
Majority coalition	16.6
Minority formal	13.5
Minority substantive	13.9
Nonpartisan	4.1
All governments	16.7

cabinet composition (coalition vs. single-party) is more strongly related to cabinet durability than is numerical status. Whereas coalition governments survive for an average of 14.9 months, single-party cabinets endure for 21.1 months. These results may be a greater source of concern for Dodd's argument than for mine.³ The durability advantage of majority party governments over majority coalitions is more than a year on average, that of the latter over substantive minority governments only 2.7 months. And for both of the latter two cabinet types, the range of variation is quite wide, with standard deviations of 13.8 and 12.7 months, respectively. Clearly, minority governments are nowhere near as transitory as nonpartisan administrations. Instead, their pre-electoral office performance does not drastically differ from majority coalitions, and there is no meaningful difference between formal and substantive minority governments.

Pre-Electoral Influence Performance

Durable governments do not necessarily enjoy a great deal of legislative success. In fact, the contrary may be the case, in that inoffensive and ineffectual administrations may be tolerated for a longer period of time. Pre-electoral influence for the governing parties is not a simple function of cabinet longevity. More direct measures of policy effectiveness are clearly desirable. But cross-national analysis of legislative effectiveness is extremely problematic. Effectiveness is greatly constrained by a variety of institutional characteristics, for which we cannot hope to control in a satisfactory manner across 15 countries (Di Palma, 1977). Hence, this analysis will use a more indirect measure. Legislative effectiveness requires a degree of cohesion among the governing parties. The unity, and consequently the influence, of any government are likely to be reflected in the circumstances under which it resigns. Thus, the mode of resignation will serve as my measure of pre-electoral influence.

I shall distinguish between the following modes of resignation:

1. *Defeat-Crisis*, when the government is forced to resign, or does so in the face of some perceived adversity.
2. *Voluntary*, in cases where the government is under no substantial pressure to resign, but does so in order to broaden its parlia-

mentary base, reshuffle personnel, or for other reasons.

3. *Technical*, that is, resignations that are neither voluntary nor related to any governmental defeat or crisis. Technical resignations tend to be a function of my rules of counting governments, as in the case when a prime minister dies or a government goes through a general election without any change in premiership, partisan composition, or numerical status (i.e., majority versus minority basis).⁴

Let us first look at the significance of various modes of resignation and then at the available data. The governing parties seek to minimize the severity of their resignations, and I reason that the more severe the resignation, the less their legislative influence. The modes of resignation have been listed in declining order of severity. A crisis-induced resignation more typically reflects a governmental effectiveness problem than does a technical resignation. Voluntary resignations form a more ambiguous intermediate category. On the one hand, it would be far-fetched to interpret the resignation of Danish premier Jens Otto Krag in October 1972, or that of Winston Churchill in April 1955, as evidence of government ineffectiveness. But the voluntary resignations of Italian prime ministers Giovanni Leone and Mariano Rumor, in November 1968 and February 1970 respectively, were just as clearly indications that their administrations lacked any long-term effectiveness or viability. Governments may resign voluntarily out of weakness or out of strength. Despite this ambiguity of voluntary resignations, I feel confident that the category overall represents an intermediate level of severity, between the defeat-crisis and technical modes.

Table 4 presents a breakdown of resignations by cabinet type. The conventional wisdom would lead us to believe that minority governments are inferior in pre-electoral influence. The argument that minority governments are ineffective is as common as the claim that they are short-lived (Dodd, 1976, p. 179; Lepsius, 1978, pp. 43-44; Linz, 1978, p. 66; Von Beyme, 1970, pp. 570-571). The results demonstrate that majority party governments resign under substantially more favorable conditions than any of the other types. Less than half of these resignations were pre-

³However, I have not sought to test Dodd's theory, because there is no distinction in the data between minimum-winning and oversized coalitions.

⁴General elections have been counted as technical resignations only in cases of continuity of all three factors mentioned above: premiership, partisanship, and numerical status. In all other cases, resignations after general elections have been considered caused by defeats or crises.

Table 4. Mode of Resignation by Cabinet Type (%)^a

Cabinet Type	Defeat/Crisis	Voluntary	Technical
Majority party	48	18	33
Majority coalition	85	5	10
Minority formal	64	36	0
Minority substantive	67	16	16
Nonpartisan	82	18	0
All governments	74	12	14

^aThe percentages in each row do not necessarily add up to 100 because of rounding.

capitated by a defeat or crisis, and fully a third were technical in nature. In contrast, no formal minority or nonpartisan administration is on record with a technical resignation, although it should be kept in mind that these cabinet types are few in number.

More important, the profile for substantive minority governments is significantly more flattering than that for majority coalitions. The relative frequency of voluntary and technical resignations combined is more than twice as high for the former cabinet type. Substantive minority governments are thus superior to majority coalitions, although inferior to majority party governments, in performance relevant to their pre-electoral influence goals. A separate analysis of causes of governments resignation (not reported here), using a sixfold classification, yields very similar results. Majority party governments resign due to the least severe causes, and the profile of substantive minority governments is significantly more favorable than that of majority coalitions (Strom, 1984a, pp. 165-168).

Post-Electoral Office Performance

Governing parties seek to maintain their hold on executive office into future administrations and electoral periods. Hence, governments can be evaluated on the basis of how well they maintain themselves in office. If two successive governments are identical in terms of partisan composition, then the postelectoral office goals of the governing parties of the earlier cabinet have been served rather nicely. Consequently, my measure of postelectoral performance is subsequent alternation or turnover in government. The operational measure reflects the percentage of parliamentary seats held by parties changing status between government and opposition in a given change of government.⁷ A hypothetical example

may clarify the computation of alternation. In a given legislature, party A has 50% of the seats, party B 30%, and party C 20%. Party A initially governs alone. If the next government consists of parties B and C, then all parties will have changed status from the initial government, and the alternation score for the first government will be: $50 + 30 + 20 = 100$. If instead parties A and C form a coalition, then only C will have changed status, and the alternation score for the initial government will be 20.

The lower the alternation score, the better the postelectoral office goals of the governing parties are served. Alternation is measured at the next change of government. Thus, this variable reflects postgovernmental (but not necessarily postelectoral) turnover. In practice the difference between postelectoral and postgovernmental alternation is unlikely to be dramatic. In a majority of the 15 countries, postelectoral and postgovernmental alternation coincide in more than three-fifths of the cases. Hence, postgovernmental alternation is also a fair indicator of post-electoral alternation.

Subsequent alternation is greater for minority governments than for those of majority size. The mean alternation scores for these two broad categories are 28.3 versus 19.9. Table 5 demonstrates that subsequent alternation is also significantly higher for substantive minority governments than for majority coalitions. Thus, from the point of view of the participating parties, majority coalitions perform better than minority governments. But some curious aspects of the findings should be mentioned. It is, for example, interesting to note the relatively high score of majority party governments and the very low

leaving government and the percentage of seats held by parties entering government. Where general elections intervene between two governments, all calculations have been made on the basis of the composition of the legislature *after* the election. For technical reasons, the maximum alternation score is 98.

⁷Our indicator for alternation is computed as the sum of the percentage of parliamentary seats held by parties

Table 5. Subsequent Alternation by Cabinet Type^a

Cabinet Type	Full Sample	Nontechnical Resignations Only
Majority party	21.7	32.8
Majority coalition	18.9	21.3
Minority formal	11.2	11.2
Minority substantive	30.3	34.6
All cabinet types	23.0	26.4
<i>N</i>	289	245

^aAlternation figures represent percentages of legislative seats involved in cabinet turnover.

score of formal minority governments. These tendencies are at odds with the results on other performance measures, where majority party governments consistently perform very well and formal minority cabinets rather poorly.

The second important point is that the range of variation is much higher for majority governments, and that the range is similarly greater for substantive minority governments than for majority coalitions. The results suggest that majority party and substantive minority governments tend to form in systems of alternative government, where turnover in office is likely to be an all-or-nothing proposition. In the Westminster model of democracy, elections either result in a complete change of government, or no change at all (Lijphart, 1984, pp. 4-9). In order to test for this possibility, I have also run the analysis excluding those governments whose resignations were technical (see above). This procedure permits a focus on those cases where alternation is a realistic possibility. As Table 5 demonstrates, the alternation score for majority party governments increases dramatically under these conditions, and there is also a noticeable increase for substantive minority governments. The convergence of these two cabinet types is again evident, and the contrast to majority coalitions more substantial. The high alternation scores of these cabinets types should be less satisfactory than those of majority coalitions for the participating parties. Whether a low degree of alternation is similarly desirable for the political system as a whole is an entirely different question.

Post-Electoral Influence Performance

Post-electoral influence is of course in large part a function of whether governing parties gain entry to subsequent governments, but given the distinction between office and influence performance, the latter must be measured without refer-

ence to government participation. So the question is: Given an institutional framework, what makes parties more or less influential, irrespective of cabinet participation? The essential determinant here is electoral fortunes, in that a large party will tend to be more influential than a small one, and a recently successful party more influential than one with a history of recent electoral setbacks. Hence, postelectoral influence will be measured as subsequent electoral success.

Beyond its usefulness as an indicator of influence, electoral success is a fundamental party objective pursued for a variety of reasons. By Sartori's definition, only electoral goals are necessarily common to all political parties (Sartori, 1976, p. 63), and considerations of future electoral fortunes are a critical part of explanations of party strategy in general and minority government formation in particular (Bueno de Mesquita, 1975; Downs, 1957; Strom, 1984b). Electoral success is therefore a crucial aspect of government performance.

Electoral success can be operationalized in various ways. One may count electoral gains and losses in either parliamentary seats or votes. For coalition governments, the analysis could also be focused either on the overall gains or losses of the coalition or on the fate of the individual parties. The measure here is the aggregate changes in popular vote (in percentage terms) for each government in the next general election after its formation.^a The findings corroborate the argument that incumbency tends to be an electoral liability (Rose & Mackie, 1983). Table 6 demon-

^aVery similar results were obtained when parties, rather than governments, were used as units of analysis. Not only do majority coalitions in aggregate suffer greater losses than minority governments, but the proportion of gaining parties within majority coalitions is also much lower (Strom, 1984a, pp. 177-178).

Table 6. Mean Aggregate Electoral Success by Cabinet Type^a

Cabinet Type	Full Sample	Pre-Electoral Governments Only
Majority party	-2.45	-2.29
Majority coalition	-4.53	-4.05
Minority formal	0.13	-0.40
Minority substantive	-1.44	-1.17
All cabinet types	-3.05	-2.61
<i>N</i>	289	107

^aPercentage points of total vote.

strates that the liability of incumbency is shared by most cabinet types. Only the small class of formal minority governments show a miniscule net gain. Yet, electoral incumbency costs are far from equally shared by the three major cabinet types. Majority cabinets lose an average of 4.09% of the total vote, minority administrations no more than 1.28%. The average majority coalition loses almost twice as much as majority party governments, and more than three times as much as substantive minority cabinets.

Relative to their former electoral basis, majority coalitions suffer an average loss of 7.2% of their support. The corresponding figures for majority party and substantive minority governments are 4.5% and 2.1%, respectively. Because majority coalitions tend to have broader initial support, their electoral losses are slightly less dramatic in relative than in absolute terms. Yet an average swing of more than 4% of the total vote away from the governing parties must be considered substantial, and the anticipation of such a hemorrhage may well be an effective deterrent to majority coalition formation.

Electoral success or failure can most meaningfully be attributed to governments in office during general elections. The second column of Table 6 presents a breakdown of electoral success confined to governments in office at election time. As we can see, this restriction has little impact on the pattern described above. All cabinet types, with the exception of formal minority governments, fare a little better when they are in office during elections. But none of these improvements is dramatic, and they are fairly parallel for the various cabinet types.

In sum, the figures in Table 6 yield a consistent picture of the electoral success of various types of governments. Majority coalitions perform considerably worse in subsequent elections than majority party governments. The latter are in turn inferior to minority governments. Thus, on a key performance variable, we find minority governments to have a clear advantage over their competitors.

Multivariate Analysis of Government Performance

We have explored the performance of various cabinet types on a number of dimensions. However, the analysis has remained bivariate, and no attempt has been made to trace the observed differences back to dissimilarities in the conditions under which various cabinets form. In the following analysis, I introduce a variety of control variables to investigate whether or not these can account for the performance differences between

majority coalitions and substantive minority governments.

Not all performance measures lend themselves to this type of study. The analysis will be confined to interval-level variables, because only these meet the assumptions of standard regression analysis. Thus, this part of the study will include duration, alternation, and electoral success. The analysis will be restricted to minority situations with partisan outcomes, which is to say that majority party and nonpartisan governments will be excluded. Majority party governments are excluded because they can form only in majority situations, in which minority solutions are not feasible and would not be predicted by any theory. Nonpartisan administrations are left out because no value can be assigned to them on the crucial variable of parliamentary basis. I shall use two distinct indicators of parliamentary basis: one continuous and one dichotomous. The continuous measure (parliamentary basis) is as earlier the percentage of parliamentary seats held by the governing parties. External support will be considered part of the parliamentary basis. Secondly, a dummy variable (numerical status) will distinguish between majority coalitions and substantive minority governments. This variable is coded 0 for substantive minority governments and 1 for majority coalitions and formal minority governments. The latter procedure allows us to get a direct measure of the impact of the difference between majority and minority governments.

The multivariate analysis will build on previous efforts to explain the formation of minority versus majority governments. Minority government formation is related to a variety of structural and proximate conditions, and I have argued that such governments can be explained as rational cabinet solutions under specified conditions (Strom, 1984b). The following analysis includes the variables that have been found to affect minority government formation. The main results of that analysis are presented in Figures 1 through 3. The general hypothesis here is that variables that affect the formation of minority government might also have an impact on their performance. These variables include party system fractionalization (Rae, 1971, pp. 53-58), polarization or extremist party support (Powell, 1982, pp. 92-96, 233-234), electoral volatility for parliamentary seats (Pedersen, 1979), the policy influence of the parliamentary opposition (Strom, 1984a, pp. 121-124),⁹ the salience of general elections for govern-

⁹This variable is a five-point scale based on characteristics of the legislative committee systems. The more specialized and autonomous the legislative committees

ment formation, the *responsiveness* of government formation to electoral verdicts,¹⁰ the *spatial concentration of the parliamentary opposition* (Dahl, 1966, pp. 332-336) on either the left or right side of the government,¹¹ the existence or nonexistence of legislative *investiture requirements*, the *duration of the crisis* preceding each government formation (in days), and the *number of formation attempts* during these crises.

Most of these variables are conventional measures or have previously been introduced in this analysis. Others are discussed in the footnotes. However, the complex and powerful measure of electoral salience deserves a few words of explanation. Electoral salience is a composite indicator based on 1) the degree of association between general elections and government formations (proximity), and 2) the identifiability of pre-electoral governmental alternatives. Each country is scored on a decade-to-decade basis, and each government assigned the mean value for the country and decade in which it was formed. A high degree of association between the two components has been established through factor analysis

(Strom, 1984a, pp. 124-127).¹² Electoral salience, the influence of the opposition, volatility, and responsiveness are the main explanatory variables in the rational choice theory of minority government formation. Each is hypothesized to covary negatively with parliamentary basis.

I have used an ordinary least squares regression technique with a backward elimination procedure. Through previous analysis (Strom, 1984b), I can also determine indirect effects on government performance through parliamentary basis. Such indirect effects will be included in causal path models for each of the three interval-level performance measures. For technical reasons, this path analysis can only be based on the continuous measure of parliamentary basis. However, the path models will be supplemented with a presentation of the regression coefficients for models using the simple dichotomy between majority and minority governments (numerical status) as the crucial predictor. These two procedures should complement each other well. The former offers a comprehensive view of the factors that directly or indirectly affect government performance; the latter yields the most precise estimates of the performance differential between majority coalitions and minority governments.

of a given nation, the higher its score on oppositional influence.

¹⁰Responsiveness has been measured for each country and decade as the proportion of parties entering government that had increased their share of parliamentary seats in the most recent general elections.

¹¹Concentration is measured as the number of seats held by parties on the numerically largest opposition side as a proportion of all opposition seats.

Cabinet Duration

Table 7 presents the regression analysis for cabinet duration, including the full equation, with

¹²Proximity is operationalized as the proportion of all government formation that occur immediately follow-

Table 7. Multivariate Analysis of Cabinet Duration

Variable	Full Equation			Final Equation		
	Coefficient	SE	Beta	Coefficient	SE	Beta
Numerical status	6.89**	1.71	.25	7.72**	1.56	.28
Influence of opposition	-.48	.88	-.03		NS	
Electoral salience	8.13**	1.41	.53	9.34**	.87	.60
Volatility	1.26	12.40	-.01		NS	
Responsiveness	-.47	3.89	-.01		NS	
Polarization	-.07	.10	-.06		NS	
Fractionalization	3.49	15.59	.02		NS	
Crisis duration	.05*	.02	.13	.04*	.02	.12
Formation attempts	-.62	.53	-.07		NS	
Opposition concentration	2.74	5.84	.03		NS	
Investiture	-.02	1.67	-.01		NS	

Adjusted R²

.305

.319

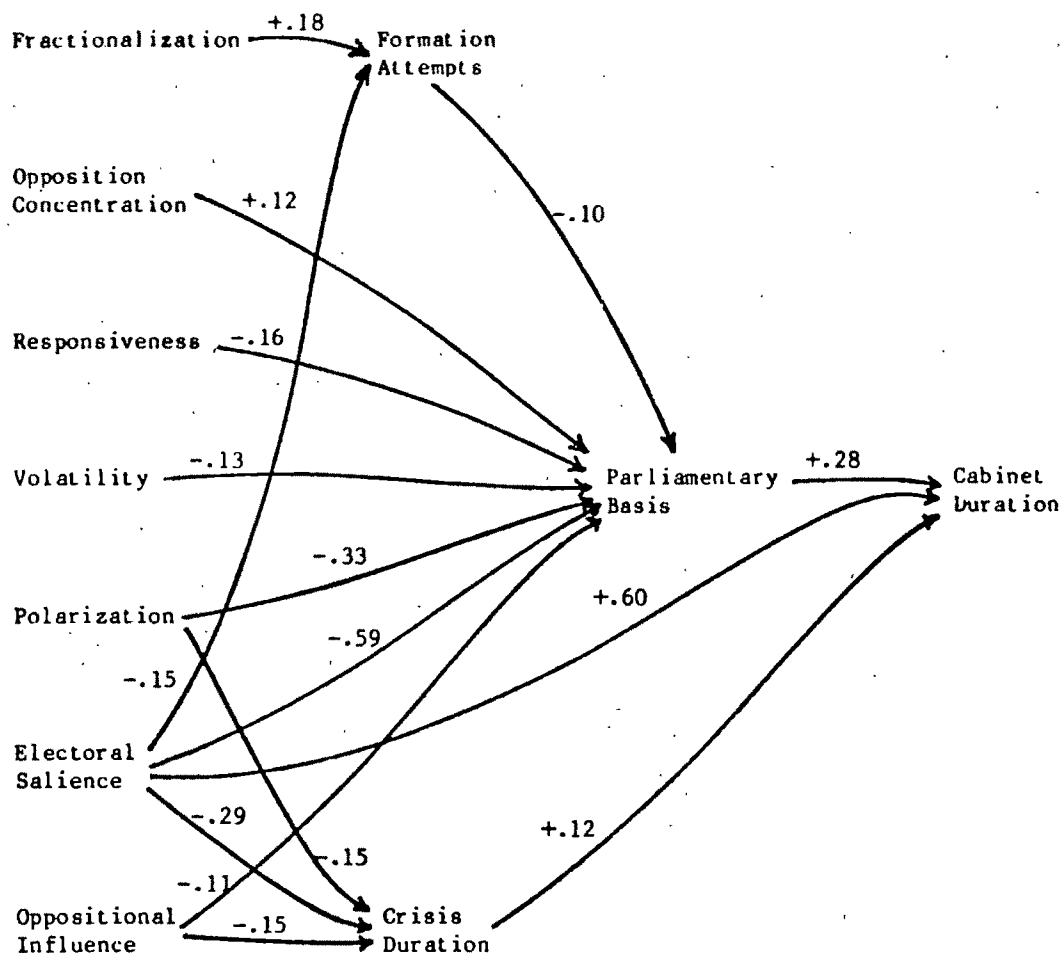
N = 251

NS = Not significant at .05 level

*Significant at .05 level

**Significant at .01 level.

Figure 1. Causal Path Model of Cabinet Duration



Note: Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

all independent variables included irrespective of significance, as well as the final equation including only significant relationships. Obviously, few of the control variables have any meaningful impact on government durability. In fact, only three relationships meet the .05 level of significance. One of these is numerical status, that is, the distinction between majority coalitions and substantive minority governments. Depending on how many other variables are included in the

analysis, majority size increases the longevity of parliamentary governments by approximately seven months. This is a substantial difference consistent with the bivariate analysis above.

However, in the path model the impact of electoral salience is considerably stronger than that of parliamentary basis. The beta weight of the former variable is .60, as against .28 for the latter. The more salient elections are for government formation, the longer governments endure. This variable alone accounts for a predicted difference between its extreme values of more than two years in cabinet duration.¹³ It is no surprise that elec-

ing general elections, measured for each country and decade. Identifiability has been scored impressionistically as low (0), medium (0.5), or high (1) for each election. The mean has then been computed for each nation and decade.

¹³The observed range for electoral salience is approximately 3.1 points.

toral salience is related to cabinet durability. After all, the former variable measures the extent to which changes of government are linked to the electoral cycle, and the frequency of general elections is upwardly bounded since there is normally considerable resistance to intervals shorter than two to three years between elections. Yet the magnitude of the observed relationship is noteworthy. The path model in Figure 1 illustrates that, apart from this strong direct impact, electoral salience has several indirect effects on government durability: through crisis duration, formation attempts and parliamentary basis. The indirect effect through formation attempts is weak and positive; the others are negative. But the strongest relationship is clearly the direct and positive one.

The third significant relationship in Table 7 links crisis duration and cabinet duration. The longer the preceding crisis, the longer the government tends to last. But the tradeoff is hardly very favorable, since an extra day of cabinet crisis buys little more than a day of cabinet durability. In addition to this rather modest effect, crisis duration has an indirect effect on cabinet durability, because there is a weak tendency for majority coalitions to result from more protracted crises than minority governments. The relationship between crisis duration and government duration may involve an inhibition on early dismissals of governments formed after lengthy negotiations. Such governments may tend to survive not only because the various parties have a vivid image of the costs of another round of difficult negotiations, but also because the previous round presumably cleared away the most immediate obstacles to government unity.

Subsequent Alternation

Table 8 presents the equation with subsequent alternation as the dependent variable. Again, there are few significant relationships, and the bivariate relationship between parliamentary basis and government performance tends to hold up when the various control variables are introduced. However, in this case only the final equation produces a significant difference between majority coalitions and substantive minority governments. There is close to 9% less turnover associated with majority coalitions, a discrepancy that is somewhat smaller than in the bivariate analysis. Polarization and the concentration of the opposition are actually stronger predictors of alternation than is parliamentary basis, but none of these relationships is particularly impressive. Polarization is the strongest predictor of alternation, as high levels of polarization reduce the degree of turnover in government. This is a reasonable finding; nor should we be surprised to find that alter-

nation increases with the concentration of the opposition. The further apart the various opposition parties are ideologically, and the more the government can split them into separate blocks, the lesser the opportunities for turnover in office. Majority size is an additional obstacle to alternation. In the full equation in Table 8, the effects of numerical status, polarization, and opposition concentration are all depressed by the inclusion of electoral salience. Figure 2 confirms these relationships. Note also that although electoral salience does not directly impinge on alternation, there is a positive indirect effect through parliamentary basis. On the other hand, electoral salience indirectly inhibits alternation through its impact on formation attempts, but this link is very tenuous.

From the point of view of the governing parties, nothing could be better than a situation that inhibits alternation in office. At least it would appear so in the short run; over time a situation without exit may become burdensome for the parties in government. High levels of polarization, fragmentation of the opposition, and large coalitions are a recipe for stability in government, but in systemic terms these factors represent an immobilist syndrome. Specifically, these conditions correspond to critical properties of the type of party system Sartori calls polarized pluralism (Sani & Sartori, 1983; Sartori, 1976, pp. 131-145). Hence, to the extent that government minoritarianism and electoral salience produce alternational party systems, these factors may in fact promote systemic stability and responsiveness.

Electoral Success

The final regression model has electoral success as its dependent variable. Table 9 presents the results. Again, there are few strong relationships to report. Numerical status emerges as the strongest predictor of electoral success, and the introduction of various controls does little to reduce the advantage enjoyed by minority cabinets over majority coalitions. This advantage amounts to approximately 2.5% of the popular vote. Two other variables exhibit significant relationships. Electoral salience is the strongest predictor in the full equation, and it is worth keeping in mind that this variable has a considerable indirect impact through its conduciveness to minority government formation. Both directly and indirectly electoral salience improves government performance at the polls. However, this impact means reduced government losses rather than increased gains, since there is a clear tendency for all types of cabinets to suffer rather than gain at the polls.

Table 8. Multivariate Analysis of Subsequent Alternation

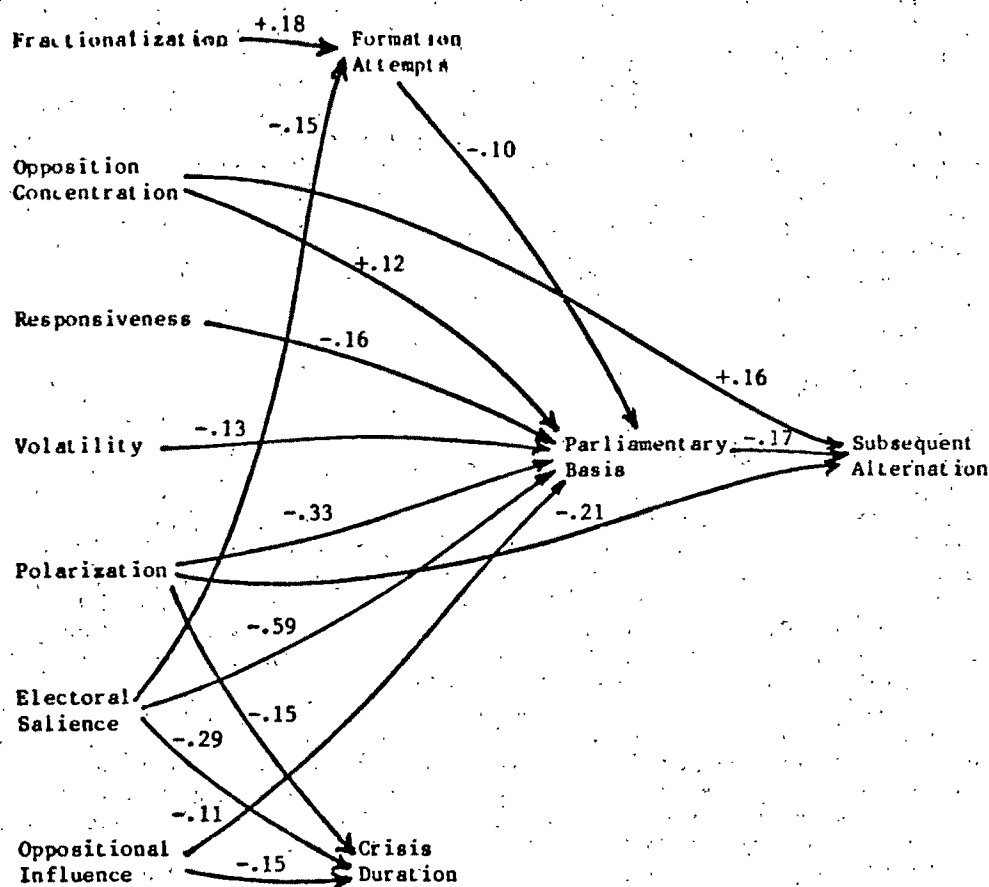
Variable	Full Equation			Final Equation		
	Coefficient	SE	Beta	Coefficient	SE	Beta
Numerical status	-5.22	4.45	-.09	-8.82**	3.76	-.14
Influence of opposition	-.65	2.25	-.02		NS	
Electoral salience	4.48	3.67	.13		NS	
Volatility	29.64	32.28	.06		NS	
Responsiveness	8.00	9.99	.06		NS	
Polarization	-.26	.25	-.10	-.46**	.17	-.18
Fractionalization	12.16	40.52	.03		NS	
Crisis duration	.02	.05	.02		NS	
Formation attempts	1.04	1.41	.05		NS	
Opposition concentration	30.31*	15.25	.14	35.34**	14.01	.17
Investiture	-3.73	4.42	-.06		NS	
Adjusted R ²		.097			.105	
N = 241						

NS = Not significant at .05 level

*Significant at .05 level

**Significant at .01 level

Figure 2. Causal Path Model of Subsequent Alternation



Note. Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

Table 9. Multivariate Analysis of Electoral Success

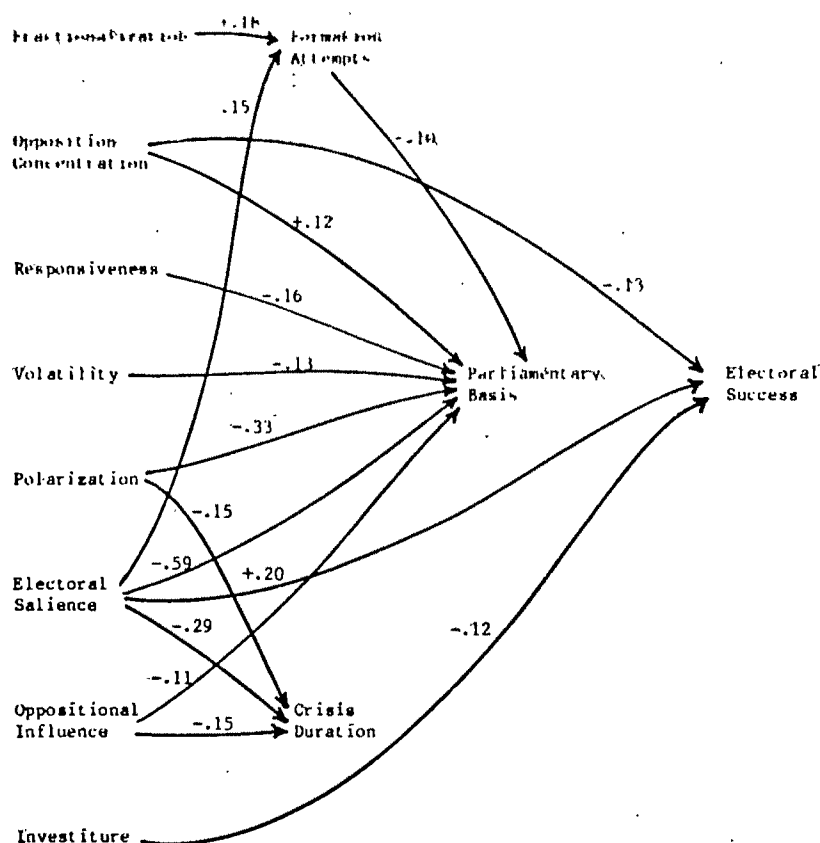
Variable	Full Equation			Final Equation		
	Coefficient	SE	Beta	Coefficient	SE	Beta
Numerical status	-2.32*	1.01	-.17	-2.92**	.86	-.22
Influence of opposition	.17	.52	.02	NS		
Electoral salience	1.87*	.85	.25	NS		
Volatility	-10.75	7.18	-.10	-12.53*	6.70	-.12
Responsiveness	-1.69	2.41	-.05	NS		
Polarization	.06	.06	.11	NS		
Fractionalization	3.81	9.08	.04	NS		
Crisis duration	.01	.01	.06	NS		
Formation attempts	.01	.31	.00	NS		
Opposition concentration	-4.78	3.43	-.12	NS		
Investiture	-1.11	.97	-.10	NS		
Adjusted R ²		.049			.045	
N = 241						

NS = Not significant at .05 level

*Significant at .05 level

**Significant at .01 level

Figure 3. Causal Path Model of Electoral Success



Note. Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

Electoral volatility, on the other hand, depresses the electoral fortunes of incumbent governments. This is another reasonable relationship. Because electoral shifts generally work against the incumbents, volatility should be no boon. The indirect effect of volatility through parliamentary basis, however, is to enhance government success at the polls. The effects of electoral salience and volatility do not both reach significance at the .05 level in the same equation. Electoral salience is significant only in the full equation, volatility in the final equation. However, both qualify at the .1 level in the same model. As in the case of alternation, it is largely the elimination of electoral salience that boosts the effects of numerical status and volatility in the final equation.

The causal pattern changes considerably when the continuous measure of parliamentary basis is used. Figure 3 shows that opposition concentration, the existence of investiture requirements, and electoral salience affect electoral success in this case. Government success at the polls is thus enhanced by a dispersed opposition and by a political system geared toward electoral contests. The modest impact of investiture requirements has no obvious explanation. The advantages of parliamentary smallness barely fail to reach significance with the continuous variable in the equation. Electoral fortunes apparently do not diminish in a linear relationship to the size of the government. Either numerically very large governments do not perform particularly poorly, or very undersized governments do not do especially well, or both. At least the former possibility is plausible enough. Because grand coalitions tend to form during periods of national emergency (e.g., war), they may be well insulated against electoral setbacks. The electoral superiority of minority governments, then, is probably strongest at the middling range of the numerical spectrum.

Conclusions

In this article I have analyzed the performance of various types of parliamentary governments. Government performance is a notoriously slippery concept, and good measures are not easily found. I have chosen to relate government performance to the objectives of political parties. This rational choice approach to government performance has several advantages over the functional analysis that has characterized much of the previous literature. From a relatively simple set of assumptions about party behavior, it is possible to derive performance criteria that are neither overly broad and vague nor excessively specific and subjective. The rationalist framework also permits theoretical and empirical linkages between the

study of government performance and the analysis of coalition formation.

The choice of a rationalist mode of analysis does not prevent us from addressing problems at the systemic level. Some of the measures developed here are in fact very closely related to performance dimensions in traditional functional analysis. But a systematic effort to understand the systemic implications of the observed patterns of government performance requires further assumptions and analysis. At the most general level, I would contend that solutions that are rational for self-interested political parties normally also work well for the larger political system. This is at least partly a result of conscious political engineering to produce favorable structure-induced equilibria in the party system (see Shepsle, 1979; Shepsle & Weingast, 1981).

But partisan and systemic interests will not coincide to the same extent on all performance dimensions. Intuitively the correspondence seems higher for short-term than for longer-term performance. Government instability and ineffectiveness between elections are public bads. But long-term stability, and particularly lack of alternation in office, will eventually become immobilism. Strong governmental influence performance is also more likely to be a collective good than strong office performance. The struggle for office is clearly a zero-sum game, the quest for policy influence at least arguably positive-sum.

In the empirical section of this analysis, I have used four performance measures developed within the rationalist framework to test a theory of government formation and performance. The analysis demonstrates clear performance differences between various cabinet types. Majority party governments fare considerably better than the other types in pre-electoral performance, and about average in postelectoral performance. In the more critical (for present purposes) comparison between substantive minority cabinets and majority coalitions, the picture is more mixed. Majority coalitions are superior in office performance, whereas minority governments have the advantage in influence performance. More specifically, minority governments are somewhat less durable and associated with a higher degree of subsequent alternation. On the other hand, undersized governments resign under more favorable circumstances, and they perform substantially better in subsequent elections. An overall assessment of the success of minority governments must involve a comparative weighting of these advantages and disadvantages. There is no definitive way to make this evaluation, yet it will be my contention that minority governments have acquitted themselves well for three reasons in particular.

First, there is a substantial difference between minority governments and majority coalitions in the area of electoral success. I have on theoretical grounds suggested that this is a particularly significant dimension of government performance for party leaders, and the superiority of minority governments therefore looms large in the overall picture.

Second, it is interesting to note that the comparative advantages of minority governments are found in influence, rather than office, performance. If strong influence performance is more likely to have positive systemic consequences than strong office performance, then we should look favorably upon governments that excel in the former area.

Third, minority governments are associated with higher rates of governmental turnover than majority coalitions. In fact, the strong similarity in alternation rates between substantive minority and majority party governments suggests that substantive minority governments serve as vehicles of alternation in power under conditions that do not allow for the dynamics of a perfect two-party system. Canada may serve as the best illustration of this practice. Frequent recourse to substantive minority governments has allowed the Canadians to maintain a system of alternative single-party government despite significant deviations for the two-party model. Although incumbent parties may not find high turnover rates to their liking in the short run, there is good reason to consider this a desirable systemic property. The problems of nonalternance politics have been widely lamented and well documented, as the case of Italy demonstrates (Di Palma, 1977, Sartori, 1976).

The first of these three points suggests that minority governments are rational solutions; the latter two indicate that they are also functional. The surprisingly strong performance of minority governments is probably in the final event related to the rationality of government formation. The less incentive there is to replace an undersized government, the more effective we should expect that government to be. However, because I have used performance data to demonstrate rationality, this explanation of minority government performance cannot be tested in the present analysis. I should emphasize one additional finding that has implications for our understanding of party system dynamics. Minority governments are less likely than majority coalitions to suffer electoral setback, but at the same time more vulnerable to turnover in office. This paradox may be an indication of systemic differences associated with the formation of minority government versus majority coalitions. Political systems conducive to minority government formation appear to be par-

ticularly sensitive to electoral shifts, and minority governments thereby more accountable to the electorate than majority coalitions.

Three main conclusions emerge from the empirical analysis. First, the conventional wisdom about minority government performance is generally incorrect. Secondly, the results support the theory that minority governments represent rational cabinet solutions. Finally, the performance of minority governments appears even more favorable from a systemic perspective than from the point of view of the actors in the cabinet formation game. Minority governments are sufficiently effective solutions to be chosen freely and frequently by party leaders. In the process, the responsiveness and accountability of the democratic system may actually be enhanced.

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An Adaptive Model of Bureaucratic Politics

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In this article we outline a new framework for the formal analysis of bureaucratic politics. It departs from standard neoclassical approaches, notably those of Niskanen (1971) and Peltzman (1976), in several important respects. First our approach explicitly models a system of three-way interaction among bureaus, politicians, and interest groups. Second, it allows for institutional features of each type of participant. Third, it is a model of dynamic process. Fourth, participants make choices adaptively rather than optimizing. Fifth, participants are only minimally informed.

The result is a dynamic model of adaptive behavior, very much in the spirit of Simon's (1947) behavioral tradition, that offers a new perspective on political control, bureaucratic power, and the "intelligence of democracy."

For decades, students of public administration have stressed that we can only understand bureaucratic behavior by looking beyond the boundaries of administrative organization (Long, 1949; Simon, Smithburg, & Thompson, 1950). Agencies operate in continual exchange with an institutionally structured environment of politicians and interest groups, and it is insight into this system of interactions that holds the key to explanation. Popular theories of bureaucratic politics, ranging in emphasis from iron triangles (Cater, 1964; Freeman, 1955; Truman, 1951) to regulatory capture (Bernstein, 1955; Huntington, 1952; Stigler, 1971) to interest group liberalism (Lowi, 1969), are clearly in agreement on this most general of points, and empirical evidence suggests that it is surely a valid one. This consensus sets bounds on what is considered controversial within the field. We argue now about how much discretion bureaus have, the relative influence of politicians and interest groups, and how these properties vary across different agencies. We do not argue about whether politicians and interest groups are central to an understanding of bureaucratic politics.

Formal models have made some useful contributions to the area, but they have uniformly shied away from modelling the kind of three-way inter-

action routinely described in the empirical literature. Perhaps the two most highly regarded efforts to model bureaucratic politics are Peltzman's (1976) theory of regulatory behavior and Niskanen's (1971) theory of budgets and bureaucratic supply. Consider, very briefly, their basic features.

Peltzman builds upon the earlier work of Stigler (1971) and Posner (1974) in an attempt to place regulatory capture within a more general theoretical context. He formalizes the problem by positing a regulator who makes policy decisions about transferring wealth between members of two interest groups (in effect, business and consumers). The regulator is a bureaucrat-politician who, based on perfect information about the groups, chooses among administrative options in an effort to maximize votes. Interest group members then decide to vote for or against him based on his wealth-transfer decisions and on their abilities to organize for political action. Analysis of these supply and demand factors then leads to an equilibrium solution that provides insight into the conditions under which the regulator may be captured by one of the groups.

Niskanen's work is a pioneering effort to explain budgetary outcomes—and to show why government is "too big"—with reference to special properties of the relationship between bureaucrats and legislators. He argues that bureaucrats value larger budgets (and perhaps "slack"—see Niskanen, 1975), and that they have distinct advantages over legislators in the bargaining game that determines budgetary outcomes: they have a near monopoly over information about the true costs of production, they are perfectly informed about legislative demand for their services, they are able to control the legislature's agenda by submitting

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take-it-or-leave-it budgetary proposals, and they are often aided by high-demand legislative committees. By putting these weapons to use, bureaucrats are able to win the bargaining game, securing larger budgets and (if they like) operating inefficiently.

Peltzman and Niskanen thus contribute interesting new ways of thinking about two of the most important issues of bureaucratic politics: which constituencies win and which lose, and how the level and efficiency of bureaucratic provision compare to the social optimum. Peltzman addresses the first, offering an innovative perspective on the relationship between clientele characteristics and bureaucratic resource allocation. Niskanen addresses the second, shedding new light on the relation between bureaucratic power and governmental outcomes. Yet their analyses are also based on five distinctive simplifications that, in imposing analytical structure on bureaucratic politics, condition the questions they raise and the conclusions they derive. In considering whether these assumptions seem reasonable, we are immediately struck by the most obvious of the five. 1) Both theories clearly ignore fundamental dimensions of political interaction. Peltzman combines politicians and bureaucrats into one actor, implicitly assuming that the dynamics of political control, such as oversight, budgeting, divergence of bureaucratic and political goals, and competition for control among politicians, make no significant difference for bureaucratic behavior. Niskanen gives no systematic attention to clientele groups, assuming their demands are represented by the legislature; and he models the legislature as a passive participant that submits to bureaucratic dominance without putting its own substantial resources to strategic use (see Miller & Moe, 1983). In neither is serious attention given to the distinct, interdependent roles of bureaucrats, politicians, and interest groups.

They also share several other properties which, though less objectionable, are major simplifications that structure their analyses. 2) Both make no real attempt to disaggregate institutional decision makers. Bureaus, legislatures, and interest groups are essentially treated as individual decision units. 3) Both are exercises in comparative statics. They are not dynamic models of interaction, and they tell us nothing about the political process that gives rise to their equilibrium results. 4) Both assume that all decision makers optimize, and thus that they choose on the basis of highly complex calculations beyond the capacity of most people. 5) Both include assumptions of perfect information (although Niskanen does so asymmetrically—the bureau is perfectly informed, the legislature is not).

All of these features are in fact quite common

in neoclassical economic models, and they are surely understandable responses to the great complexity of bureaucratic politics. It is one thing for political scientists to point to multiparty interaction, dynamic processes, institutional context, uncertainty, limitations on cognition, and the like, and quite another to construct models that somehow incorporate these properties. In all modelling efforts, simplification is absolutely essential. The problem is always to figure out which aspects can safely be assumed away and which cannot—recognizing that there are different ways to simplify, and that even the most elegant, deductively powerful models run the danger of being very misleading if founded on simplifications of the wrong kinds.

In this article we outline a model—really, a general framework accommodating a whole family of models—that allows us, via computer simulation techniques, to pursue the same basic issues of constituency influence and governmental size addressed by Peltzman and Niskanen, yet also reflects a very different and more general approach to the formal analysis of bureaucratic politics. It departs from their models along each of the five simplifying dimensions noted above. 1) Most important, it is a model in which the central outcomes of interest—governmental outputs, budgets, and bureaucratic efficiency—are in every sense jointly determined by the interdependent decisions of bureaucratic, legislative, and interest group participants. Attention focuses on the entire system of political relationships and on the integral roles these participants play as part of that larger system. 2) It allows us, if we like, to incorporate institutional features of each type of participant, for example, by recognizing decision processes internal to bureaus or legislatures. In this article we take one of many possible steps in this direction by introducing a majority-rule legislature. 3) The model is dynamic, allowing us to explore the process of interaction and adjustment as it unfolds over time. 4) Decision makers are assumed to adapt in simple ways to their environments, moving in directions that appear to promise them greater utility. They do not optimize, nor do they carry out complex calculations. 5) Participants are only minimally informed about their environments and about each other, basing their adaptive decisions on feedback about the success of their prior decisions.

These elements have a certain natural compatibility. A model of three-way interaction is intrinsically complex, and the assumption of optimization would only magnify the problem by forcing inquiry into an array of complicated game-theoretic considerations. Because formally incorporating a third actor jeopardizes the mathematical tractability of bureaucratic models in the

neoclassical tradition, it is no accident that they address only dyadic interaction.¹ Assumptions of adaptive behavior, on the other hand, are not only far more reasonable empirically, but they are also particularly well suited to the task of modeling dynamic, interactive decision making under uncertainty. From a purely technical standpoint, it is a relatively easy matter to model individual decision makers in terms of utility functions and adaptive rules, impose a structure (reflecting, e.g., institutional context) on their interactions, and then allow a computer to map out the corresponding implications for individual choices and collective outcomes as they occur over time. Large numbers of interacting decision makers and varying institutional contexts can be handled without any real difficulty, given the computer's admirable skill at calculation. This technical feasibility does not, of course, imply that any given model will provide an adequate or insightful explanation of bureaucratic politics. But it is an important advantage which, in combination with the greater realism of its assumptions, sets this approach apart from the conventional neoclassical approach and makes it particularly worthy of investigation.

Thus, our interest in constructing a model that incorporates a basic fact of political life—interaction among bureaucrats, politicians, and interest groups—has led us to follow a strategy that departs from the norm. What we are proposing, in fact, is a dynamic model of adaptive behavior that is very much in the spirit of the behavioral tradition associated with Simon (1947), March and Simon (1958), Cyert and March (1963), Crecine (1969), Cohen, March, and Olsen (1972), Axelrod (1976), Padgett (1980), Cohen (1981, 1984), and Cohen and Axelrod (1984). Works in this tradition, grounded in the limitations on human decision makers, have long pointed to adaptation and dynamic process as central to an understanding of organizational behavior, and they have pioneered in the development of computer-assisted models of organizational behavior. Because our model is in some sense an application of this theoretical perspective to the study of bureaucratic politics, its success (or lack of it) says something not only about the nature of bureaucratic politics, but also about the value of putting Simon's behavioral tradition to new use.

The Structure of the Model

Our general framework can be applied in a variety of ways. Here we focus on bureaucratic

behavior in a classic type of political environment—one characterized by interest group conflict—and we develop a simple model to explain bureaucratic output, budgets, and efficiency. The model's central components are an agency interested in some combination of bigger budgets, more slack, achieving policy goals, and avoiding oversight;² a legislature made up of 101 elected politicians, each of whom is interested only in getting re-elected;³ and two interest groups, one that benefits from the agency's program and one that is hurt.⁴ None of this is anchored in particular agencies or policy areas. For purposes of illustration, however, we will develop the analysis below with reference to an agency engaged in consumer protection regulation. On this interpretation, which is obviously just one of many possibilities, the agency's output is taken to be the level of regulatory enforcement, and its enforcement activities are assumed to be supported by consumers and opposed by business.

In broad outline, the model is designed to reflect the circular flow of influence characteristic of representative government. Citizens pressure legislators through elections, legislators influence the bureau through budgets and oversight, the bureau affects citizens through the costs and benefits generated by regulatory enforcement—and the circle is closed when citizens link their electoral support to legislators' positions on agency-relevant issues. This iterative process works itself out in the following way.⁵ (See Figure 1.)

Every period, the legislature transmits to the bureau a new budget that may differ by as much as 10% from its budget in the prior period. Each

²Although there is no agreement in the literature on exactly what motivates bureaucrats, these elements are prominently mentioned. Note that our model allows the agency to value just one of these, or any weighted combination, giving us a basis for exploring the implications of alternative bureaucratic goals.

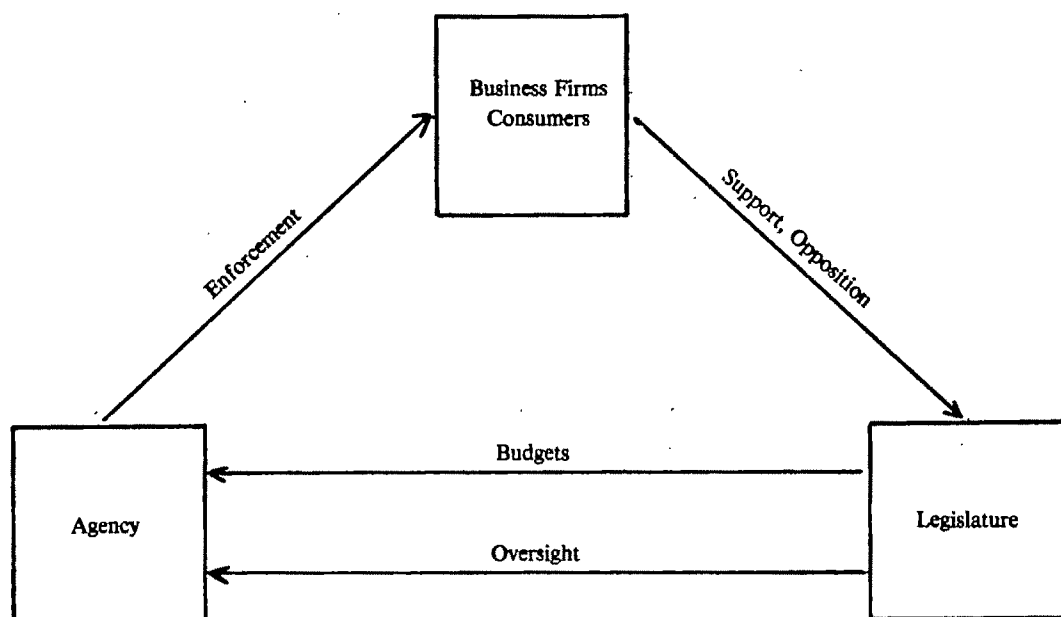
³On the importance of the re-election incentive, see Mayhew (1974) and Fiorina (1974). We chose 101 for the size of the legislature because it is large enough to prevent the institutional dynamics from being obscured by the idiosyncratic behavior of one or a few legislators, and as an odd number, it allows for a clear majority winner. Had we chosen a larger (odd) number, say 501, the model's results would be identical in all essential respects to those presented here.

⁴These groups need not be monolithic organizations, and may even be sets of unorganized citizens. We think of them, abstractly, as sectors that generate votes, money, and other resources in response to political outcomes. Their comparative effectiveness in doing so can be varied by parameter settings of the model.

⁵Readers interested in the finer details may write the authors for a copy of the computer program.

¹For an interesting exception relying on general equilibrium analysis, see Fiorina and Noll (1978).

Figure 1. The Flow of Influence



legislator must take a position somewhere on a scale between -10 and $+10$ percent.⁶ In deciding how to vote, the individual legislator compares his past votes with past changes in the mix of support and opposition he has received from the two interest groups, that is, changes in his electoral security. If he changed his position in a pro-agency direction last period—say, by moving from $+2.5$ to $+3.1$ on the budget scale—and his electoral security subsequently increased, then he will want to take another step in this direction, with the size of the step determined by the size of his utility gain. Similarly, had this earlier move been sanctioned by a drop in electoral security, he would now reverse course and support a smaller, perhaps even a negative, budget increase. The same kind of adaptive logic applies for other possible permutations. Once each legislator has adopted a new position, the entire legislature votes and the median position determines the new budget that is transmitted to the agency.

The legislature also engages in oversight, although it is oversight of a very limited sort. It is

activated in the current period only if the legislature increased the agency's budget in the prior period and subsequently discovers, via feedback from the interest groups, that the agency produced less enforcement with this larger amount of money.⁷ This obvious inefficiency results in "hassling" from the proconsumer legislators (those occupying positions greater than 0 on the budget scale), and the intensity of hassling varies with the political weight of this consumerist contingent (i.e., with the number of legislators to the right of 0, and how far to the right they are on average). If oversight is not triggered in the current period, then the level of hassling decays exponentially.

The legislature's work ends once the new budget and hassling levels are decided. These new quantities are now inputs to the agency that alter its utility, telling it whether the environment is approving or disapproving of its past behavior. As noted, its utility function contains one or more of four potential components—budget, slack (the amount of the budget not devoted to enforcement), hassling, and policy (in the form of an enforcement level the agency prefers to all others,

⁶Initial positions are assigned by random draw from a normal population with mean 0 and standard deviation 4. Hence, the first change in the budget equals the median vote of this random sample. The random character of the first budgetary change does not affect the model's basic results.

⁷This oversight strategy, similar in spirit to the fire-alarm monitoring analyzed by McCubbins and Schwartz (1984) and Weingast (1983), economizes on the information-gathering resources of the legislature.

its ideal point)—and therefore the legislature's impact on its utility will depend in part on precisely what combination of these the agency happens to value. When relevant, budget and slack contribute positively to utility, whereas both hassling and departure from the agency's ideal point contribute negatively. Once the agency has assessed its utility change, it follows an adaptive strategy in setting a new value for its choice variable, efficiency. Thus, if it increased its efficiency last period and was rewarded with an increase in utility, it will increase efficiency again in the current period. And so on for the other logical possibilities.

Efficiency is defined as the fraction of the budget spent on enforcement, and therefore ranges between zero and one. Enforcement is generated as a linear function of the actual amount of money spent on it—that is,

$$\text{enforcement} = c \cdot \text{efficiency} \cdot \text{budget}, \quad (1)$$

where c is a constant representing the units of enforcement purchased by a unit of spending.⁸

Having chosen a new level of efficiency, the agency generates a new level of enforcement which in turn yields new levels of costs for business and benefits for consumers. The business costs imposed by regulation increase at an increasing rate (increasing marginal costs), while benefits to consumers increase at a decreasing rate (decreasing marginal benefits).⁹ Specifically,

⁸One reasonable interpretation of enforcement "units" is person-hours devoted to enforcement. Because there is no unique scale for such units, c can be any positive constant; for convenience, we assume throughout that $c=1$. Similarly, there is no unique scale for measuring budgets. Although the budgets of real agencies can range from the millions to the billions, for simplicity—and because the scale does not affect the model's basic results—we have set the initial budget equal to 100. This, together with our assumptions about efficiency and the constant c , determine the scale for enforcement.

⁹This is a standard assumption and is certainly a reasonable place to start. Yet costs and benefits may take on far more complex patterns empirically, and they may be interrelated in various ways—as, for example, when regulatory costs bring about business failures and subsequent loss of jobs for consumers. Our curves are not inconsistent with many of these scenarios (e.g., diminishing marginal benefits for consumers captures the notion that the rewards of enforcement are diluted by its negative by-products), but new work along these lines, empirical as well as analytical, is clearly needed if our theories are ultimately to be well designed.

$$\begin{aligned} \text{benefits} &= k_1 \cdot \text{enforcement} \\ &- k_2 \cdot \text{enforcement}^2 \end{aligned} \quad (2)$$

$$\text{costs} = k_3 \cdot \text{enforcement}^2 \quad (3)$$

where k_1 , k_2 , and k_3 are positive constants. In our initial version of the model, consumers evaluate the benefits of regulation without taking into account the taxes needed to finance the agency. In the final version, consumers base their evaluation of regulation on net benefits: the value of enforcement minus its tax costs.¹⁰ Under both versions the social optimum is the point at which enforcement's marginal costs and marginal benefits are equal, with budgetary costs incorporated into the calculation.

For both business and consumers, the political stakes rise as enforcement rises, and both therefore devote more resources (campaign contributions, votes, etc.) to the electoral process in support of politicians who promote their interests. Consumers increase their contributions in proportion to the benefits they derive from enforcement; firms increase their contributions in proportion to the costs imposed on them:

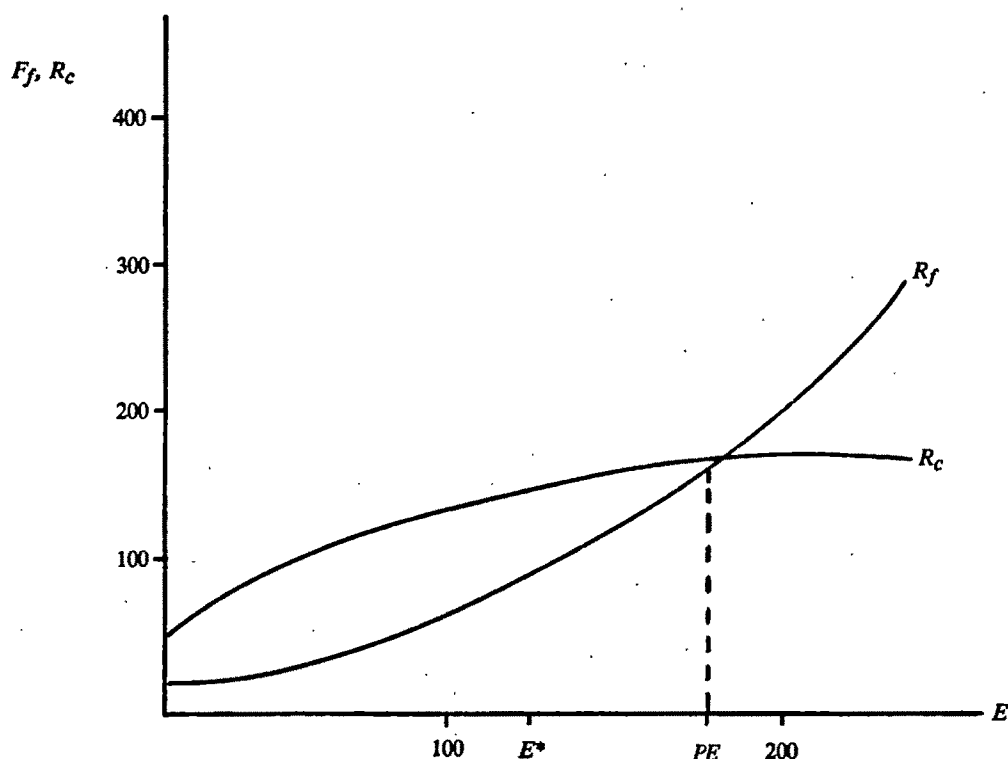
$$\begin{aligned} \text{consumer's political resources} &= k_4 \\ &+ k_5 \cdot \text{benefits} \end{aligned} \quad (4)$$

$$\text{firms' political resources} = k_6 + k_7 \cdot \text{costs}, \quad (5)$$

where k_4 through k_7 are positive constants. (See Figure 2.) Thus, the more an interest group is helped by a program, the more it is willing to contribute to politicians supportive of the agency. Similarly, the more a group is hurt by a program, the more it is willing to contribute to politicians who oppose the agency. Empirically, however, the existing system of interest organizations is unbalanced in favor of business, and this imbalance is enhanced by free-rider problems that disproportionately plague the emergence of new consumer groups (Olson, 1965; Stigler, 1971). There is accordingly a closer correspondence in the model between additional costs imposed on business and its political contributions than is true of the analogous benefit-to-contributions translation for con-

¹⁰Because the budgets of regulatory agencies are only a tiny fraction of the federal budget, it is a reasonable first approximation to assume that an agency's beneficiaries are more aware of benefits than tax costs. Moreover, we gain a clearer understanding of the distinctive contributions of the model's various components by dealing first with the simple nontax world and introducing taxes later.

Figure 2. The Pluralist Equilibrium in the Taxless Model

*Cost and Benefit Functions (not graphed)*

$$\text{Costs} = .023E^2$$

$$\text{Benefits} = 20E - .04E^2$$

E = Enforcement

PE = Pluralist Equilibrium

E^* = Social Optimum

R_f = Political Resources of Firms

R_c = Political Resources of Consumers

Resource Functions

$$\begin{aligned} \text{Firms } R_f &= 10 + .20 (.023E^2) \\ &= 10 + .20 (\text{costs}) \end{aligned}$$

$$\begin{aligned} \text{Cons. } R_c &= 50 + .05 (20E - .04E^2) \\ &= 50 + .05 (\text{benefits}) \end{aligned}$$

sumers. (In equations (4) and (5), constant $k5$ is smaller than constant $k7$.)¹¹

¹¹Note that our interpretation of organizational advantage is that firms are better able to respond to changes in enforcement than are consumers. This in itself says nothing about the absolute level of resources when enforcement is zero. We assume, for present purposes, that when there is no enforcement, consumers have more political resources than firms do. This makes sense for the following reasons: consumers are numerous, they vote, voting requires little organization, and when there is no regulation, the issue is likely to be salient to voters. Moreover, the opposite assumption is easily accommodated by our model, as we suggest in note 24 below.

Once business and consumer groups decide upon resource levels, they expend them for or against individual legislators in accordance with the latter's positions on agency budgets. Legislators favoring budgetary increases are judged as pro-agency and therefore as proconsumer; they get resource support from consumers and resource opposition from business in amounts that are a positive function of the size of the budget increase they favor.¹² The same logic

¹²In versions of the model where consumers take taxes into account, consumers will support legislators voting for smaller budgets if the taxes required to finance the

applies for legislators favoring reduction in the agency budget.

Legislators now find that owing to their earlier adaptive shifts in position on the budgetary scale, they face altered patterns of electoral support and opposition—that is, changes in their utility—anchored in the adjusted evaluations of business and consumer groups. This new feedback prompts them to adapt by choosing a new budgetary position for the coming period, following the decision rule outlined above. This completes the circle and sends the process into another iteration.

Before summarizing the results, we would like to pause briefly to underscore a few basic aspects of this model. First, all of its decision makers are self-interested: they seek to realize their own goals, whatever they might be, and they are not directly concerned with the well-being of other participants.¹³ Thus, if the model generates societally attractive outcomes, or if one participant makes decisions beneficial to one or more of the others, these will rarely be intended consequences, but rather the by-products of self-interested behavior. In this respect, we adhere to a central theme of rational choice models of regulation and bureaucracy, and we adhere as well to a classic line of pluralist theory.

Second, our participants are endowed with very little knowledge. Legislators do not know the marginal electoral value of another dollar added to the regulatory agency's budget. Bureaucrats do not know the marginal gain of one more unit of enforcement. What these decision makers do know is rather modest. Legislators know such things as how they voted on the previous appropriations bill and how full their campaign war-chests are. The regulatory agency knows only what percentage of the budget it has devoted to enforcement and how well off it is; it knows nothing directly of the effects of regulation, and it must choose a level of efficiency in ignorance of the true consequences of its actions. In short, as incrementalist analyses of the political process have long contended, these participants adapt as best they can based on what little they know: They muddle through (Braybrooke & Lindblom, 1963; Lindblom, 1959).

Third, our participants are simple adaptive decision-makers. They learn about their environ-

ments only to the extent that they link new information about changes in behavior with new information about changes in utility, and they adapt by following a trial-and-error procedure that prompts them to repeat rewarded actions and to avoid sanctioned ones. They do not generalize or otherwise develop belief structures about their environments, nor do they try to guess the future. The specific model we use here, then, assumes perhaps the simplest possible adaptive scheme—a reasonable place to start, given that we are moving into uncharted territory. More complex adaptive strategies can always be introduced once simple models of this sort are better understood.¹⁴

Fourth, the distribution of influence is typically pluralistic: no one is in charge. The structure of modern democratic government virtually guarantees that citizens, politicians, and bureaucrats will interact through a network of relationships in which no single participant can truly dominate or be entirely autonomous, and in which each must adapt to the decisions of others.

Finally, the model emphasizes the role of indirect influence, of the importance—more, the necessity—in American politics of getting what you want via other decision makers. The bureau likes bigger budgets, but cannot directly influence the legislature. Its behavior directly affects the interest groups, but these groups cannot influence the agency directly; they must try to induce the legislators to move the bureaucracy in the desired direction. The legislators' electoral fortunes are directly affected by the groups, but in order to better their chances the legislators must work through the agency. These indirect paths of influ-

¹⁴The adaptive strategy our actors follow, a simple trial-and-error procedure operations researchers call "hillclimbing," is a weak strategy: it gets trapped on local maxima, makes inferential errors, and is often quite slow. And its implied cognitive processes underestimate the sophistication of real decision makers. But positing trial-and-error yields a significant modelling benefit: although a weak strategy, it is highly general (Rich, 1983). Unlike most optimization techniques, hillclimbing can be used in an extraordinarily wide variety of task environments—singlepeaked or multi-peaked, linear or nonlinear, deterministic or stochastic—and the modeller can avoid specifying detailed beliefs and heuristics of the decision makers. In contrast, when psychologists model a problem-solver facing a particular task, they find that the heuristics are extremely context-specific. Chess heuristics do not closely resemble theorem-proving heuristics (Newell & Simon, 1972). Since political scientists are more interested in systemic behavior than in individual behavior, we are willing to trade some accuracy at the microlevel for generality at the macrolevel, and in the class of adaptive strategies, hillclimbing is one of the most general.

bureau outweigh the benefits of regulation. Typically, however, consumers help legislators voting for larger budgets and oppose those voting for reductions.

¹³This is not invariably true. In some runs the agency has policy goals that do not derive from preferences for budget or slack. These policy goals could derive from more general, nonegoistic ideologies. But the general sense of the model is of self-interested behavior.

ence and the uncertainties they create are the hallmarks of democratic government.

Simulation Results

A computer model of this sort necessarily has many parameter settings, and this can obscure its central tendencies. To highlight major patterns, we will first present simple versions and then add complications one at a time.

Version 1: Budget-Maximizing Agency

Clearly, an agency with only one goal is simpler to analyze than an agency with several. Consistent with most formal theories of bureaucracy, our baseline version is a regulatory agency that cares only about budgets. The outcomes generated by this system—the equilibrium levels of enforcement, efficiency, and budget—are presented in Table 1. We suspect they surprise most readers. Formal models of bureaus, from Niskanen on, have typically concluded that budget maximization leads either to gross inefficiency or gross oversupply of output. Indeed, this is approaching the status of conventional wisdom. But our model suggests quite a different effect of budget maximization: that it can make an agency responsive to the power configuration in its environment.

The system's logic of adjustment is easy to discern. The agency quickly discovers that increases

in its efficiency are rewarded by the legislature—whose members, unbeknown to the agency, are disproportionately rewarded by consumer groups for supporting budget increases; this occurs because, at the initial enforcement level of 80 (an arbitrary starting point), consumer groups out-contribute business groups, and many legislators are motivated to shift to the right on the budget scale.¹⁵ The agency adapts by jacking up its efficiency. The combination of higher efficiency and bigger budgets provided by the legislature rapidly increases enforcement. This increase, however, eventually mobilizes business, and the consumers' early success proves to be self-limiting. Once business outspends consumers (which, given equations (4) and (5), occurs after enforcement exceeds 184), legislators tend to be rewarded for cutting rather than increasing the agency's budget. The legislature therefore reverses course—yet it will not go far below 184, for then consumers begin to outspend business again, and a legislator who continues to vote against the agency will tend to see his opponent's war chest rise.

These oscillatory adjustments damp out over time as the system settles down to its equilibrium enforcement level of 184: the level at which the

¹⁵We experimented with different initial enforcement levels. Equilibrium outcomes remained the same.

Table 1. Simulation Results

Version	1	2	3	4	5	6A	6B
Parameters							
Budgets	Yes	Yes	Yes	Yes	Yes	No	No
Slack	No	Yes	No	No	Yes	No	No
Policy	No	No	No	No	No	Bus	Con
Oversight	No	No	No	No	Yes	No	No
Adaptation	Fast	Fast	Fast	Slow	Fast	Fast	Fast
First Move	Incr	Incr	Decr	Decr	Incr	Incr	Incr
Nontax model							
Enforcement	184	Deg	Deg	184	184	CR	184
Budget	249	Deg	Deg	360	371	CR	196
Efficiency	.74	Deg	Deg	.51	.50	CR	.94
Tax model							
Enforcement	178	87	56	171	170	101	174
Budget	210	1509	1485	321	382	1473	197
Efficiency	.85	.06	.04	.53	.45	.07	.88

Key. Deg, Degenerate (enforcement and efficiency ≈ 0)
 CR, Compromise
 Con, Proconsumer
 Bus, Probusiness
 Incr, Increase
 Decr, Decrease

political resources of business and consumer groups are equal (as described by the intersection of their resource functions—see Figure 2). We call this the pluralist equilibrium, because it obviously formalizes the old pluralist idea of a balance of group forces. As the system approaches this equilibrium level, the other key variables—efficiency, the budget—hit steady-state values consistent with it, and legislators become tightly distributed around the median position of 0 on the budget scale.

In a pluralist political environment, then, the budget-maximizing agency of Version 1 cannot impose output levels that exceed the amount determined by the environment's power balance. Here, unlike in the usual formal models of bureaus, any oversupply of output is due not to the agency's lust for revenues, but to a power mismatch in the bureau's environment.¹⁴ Because legislators are responding to electoral rewards and sanctions from interest groups rather than to the blandishments of the bureau, the legislature simply refuses to provide the agency with appropriations that, combined with its high efficiency, would yield so much enforcement that the electoral chances of incumbents would be dimmed. The agency would surely like to obtain much larger budgets—but legislators have no incentive to go along, and they hold the purse strings. Empirically, this result makes perfect sense.

In part, the intuitive appeal of the excessive output hypothesis derives from a specific class of policy sectors, where the high demand interest group is well organized. This includes porkbarrel projects (Shepsle & Weingast, 1981) and, more generally, policies that create compact winners and diffuse losers. But, as Wilson (1980) has argued, many policy sectors lack this property, and in these the excessive output hypothesis is less plausible. Many regulatory agencies either have compact losers as well as compact gainers (the NLRB, the ICC) or, still worse from the stand-

point of the excessive output hypothesis, diffuse gainers and compact losers (the EPA, the Consumer Product Safety Commission). Our model incorporates Wilson's insight that the relative compactness of interest sectors influences bureaucratic outcomes. It does so by means of two parameters representing the fraction of the benefit or loss deriving from enforcement that is subsequently translated into political resources. The more compact the group, the higher the fraction. Because empirically business is more compact than consumers, the business parameter exceeds the consumer parameter in all versions of our model, and the pluralist equilibrium is therefore farther to the left—that is, the two resource curves intersect at a smaller level of enforcement—than would be the case if the groups were equally able to translate costs and benefits into political resources. Marginal changes in parameters from these settings have predictable effects. As consumers become more compact, the pluralist equilibrium shifts to the right, and as business becomes more compact, the pluralist equilibrium shifts to the left. Because outcomes are determined by the pluralist equilibrium in this version of our model, changes in relative compactness produce—through a chain of interactive adjustments—changes in regulatory performance, which formalizes Wilson's assertion that bureaucratic outcomes reflect group compactness and diffuseness.

Version 2: The Effect of Slack

Presumably bureaus prefer bigger budgets because of what they can buy, such as greater prestige, higher salaries, and more output or perquisites. Version 1 left those higher-order goals unspecified. We now specify one such goal: slack, defined here as appropriations devoted to any use other than output. The agency in Version 2 seeks slack in addition to larger budgets. Table 1 reveals the effect on enforcement, efficiency, and the budget. They differ dramatically from Version 1's pluralist equilibrium. In Version 2, efficiency plunges to zero. Ironically, though *all* the legislators quickly start voting in accord with consumers' desires, the consumers do not benefit at all, because enforcement is fast approaching zero despite the constantly increasing budget. This is puzzling: everyone is happy except consumers, even though they outspend firms in *every* period. Unlike Version 1, this result does not accord with intuitions based on simple power reasoning. What is going on?

The explanation is simple. A slack-seeking

¹⁴This difference between our model's results and Niskanen's reflects different assumptions about the distribution of resources rather than differences in decision-makers' goals. In his model, the deck is stacked in favor of the bureau: it has crucial informational advantages and in effect imposes outcomes on the legislature (Miller & Moe, 1983). In our model, because key resources are not monopolized by the bureau, outcomes are jointly determined. In a system with a pluralistic distribution of power, budget-maximization by a bureau does not necessarily lead to superoptimal budgets, just as in a competitive market, profit-maximization by a firm does not necessarily lead to superoptimal profits. (Indeed, in a perfectly competitive market, firms earn zero profits.) Such systems often do not exhibit simple links between individual motives and collective outcomes.

bureau quickly discovers that it can improve its well-being by lowering its efficiency. Because it decreases productivity faster than the legislature increases its budget, output falls. As enforcement falls, consumers continue to outcontribute firms. But though the legislature is responding to the wishes of consumers, it has only one instrument, the budget, and budgets are singularly ineffective instruments in such circumstances. A proconsumer legislature is caught in a bind. On one hand, because the more powerful interest group desires more output, it makes some sense to give the agency more money. In this line of reasoning, budgets are resources needed to produce enforcement. But if the agency is a budget-seeker, both intrinsically *and* because it has multiple uses for appropriations, then revenues are also incentives. Thus increasing the budget not only gives the bureau more resources to do its job, but also rewards it for past behavior. Here the reward is inappropriate, since the agency's inefficiency caused the problem in the first place. But if budgets are the only instrument, what else can the legislature do? It does not make sense, at least not in the short run, to deal with a problem of diminished output by denying the agency the resources necessary to produce more output. Thus the bind, produced by a combination of the bureau's monopoly position and the legislature's impoverished repertoire, generates a pathological cycle, a degenerate case of huge budgets and negligible output.

The dilemma the legislature faces in trying to use the budget as both a production mechanism and an incentive mechanism has been recognized in the empirical literature (Quirk, 1981), and it emerges quite naturally from our model. By contrast, the conventional notion associated with Migué and Bélanger (1974) and Niskanen (1975) is that slack seeking leads to smaller, more nearly optimal budgets and outputs. In their models, a preference for slack prompts the agency to maximize the difference between the budget and production costs rather than simply the size of the budget itself, and this leads it to prefer (and receive, given its alleged power) a budget-output combination that is closer to the social optimum than the combination preferred by a budget-maximizing agency. In our model, slack seeking makes the situation unambiguously worse. Budgets explode rather than shrink. Outputs (enforcement) do shrink, but they go to zero as the agency chooses to become totally inefficient. And the agency is consistently rewarded for this behavior by a proconsumer legislature that throws money at it in order to gain the electoral support of consumer groups. Other things being equal, society is far better off with an agency solely concerned with maximizing its budget.

Version 3: Superstitious Learning

Noting that the agency in Version 2 garnered much bigger budgets than that in Version 1 did, the reader may now doubt our explanation of Version 1's behavior. If reducing productivity can procure such enormous budgets, why did the budget maximizer of Version 1 increase its efficiency? Is that adaptive behavior?

Adaptive, yes; optimal, no. The fundamental attribute of an adaptive decision-maker is pragmatism: if an action worked once, try it again. Subtle causal inferences about *why* an action paid off are avoided. This strategy opens the door to inferential errors. An action may have worked because of characteristics of the environment rather than of the decision. This is so in Version 1. Because initial enforcement is less than the pluralist equilibrium, the legislature will vote for larger budgets *regardless* of how the agency behaves. This is what learning theorists call a benign environment: anything the agency tries is rewarded. The combination of a benign environment and adaptive decision making renders the bureau's first move important, for it is the first move that, given the inevitable reward, leads the agency down one path (greater efficiency) or the other (less efficiency).¹⁷

The explanation for Version 1's behavior is now apparent. The agency's first move is a random choice: lacking experience to guide it, the bureau is equally willing to try increasing or decreasing efficiency. Version 1 is unlucky and increases efficiency. It is rewarded for doing so. It repeats the choice, and again is rewarded. Convinced it has discovered the road to bureaucratic paradise, it steadily boosts its productivity. The agency is the victim of *superstitious learning*: misunderstanding the causal structure of its environment, it has erroneously attributed its success to its own conduct.¹⁸

Confirmation of superstitious learning is provided by Version 3. Luckier than Version 1, its first move is to decrease efficiency. It too is rewarded by a larger budget. Equally convinced

¹⁷Ours is a model of deterministic adaptation: if increasing efficiency is followed by more utility, then the agency will increase efficiency in the next period with certainty. In a model of probabilistic adaptation, success would increase the odds that the agency would become more efficient. Probabilistic adaptation is empirically more realistic, and also less likely to result in superstitious learning because, even after an initial success, a decision maker has a chance of trying another alternative.

¹⁸It is well known that adaptive behavior in benign environments produces superstitious learning (Lave & March, 1975).

of its brilliance, it continues to reduce its productivity. The consequences are virtually identical to Version 2's explosive results: huge budgets, complete inefficiency, negligible output (Table 1). The control pathology of Version 2 is repeated. The proconsumer legislature teaches the bureau that inefficiency pays.

Therefore, the new conventional wisdom is not entirely wrong. If the legislature has only one instrument (the budget) and constituents ignore taxes, an adaptive budget-seeking bureau is as likely to become inefficient as it is productive. Indeed, only the extreme myopia of deterministic adaptation made the agency of Version 1 wind up at the pluralist equilibrium: the agency in Version 2 is far better off.

Version 4: The Rate of Adaptation

The superstitious learning of Version 3 introduced the idea that the power characteristics of the bureau's environment are mediated by institutional structure (the legislature's instruments) and by the agency's adaptive strategy. Unlike a simple pressure model, in our model the power of interest groups, though important, is not conclusive. Version 4 explores the effects of a second property of adaptation: the speed of adjustment.

A budget-maximizing agency is by definition indifferent to policy and therefore has no intrinsic desire to lower enforcement. Yet in Version 3, though consumers outspent firms, the agency drove enforcement to zero. Because enforcement = efficiency \cdot budget, the agency was reducing efficiency faster than the legislature was increasing the budget. This observation suggests that the agency's rate of adjustment is crucial. Version 4, keeping everything else constant, introduces an agency that adapts more slowly. Table 1 records the effects of this change on the equilibrium enforcement, budget, and efficiency. Because Congress pumps in funds faster than the bureau becomes unproductive, enforcement reaches and temporarily passes the pluralist equilibrium. When this occurs, the legislature reverses its practice of granting it larger budgets, and the agency for the first time gets negative feedback suggesting that still lower efficiency is unwise. The eventual result is the pluralist equilibrium.

There are two important lessons here. First, the pluralist equilibrium of Version 1 is more than an accidental by-product of superstitious learning. The inertia introduced in Version 4 ultimately destroys the agency's benign environment and requires that it make choices in the face of negative feedback. That it settles down at the pluralist equilibrium, rather than somewhere else, indicates that there is something truly general about this particular level of enforcement. The second lesson

is that inertia performs a systemic function of real importance in this version of the model. Because bureaucratic inertia is ritually condemned in both popular and scholarly accounts, this is an intriguing result that underlines the value of further inquiry into its positive aspects.

Version 5: Oversight

Clearly the consumers are handicapped by the paucity of instruments available to the legislature. The sole response in Versions 2 and 3 to declining enforcement is to throw more money at the bureau, but this only teaches the agency to become still less productive. In the real world, however, such a vicious spiral could not continue indefinitely, for society would eventually be devoting all of its resources to regulation, and such a system is not viable. Systems that survive, therefore, must have mechanisms for checking these explosive forces. One such mechanism is taxes. We will turn to this shortly. Here we focus on a second mechanism, legislative oversight.

Specifically, Version 5 of the model endows the legislature with an oversight capacity to complement its budgetary authority, and assumes in addition that the bureau dislikes the hassling that oversight entails. Note that the latter condition is necessary if oversight is to have any impact on bureaucratic behavior. A bureau insensitive to hassling will simply ignore the legislature's complaints. Although this may certainly happen empirically (in fact, legislatures probably feel it happens all too often), it means that oversight would be superfluous to the model.

In keeping with the model's overall theme that decision makers rely on simple heuristics, the oversight mechanism is exceedingly crude. It is activated whenever two events occurred in the previous period: first, the legislature gave the agency a bigger budget; second, enforcement declined. Although we assume that legislators do not have a good grasp of the agency's internal affairs, they do hear about the enforcement level from the interest groups, and of course they know what the budget is. These two pieces of information suffice to yield an inference of mismanagement whenever the agency produces less output with more money. In such instances, the legislature hassles the agency with time-consuming hearings, bad publicity, threats to bureaucratic careers, and similar measures.

This mechanism is crude, but it turns out to be surprisingly potent. As we saw in Version 2, an agency that likes slack and budgets creates the degenerate solution: an explosive budget with efficiency and enforcement moving to zero. Version 5 takes the same context and introduces oversight. Simpleminded and myopic though it is, epi-

sodic legislative hassling has a striking impact on the process of bureaucratic adjustment. The agency is no longer in a benign environment: as it moves toward the degenerate solution, the combination of bigger budgets and drops in efficiency produce mounting sanctions in the form of hassling, until finally the costs of greater inefficiency outweigh the benefits. The bureau then reverses course. After a series of adjustments and counter-adjustments, it winds up at the pluralist equilibrium. Thus, legislative oversight interrupts the process of superstitious learning and saves the system from the degenerate solution, producing the same equilibrium level of enforcement as Version 1's budget-maximizing model—although, owing to its circuitous time path, at a higher budget and lower level of efficiency.

Oversight does not produce socially optimal behavior, nor does it guarantee legislators the best of all possible electoral worlds.¹⁹ It does, however, perform a crucial systemic function that puts a rein on the agency and allows regulation to "work." This occurs even though the legislature knows virtually nothing about bureaucratic costs, performance, or efficiency, and even though it reacts only on those occasions when the evidence of inefficient performance is painfully obvious.

The impact of oversight also helps to illustrate the special status of the pluralist equilibrium. We have presented five runs of our model now; two have produced the degenerate solution, three the pluralist equilibrium. In fact, it is a general property of our model (in the absence of taxes and agency policy preferences) that these are the only two solutions. The pluralist equilibrium, therefore, appears to be the natural equilibrium of a stable system, for it is the only solution compatible with the survival of the system as a whole.

Version 6: Agency Policy Preferences

In their motivational assumptions for bureaus, the standard formal models have clearly been influenced by the analogy of profit- or revenue-maximizing firms. Hence the emphasis on budgets and slack. But although it is ordinarily true enough that firms are not directly motivated by output, this is a less plausible assumption for bureaus, for there is good evidence that bureau-

cratic officials are often motivated by policy preferences (Aberbach & Rockman, 1976). The battles between liberal career civil servants and conservative political appointees in the Nixon and Reagan administrations are hardly battles over budgets and slack. They are battles over policy, over different notions of the "ideal" levels and directions of bureaucratic performance.

To isolate the effect of policy preferences, Versions 6A and 6B introduce agencies that care only about policy. We assume throughout that the agency's preferences are those of its dominant coalition, and that the composition of the dominant coalition is strongly influenced by presidential appointments. Although a more comprehensive model would surely include the president in an explicit fashion, this at least gives us an indirect opportunity to investigate whether presidents can shape regulatory performance by, in effect, changing some of the parameters of the system. In a regulatory context where firms oppose consumers, it is reasonable to categorize the agencies as either probusiness (perhaps reflecting appointments by a Republican president) or proconsumer (perhaps due to Democratic appointments). Abstractly, we consider an agency to be probusiness if its ideal enforcement level is less than the social optimum, as proconsumer if its bliss point exceeds the social optimum. To minimize ambiguity, we proceed by comparing a probusiness agency whose ideal level of enforcement is 100 (smaller than both the social optimum of 150 and the pluralist equilibrium of 184) with a proconsumer agency whose ideal point is 300 (larger than both).

Not surprisingly, a probusiness bias leads to less enforcement than the pluralist equilibrium. Seeking to reach its optimal enforcement of 100, the bureau tends to diminish its efficiency. However, the agency's success in reducing enforcement mobilizes consumers, which ensures a proconsumer legislature. Thus ensues an odd conflict between the two branches: the legislature pushes money on a reluctant bureau, which combats the added revenues by further decreasing its efficiency. The legislature responds by giving still more money, and the pattern continues. The result is that enforcement cycles in a compromise region bounded by the agency's ideal policy position and the pluralist equilibrium. As enforcement cycles, budgets eventually explode, and efficiency plunges virtually to zero.

Thus, a characteristic feature of a system where the pluralist equilibrium exceeds the agency's bliss point is conflict between the legislative and executive branches. (Our simple model does a fair job of simulating, for example, the short-term recent conflict between Congress and the EPA.) What happens if the agency is proconsumer? At first

¹⁹As one example of its suboptimality, note that the oversight strategy permits declining productivity if output is rising. Because this can happen only when the budget is increasing, this toleration represents systemic slack: when the budgetary climate is good, for either macroeconomic or political reasons, hierarchical control loosens. There is a strong similarity here to the dynamics of slack accumulation in the behavioral theory of the firm (Cyert & March, 1963).

blush, it appears that a proconsumer bias should produce a result symmetric to the probusiness bias: enforcement would equal a weighted average of the pluralist equilibrium and the agency's bliss point, with ongoing struggle between the legislature and the agency. Yet this is wrong. It reflects an extension of simple power reasoning that is inappropriate given the institutional structure of the model. As Version 6B suggests, the agency's decision variable, efficiency, cannot drive enforcement permanently higher than the pluralist equilibrium. The agency's policy preference has no significant effect. Jacking up productivity briefly augments enforcement, but this disproportionately mobilizes business, shifting the legislature to a budget-cutting posture. Adjustments and readjustments eventually restore the pluralist equilibrium.

To understand the agency's impotence more clearly, imagine that the agency simply sets efficiency equal to 1 and keeps it there. It would then be doing its utmost to reach its ideal point, but its utmost is inadequate. The legislature controls the budget, and legislators will be induced by the parity of interest-group campaign contributions to cut the budget until enforcement equals the pluralist equilibrium.

The asymmetrical impacts of proconsumer and probusiness policy preferences result from the distinctive manner in which the legislative and bureaucratic control mechanisms interact. The legislature can force a budget on a probusiness agency, but it cannot force the agency to spend all (or any) of it on enforcement, for the choice of efficiency is up to the agency alone. Thus, there is a constant struggle giving rise to the compromise region. On the other hand, the legislature can respond to the expansionist designs of a proconsumer agency by cutting its budget, and the agency can only fight back by increasing its efficiency. Once the agency reaches perfect efficiency, budget cuts are fully translated into declines in enforcement, and the agency can do nothing about it. Thus, the pluralist equilibrium.

It is a bit strong to say that a proconsumer bent has *no* influence: agencies that try to drive enforcement beyond the pluralist equilibrium become highly efficient. This result has a curious implication for antiregulation interests: if these interests faced a choice between a proconsumer agency and a budget seeker disciplined by oversight, they should prefer the zealous bureau. The explanation for this counterintuitive preference is that, owing to the power configurations of the bureau's environment, both agencies will wind up producing the same amount of enforcement, but the proconsumer agency will cost less. Thus a realistic assessment of power dynamics should make antiregulation forces look more kindly upon

career bureaucrats who singlemindedly support their opponents.²⁰

Overview: Basic Types of Solutions of the Model without Taxes

Although we have thus far analyzed only seven versions, the reader has been introduced to all the basic types of solutions attained by our much-more-extensive experimental manipulation of the system. Despite the combinatorial complexity of alternative goals, adaptation rates, congressional instruments, and initial values, all the versions reach one of three outcomes: the pluralist equilibrium, the degenerate solution, or the compromise region.²¹

Analyzing the versions within each type strengthens the inferences made on the basis of pairwise comparisons. Consider first the degenerate versions. With only one exception, all runs producing the degenerate solution were characterized by three properties: the agencies like slack, they adapt quickly, and Congress lacks oversight. When the three properties are present, the agency's policy preferences are irrelevant, as is budget-seeking.²² Normatively, the combination of a slack-seeking bureau and a legislature that

²⁰In real bureaucracies, policy goals usually come along with less lofty objectives. What happens when the agency seeks other goals—budgets, slack, and avoiding oversight—in addition to policy? The three familiar patterns are recreated. Adding slack to the agency's motivation recreates the exploding pattern of Version 2: huge budgets, abysmal inefficiency, little enforcement. Adding budgets is less damaging. The budget seeking probusiness agency merely cycles closer to its ideal point, and the proconsumer agency is basically unaffected. Thus, slackseeking and budgetseeking differ more than is usually believed. Because revenues are allocated either to slack or output, a preference for more perquisites directly impairs efficiency. Budgetseeking lacks this directly corrosive effect. Finally, applying oversight to a slackseeking proconsumer bureau restores the pluralist equilibrium; overseeing a slackseeking probusiness agency reproduces cycling in the compromise region. (The authors will provide a complete table of results upon request.)

²¹Although one cannot prove that a simulation model will remain confined to a given pattern for all parametric combinations, extensive experimentation makes us confident that realistic parametric settings result in one of the three solutions.

²²We should note that these statements pertain to "on-off" versions of policy preference, that is, certain parameters are set equal to 1 (on) or 0 (off). One can increase the relative importance of any goal by increasing a parameter's value from 1 to, say, 3. We experimented with these possibilities, and found that no new types of solutions appeared. Sometimes, however, a version would switch from one type to another.

does not monitor is unfortunate. Empirically, we believe the combination to be rather rare.

All of the versions attaining a compromise solution share one property: a probusiness policy preference. No other attribute much mattered. If the agency lacked a business orientation, no combination of the other parameters would drive it into the compromise region; if it had it, there were few combinations that would move it out of the region.

Four patterns characterize the pluralist equilibrium category. First, most proconsumer agencies reached the pluralist equilibrium. The only ones failing to do so liked slack and were not overseen. Second, of the bureaus indifferent to policy, all those disciplined by the crude oversight procedure reached the pluralist equilibrium. Third, the rate of adaptation matters. Surprisingly, fast learning is not always a blessing, nor slow learning always a curse.²³ Several degenerate versions are transformed into pluralist equilibrium solutions when they adapt slowly rather than quickly. A slow adjustment rate means that Congress is increasing the budget faster than the bureau is decreasing productivity. The net result is increased enforcement, indicating that the legislature can push on a string—if it pushes fast enough and is willing to pay the price.

Fourth, although all the pluralist equilibrium versions produced the same amount of regulation, they did so with varying degrees of efficiency. Of those that were most efficient, not one both sought slack and did so with impunity. There were some bureaus that liked slack, but in all these cases the legislature monitored it. Finally, many more proconsumer than probusiness agencies are very efficient pluralist equilibrium versions.

The Final Step: Introducing Taxes

It has been convenient thus far to assume that firms and consumers ignore taxes, treating governmental policy as free goods or bads generated by the bureau. There are empirical grounds for using this as a starting point. The budgets of regulatory agencies are typically small compared to either the national budget or the social costs and benefits of regulatory policy. In such contexts a model without taxes is a reasonable approximation. There are also important analytical grounds. This approach has allowed us to develop a theoretical foundation that highlights the basic forces

of the system—forces that, it turns out, are constrained and masked by the role that taxation ultimately plays. By waiting until the final step to introduce taxation, we can better understand the more general model and the distinctive contributions of its parts.

Taxes might be added to the model in different ways. To keep matters simple, we assume that all budgets are financed by taxes and that all taxes are paid by consumers. Little is lost by assuming that firms escape taxation, since they already oppose regulation anyway. The interesting effects are on consumers.

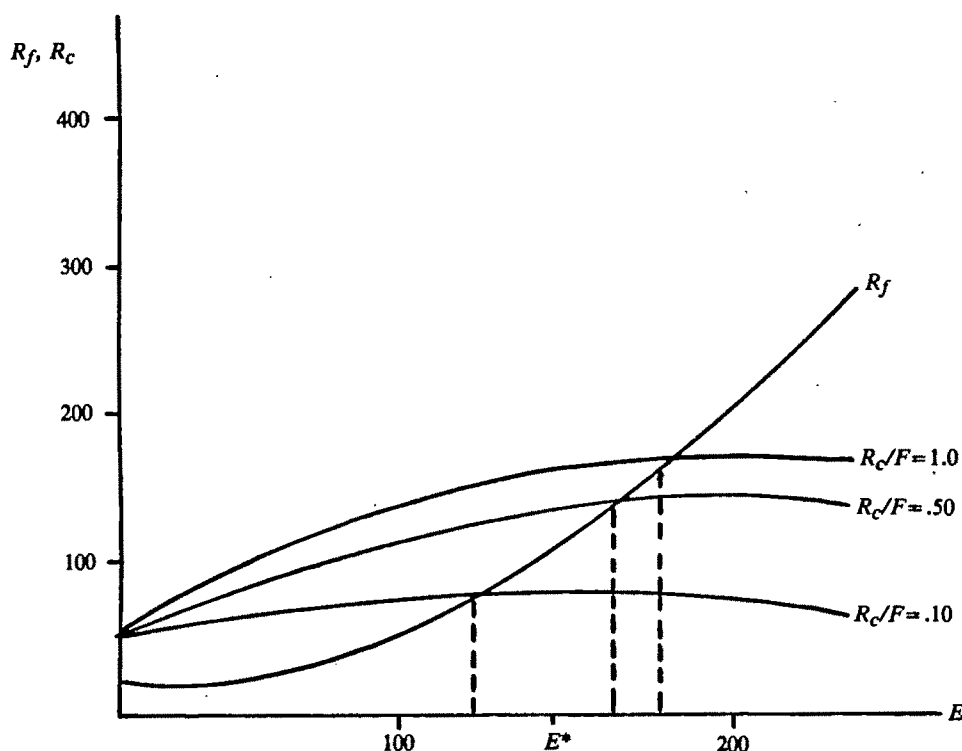
Formally, these effects derive from a change in how consumers calculate the benefits of enforcement: we now assume that the value consumers ascribe to regulation equals the benefits of regulation minus its tax costs, where taxes just cover the agency's budget. What they receive they must now pay for. As a result, how much consumers value any level of enforcement depends on the budget required to produce it, and therefore on agency efficiency. As efficiency increases, the required budget decreases, taxes decrease, and consumers gain. More generally, the consumer benefit function shifts every period as the agency chooses new efficiency levels. It has become an endogenous component of the model.

This has two far-reaching consequences. First, because the consumer resource function is anchored in the consumer value function, it no longer describes a fixed relation between enforcement levels and resource contributions. It, too, is dynamic. As agency efficiency increases, the resource function shifts upward, reflecting greater consumer value and support of agency efforts at each enforcement level. Second, because the consumer resource function is now dynamic, there is no longer a unique point at which the consumer and business resource functions intersect. That is, there is no longer a single pluralist equilibrium. Instead, as illustrated in Figure 3, shifts in the consumer resource function define an infinite number of pluralist equilibria, each representing a potential balance of power for the system. Thus, although the meaning of the pluralist equilibrium remains unchanged, it is no longer an exogenously determined point to which the system gravitates. The pluralist equilibrium itself has become endogenous.

What can we say about political behavior in this new world of taxpaying consumers? Following the same logical steps as before, computer simulation indicates that this is one of those fortunate cases in which adding complexity creates simpler, more unified patterns of behavior. The salient transformation is that the degenerate solution and the compromise region of our earlier model now vanish—with rare exceptions, all system outcomes

²³The seemingly paradoxical benefits of slow learning have been noted before: see, for example, Lave and March (1975).

Figure 3. Pluralist Equilibria in the Tax Model

*Cost and Benefit Functions (not graphed)*

$$\text{Costs} = .023E^2$$

$$\text{Benefits} = 20E - .04E^2 - \text{budget}$$

E = Enforcement

F = Efficiency

E^* = Social Optimum

R_f = Political Resources of Firms

R_c = Political Resources of Consumers

Resource Functions

$$\text{Firms } R_f = 10 + .20 (.023E^2)$$

$$= 10 + .20 (\text{costs})$$

$$\text{Con. } R_c = 50 + .05 (20E - .04E^2 - \text{budget})$$

$$= 50 + .05 (\text{benefits})$$

are pluralist equilibria.²⁴ What we have is a very stable balance-of-power system.²⁵

²⁴Of course if the slopes and intercepts of the resource functions were significantly altered, the system would not necessarily reach a pluralist equilibrium. With resources continuing to be linear functions of convex costs and concave benefits, there are two cases to consider. First, if the firms' resources exceed the consumers' at all enforcement levels, then the system would obviously not attain a pluralist equilibrium. Instead, no enforcement would be produced. Second, if firms' out-mobilize consumers at very low and very high enforcement levels, but the consumers out-mobilize firms at intermediate quantities, there are two locally stable out-

comes: the interior solution of the pluralist equilibrium and the corner solution of zero enforcement. In this case the more diffuse group has to overcome a mobilization threshold in order to sustain the program.

²⁵The exceptions are odd cases of persistent instability. One is a proconsumer agency that values slack, which fluctuates indefinitely. Experimentation with this case shows that if policy is weighted a bit more or slack a bit less in agency utility, equilibrium is reached. Thus, here and elsewhere, instability is the exception, not the rule. Moreover, in contrast to the nontax model's

To see why the degenerate solution disappears, consider an agency that seeks both slack and budgets. In the taxless model, this bureau decreased its efficiency relentlessly. Because it reduced enforcement, consumers were more mobilized than firms, inducing legislators to pump up the bureau's budget. Rising revenues taught the bureau that sloth pays. It therefore became still less efficient, output fell, legislators pumped in more money, and the cycle continued, leading to exploding budgets and zero efficiency. But in the tax model, consumers recognize the cost of the bureau's profligate ways: their support for the agency diminishes as efficiency declines, thus giving legislators less incentive to add budgetary fuel to the bureaucratic fire. Eventually, the alienation of support becomes so large that the bureau can no longer gain from reducing efficiency, and it reverses course, ultimately settling down—with the rest of the system—to a stable balance-of-power equilibrium.

More generally, the forces that earlier produced the degenerate solutions of zero efficiency, zero enforcement, and infinite budgets are constrained by the incentive-effects of taxation, which induce consumers and therefore legislators to put on the brakes. The result is hardly an absolute blessing for consumers, since enforcement and efficiency are low and budgets are much higher than necessary. But the system is in stable equilibrium, positive levels of enforcement are achieved, consumers do realize net benefits on the exchange, and all of this is far preferable to the degenerate solution.

Similar logic explains why the compromise region vanishes in a world with taxation. Recall that the compromise region arose because of a policy difference between a probusiness agency and the induced preference of the legislature, which is striving (in effect) toward the pluralist equilibrium. The agency, trying to reach its probusiness bliss point, reduces efficiency; the legislature responds by throwing money at it, which produces a see-saw contest in which enforcement cycles within a region bounded by the agency's bliss point and the pluralist equilibrium, accompanied by plummeting efficiency and exploding budgets. When consumers internalize taxes, however, the legislature finds itself at a disadvantage in its struggle with the agency. The legislature's crucial weapon is now treated as a cost by consumers, who will not long support throwing

money at the agency. Budgetary increases diminish the consumer resource function, shifting the pluralist equilibrium to the left. The compromise region therefore contracts—but, because the agency's bliss point is fixed, this contraction is entirely due to a movement of the pluralist equilibrium toward the agency ideal. The net effect of these new forces is to destroy the compromise region, along with the rising budgets and declining efficiency that upheld it. It is replaced by a stable equilibrium very near the agency's bliss point. For a probusiness agency motivated purely by policy, efficiency is very low (.07) and enforcement is virtually right at the agency's ideal (101). But all the relevant values are quite stable, and balance of power prevails in the environment.

As in the degenerate case, then, the forces creating the compromise region continue to operate: the struggle between the agency and the legislature is underpinned by an explosive potential for infinite budgets and zero efficiency. But this potential is constrained by the disciplining role of taxation, which saves the system from budgetary disaster—and, in the process, creates a context in which business firms are better able to get what they want from government.

Aside from the elimination of the degenerate solution and the compromise region, the tax model is virtually identical to our earlier model in its implications for the system's outcomes. Virtually all outcomes from the tax model are now pluralist equilibria, whose relative values clearly reflect the familiar operation of the same basic set of forces, that is, slack, oversight and so forth have the same effect as before.

Discussion

In this article we have proposed a general framework for pursuing a theory of bureaucratic politics. By design, it incorporates a range of components that more conventional models of bureaucratic politics have simplified away, components that are fundamental to the dynamic interaction among bureaucrats, politicians, and interest groups. In surface respects it cannot avoid being highly complex, and it may appear for that reason to fall into the trap that models of the neo-classical variety have tried to avoid. Yet, as our analysis has shown, its essential property is not complexity but simplicity—and it imposes coherence on bureaucratic politics by drawing upon and integrating the complexity inherent in the substantive context, not by assuming important aspects are irrelevant. Among its most general implications are the following.

1) Under a wide range of conditions, the regulatory system tends toward the pluralist equilib-

degenerate solution and compromise region, the unstable cases in the tax model are not explosive: all variables cycle within reasonable bounds. Even its instabilities are stable by comparison.

rium.²⁶ The logic of adjustment is very much a reflection of traditional pluralist thought. As Truman (1951) argued long ago, mobilization begets countermobilization, and changes in the underlying balance of power among social groups are translated by political institutions into corresponding changes in policy outcomes.²⁷ Due to this process of negative feedback, success in this kind of policy sector is self-limiting.²⁸ As we have

²⁶Is the pluralist equilibrium the same solution that completely rational decision makers would reach? This is an important question, both normatively and empirically, but it has no easy answer. Part of the problem is that there are many ways to specify an optimization model. Analytical choices must be made regarding, for instance, the information available to each actor, how they formulate beliefs including expectations about the future, the nature of their decision rules, for example, maximize expected utility or minimax loss, and how the actors themselves are conceptualized, for example, are the interest groups unitary rational actors, coalitions, or sectors of decentralized decision makers? Obviously, different choices along these dimensions could easily produce different systemic outcomes, so we cannot say in general what a shift from adaptation to optimization might imply. We can, however, offer two specific conjectures. First, if the situation were modelled as a cooperative game—that is, actors can costlessly make binding agreements—with the interest groups taken as rational actors, then we conjecture that if an equilibrium exists, it is to produce the socially optimal amount of enforcement (Coase's theorem). Because in general the pluralist equilibrium is not equal to the social optimum (see text, below), the adaptive model does not converge to the equilibrium attained by these rational agents. Second, if the situation were modelled as a non-cooperative game among perfectly informed legislators, with the interest groups and the agency taken as passive reaction functions rather than as strategic agents, then we conjecture that the pluralist equilibrium is the Nash equilibrium of the game. (We would like to thank an *APSR* referee for suggesting this conjecture.) More generally, however, computing the equilibria reached by optimizers in a multiperiod world of the sort represented here would be very difficult—indeed, a fullscale project in its own right.

²⁷Here, as in virtually all dynamic models, the parameters are held constant in order to investigate the system's long-run tendencies. Because empirically these coefficients may change, one may never observe a regulatory system in equilibrium. Instead, it may always be moving toward equilibrium.

²⁸We must point out that we have assumed that the regulatory policy system is not so important that losing is debilitating. In such a world, the positive feedback of the Matthew effect—"To him who hath shall be given"—creates an unstable system of cumulative advantages (Dahl, 1971). The mobilization of previously unorganized interests such as minorities and environmentalists indicates that the American system is described more by negative than by positive feedback (Landau, 1973).

shown, moreover, this result is entirely consistent with—indeed, to be expected from—rationally adaptive behavior, and it holds even though bureaucratic and legislative participants play distinctive, self-interested roles in creating policy from social inputs. The common notion that rationality and the translation effects of institutions are somehow inconsistent with pluralism, then, is overdrawn. Although more elaborate versions of our model (incorporating, for example, legislative committees) may point to conditions that imply alternative expectations, there is every reason at this stage to stress the contribution that pluralist ideas can make to our understanding of bureaucratic politics, as well as their compatibility with more "modern" lines of analysis.

2) This equilibrium is reached in a groping fashion by decision makers who only dimly understand the effects of their own actions. Thus, it is highly consistent with the descriptive literature on policymaking, which emphasizes the complexity of the policymaking environment, the frequent missteps, the unintended consequences, as well as the "disjointed incrementalism" of policymaking in a polyarchical system (Dahl, 1971; Lindblom, 1965). Indeed, in many ways the model is a formalization of the "intelligence of democracy." With Lindblom, we attribute the intelligence (what there is) of policy outcomes to properties of the system rather than to the brilliance of individuals. Our actors are not brilliant. Like the decisionmakers in the "Science of Muddling Through" (Lindblom, 1958), our actors ignore side effects, consider only a limited range of alternatives, and adapt as best they can to a complex environment. Through the feedback and mutual adjustment occasioned by their interaction, they tend collectively to produce coherent, sensible outcomes that the individuals themselves neither intend nor have the power to bring about.²⁹

3) The system is not socially optimal—but the direction of suboptimality differs from what the critics of pluralism have claimed. Since Olson's (1965) work first appeared, it has been common to emphasize the advantages of compact groups over diffuse groups and to explain the suboptimality of policy outputs via this disparity. In our model, this logic would imply a definite bias in favor of business and against consumers, and thus an equilibrium level of enforcement lower than the social optimum. Yet this argument is not in general true, and for the specific model of this

²⁹Indeed, it is intriguing to note that this system of myopic and adaptive decisionmakers may be converging to the same outcome that would be attained by perfectly informed and perfectly rational legislators engaged in a noncooperative game, as conjectured in note 26.

article it is quite false. The reason turns on the difference between the pluralist equilibrium and the social optimum. Enforcement generates costs and benefits for society, and its level is socially optimal when net benefits are maximized, that is, when the marginal benefits and marginal costs of enforcement are equal. The pluralist equilibrium occurs, however, at the enforcement level for which the political resources of consumers and firms are equal. There are accordingly two crucial dimensions of difference between the two solution concepts. First, the social optimum is based on the cost and benefit curves themselves, whereas the pluralist equilibrium is based on the political resource curves. Second, the social optimum is based on marginal curves; the pluralist equilibrium on total curves. The combination of these factors implies that the pluralist equilibrium may be above or below the social optimum.¹⁰ Since the Pluralist Equilibrium is the "natural" policy outcome of our system, this means that regulatory enforcement may often be above the social optimum, even though consumers are at a distinct organizational disadvantage relative to business. In the model presented here, this is precisely what happens: business is assumed to be four times more effective at translating values into resources, but the pluralist equilibrium usually lies above the social optimum anyway.

4) The power of bureaus to get what they want has been exaggerated. Although bureaus can sometimes move to a paradise of exploding budgets and slack, they cannot do so when checked by even a primitive form of legislative oversight, nor when their adjustment is slowed by inertia, nor when their constituents are aware of the burden of taxes. In the most general case they wind up at the pluralist equilibrium. There, budgets and slack may vary depending upon the time path of the process, but the agency has clearly been "led" by the system's logic of adjustment and has not engineered its preferred bureaucratic outcome. When the agency has policy preferences, its influence is more apparent—but it is asymmetrical, and it only works to produce smaller levels of enforcement than critics lead us to expect. Specifically, a probusiness agency can move the system toward its own ideal point, increasing its budget and slack in the bargain. A proconsumer (and therefore expansionist) agency,

on the other hand, is helpless to prevent movement to the pluralist equilibrium.

At a more aggregate level, these conclusions provide a different perspective on the growth of government. In the formal modelling literature on this matter, we have seen the blame for big government assigned to different institutions. First, the bureaucracy did it (Niskanen, 1971). Next, Congress did it (Fiorina, 1977). Now, we argue that the routine functioning of a pluralist system is responsible for the size of government. Outcomes are collective by-products of the decentralized choices of several institutions, not the unique responsibility of any single one. And there is no uniform bias toward bigness—government may often be too small rather than too large.

5) The legislature and, indirectly, consumer groups face a fundamental dilemma in the use of control mechanisms. By cutting budgets, the legislature can effectively prevent an agency from producing "too much" enforcement. But it does not work the other way round: a probusiness agency producing "too little" can absorb a larger budget by increasing inefficiency. Moreover, because the budget is an incentive in itself as well as a means of generating output, a policy-indifferent agency may view increased budgets as rewards for inefficiency and low production. If the agency's constituents are not cost-conscious, this view of budgets will prompt the bureau to move toward the degenerate solution—just what consumers seek to avoid. Fortunately, even if constituents ignore taxes, the legislature has a second mechanism—oversight—which plays a crucial role in saving the system from explosive budgets and inefficiency. The wonder of oversight is that it requires so little of the legislature and nonetheless works so well in reversing systemic pathologies.

6) Bureaucratic inertia can be good for the system. Although this seems odd, given the bad reputation inertia has acquired, it actually makes sense: for inertia constrains the flexibility of bureaus in their pursuit of ever-larger budgets and slack, inhibiting their adaptation.

7) The implications of the model pose distinct problems for liberal supporters and conservative opponents of regulation. For liberals, the dilemma arises because the peculiar asymmetry of the budget mechanism operates to their disadvantage. A conservative majority can achieve lower enforcement quite effectively by cutting the budget, but a liberal majority is far less able to engineer higher enforcement by increasing the budget, and its efforts can easily backfire by rewarding agencies for poor performance. Conservative opponents face a different dilemma: a probusiness agency is able to triumph over a proconsumer legislature, but only at the cost of gross inefficiency. They are thus torn between the com-

¹⁰To clarify further, that the social optimum derives from marginal cost-benefit curves whereas the pluralist equilibrium derives from total resource curves tends to make the latter solution exceed the former—yet the greater the relative advantage of business in mobilizing resources, the farther to the left the two resource curves will intersect, and the smaller the pluralist equilibrium will be. Either force may predominate.

peting desires for less regulation and more efficient government.

Conclusion

This model is just a beginning, for we purposely ignore institutional and strategic aspects of policy-making that must eventually be part of a satisfactory theory. The task for the future is to move toward more elaborate models that take these additional aspects systematically into account. In our view, one of the real advantages of the general framework we have developed here is that it easily accommodates efforts to move in a variety of interesting directions, thus encouraging the proliferation of a family of models whose similarities and differences may well provide important new insights into bureaucratic politics. The following are but a few of many elaborations that seem to us both feasible and promising.

Legislative Committees. Legislatures can be assumed to vote only on alternatives generated by committees, which are therefore in a position to use their agenda control to shape outcomes for the legislature and, less directly, for the system as a whole.

Electoral Districts. Legislators can be assumed to be elected from geographically distinct districts, with the distribution of group costs and benefits and the ensuing balance of group resources varying from district to district.

The President. The role of the president can be explored not only by means of the appointment mechanism but also by introducing an OMB with budgetary and monitoring powers and perhaps by incorporating the president's veto power.

Interest Group Strategy. Groups can be allowed to adopt various strategies in their efforts to influence outcomes, for example, contributing only to supporters (not against opponents), basing all contributions on the distance of the legislators from the median, or rewarding opponents if they move in the right direction.

Bureaucratic Innovation. Agencies can be allowed to devote a portion of their budgets to the generation of new programs. These innovations would then alter the production of enforcement or its consequences for business and consumers.³¹

Modes of Adaptation. Decision makers can be assumed to adapt in more sophisticated ways to their environments. Among other things, this may involve probabilistic adaptation or the development and modification of belief structures.

Whatever elaborations may be introduced, they will be unified by a common framework, a macro-

theory of politics based on the microfoundations of bounded rationality. This theory, particularly its formal representations, stands to provide new insights into the nature of bureaucratic politics by placing Simon's "administrative man" (1947) squarely in the context of the larger political system. It also stands to illuminate longstanding issues concerning the intelligence of democracy. For decades political scientists have wondered whether voters are too ill-informed, or politicians insufficiently rational, for the overall health of polyarchies. The classical response of pluralist thinkers to these concerns is that, although we cannot depend upon individual decision makers, we can rely upon properly designed systems that "pit ambition against ambition" and correct for the myopia and ignorance of its constituent elements. Our framework provides a formal means for systematically investigating these issues.

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³¹For work that moves in this direction, see Levinthal and March (1982).

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Vulnerabilities and Responsibilities: An Ethical Defense of the Welfare State

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We all acknowledge strong special responsibilities towards families, friends, clients, and compatriots. The moral basis of these responsibilities is traditionally analyzed in terms of self-assumed obligations. That analysis substantially restricts their scope, because we voluntarily commit ourselves to only a limited range of people. In this article I argue that it is the beneficiary's vulnerability rather than any voluntary commitment as such on the part of the benefactor that generates these special responsibilities. This analysis provides an argument for broader notions of responsibility, because there are many more agents vulnerable to us (individually or collectively) than to whom we have made commitments, in any sense. The welfare state is one particularly apt way of discharging at least some of these further responsibilities. Unlike more individualized responses, the welfare state can satisfy the criteria of a morally acceptable dependency relationship.

The aim of this article is to broaden our sense of social responsibility. It is first and foremost an argument in favor of state welfare services. The basic strategy is to try to put them morally on a par with the services we render—and which we firmly believe we *should* render—to family and friends. The method of discharging these other responsibilities differs but, I will argue, their moral basis is the same. A parallel argument, which here I have space only to sketch baldly, generates analogous international and intergenerational responsibilities and responsibilities for protecting animals and natural environments. Thus this approach promises to provide a unified analysis of a great many of our moral responsibilities.

The argument employs the method of the “reflective equilibrium,” in which the goal is to bring our general moral principles and our “settled

intuitions” about what is right and wrong in any particular case into line with one another, striving all the while for coherence within our set of general principles and for concordance between those principles and our various other “background theories” (Daniels, 1979; Rawls, 1971, sections 4 and 9).

In such a procedure, we must always be prepared to reject some of the moral duties and responsibilities that untutored intuition lays upon us. By the same token, the reflective equilibrium can also carry us well beyond ordinary moral instincts by showing us that we have moral duties and responsibilities that intuitively we might deny having. This is the side of the reflective equilibrium that interests me here.

The moral theory that best explains those intuitions that we do have will, typically, also commit us to moral judgments about what we should do in situations that are strictly analogous but about which we have no strong intuitions. By showing us what lies hidden within our ordinary moral sentiments, the reflective equilibrium can thus drive us to acknowledge commitments we would pre-analytically have shunned.

Special Responsibilities

My argument starts from what I take to be one of our firmest moral intuitions, one that we would be loath to sacrifice in this process of reflective equilibrium. That is the intuition that we have especially strong responsibilities toward our families, friends, clients, and compatriots. Charity, we seem strongly to believe, not only does but *should* begin at home. Our primary responsibilities are toward those who stand in some special relationship to us. Strangers get, and are ordin-

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arily thought to deserve; only what (if anything) is left over.

An example from Bernard Williams effectively evokes this intuition. Suppose you were in a fiery air crash and able to pull out only one other victim before the entire plane exploded. On one side of you is a distinguished surgeon, black bag at hand, who could save the lives of several other passengers thrown clear of the airplane when it went down. On the other side is your son. It would, according to the intuition Williams shares with so many others, be morally reprehensible for you to save the surgeon in preference to your own son.¹

Now, of course, such favoritism would be perfectly understandable from any number of perspectives. Both psychologically and sociologically, it is easy to see why our affections should extend principally to those "of our own kind" rather than to the unknown strangers the surgeon might save. Sociobiologists would point out that saving your son increases the chances of your own genes surviving in the next generation. There is no shortage of adequate explanations for why you would, in fact, favor your own flesh and blood. The question is whether morally you should.

A long tradition in moral theory says firmly you should not. Indeed, any universalistic theory—be it Kantian or utilitarian—would take just that stand. But that, say many philosophers and even more laypersons, is precisely what is wrong with all universalistic ethics. Sir David Ross (1930, p. 21) insists that "the essential defect of the . . . utilitarian theory is that it ignores . . . the highly personal character of duty"; he is outraged that, for example, act-utilitarians are incapable of distinguishing between my giving \$10 to my benefactor and my giving the same sum to someone who needs it just as badly but "to whom I stand in no special relation." In a continuing stream of articles, Williams (1973, p. 110; 1981) has been arguing for the moral importance of respecting people, which entails among other things respecting their "projects," including their special commitments to their families and friends. For Parfit (1984, p. 95), "common-sense morality largely consists" in "special obligations" owed to "the people to whom we stand in certain relations—such as our children, parents, pupils, patients,

members of our own trade union, or those whom we represent." Or, for a final example, a central theme in Walzer's (1983, p. 31) *Spheres of Justice* is that, "The idea of distributive justice presupposes a bounded world, a community within which distributions take place, a group of people committed to dividing, exchanging and sharing, first of all, among themselves."

Our firm moral intuitions go further still. Not only do we feel that we have these special duties to family and friends, alongside our other, more general moral duties. We also feel that "carrying out these obligations has priority over helping strangers," as Parfit (1984, p. 95) goes on to say. Perhaps those special obligations are not quite strong enough to override negative duties not to harm others. It would be thought wrong for us to commit murder, even if that were necessary for us to feed our own children. But apparently those special duties are quite strong enough to override any duties we might have to render positive assistance to others in general. In *Bleak House*, Dickens ridicules Mrs. Jellyby's "telescopic philanthropy," that is, her tendency to aid distant Africans while neglecting her own family. No doubt most of us are intuitively inclined to say that it is right to satisfy our own children before giving food to needy neighbors. The consequence of acting on such intuitions, however, is that our duties to those with whom we enjoy some special relationship ride roughshod over our duties to help others at large. That, obviously, is an outcome that any theory of social justice, or even of simple humanity, would dearly hope to avoid.

In what follows, I shall *not* be arguing that we lack any such responsibilities toward family and friends. Neither shall I be denying the strength of such claims. What I shall be arguing is that there is *nothing special* about those responsibilities. There are many others with precisely the same basis—and, depending on circumstances, perhaps even the same strength—as those responsibilities that we have always regarded as particularly binding. The upshot of this argument, if it is successful, is that we are not justified in our present practice of serving one set of claimants systematically to the exclusion of the other.

Alternative Bases of Special Responsibilities

The theory ordinarily offered to account for the moral importance of these intuitively appealing special responsibilities deals in terms of self-assumed obligations. That analysis figures most famously in H.L.A. Hart's classic essay, "Are There Any Natural Rights?" which first introduced the concepts of special rights and duties into modern moral philosophy. "I think it is true of all special duties," Hart (1955, p. 185) wrote,

¹This is just updating and inverting Godwin's (1793, bk. 2, chap. 2) infamous example of being forced to choose between saving your mother or some greater public benefactor (Archibishop Fenelon) from a house fire. William's air crash example is related by Hare (1981, p. 138), who is highly critical of the conclusions reached by Williams there and elsewhere (Williams, 1981, pp. 17-18). See similarly Gorovitz (1977), Blum (1980), Oldenquist (1982), and Winkler (1982).

"that they arise from previous voluntary acts." Hart's own discussion is characteristically cagey, abounding with qualifications, exceptions, and ambiguities. His general principle, however, has passed into the philosophical conventional wisdom pretty well shorn of equivocation. Thomson (1971, p. 65), for example, asserts quite baldly, "Surely we do not have any . . . 'special responsibility' for a person unless we have assumed it, explicitly or implicitly" (see similarly Rawls, 1958, p. 158).

The great appeal of the model of self-assumed obligations is that it explains so neatly what is "special" about our special rights, duties, obligations, and responsibilities. Special obligations are distinguished from general ones in two ways. First, whereas everyone has the same general duties, special obligations vary from person to person: I have special responsibilities that you do not. Second, whereas general duties are owed to everyone equally, special obligations are owed to specific others: I have different responsibilities to different people. The analysis of special obligations as self-assumed obligations accounts for both features beautifully. According to that analysis, I have some obligations that others do not because I have assumed them and others have not; my obligations vary because I have assumed obligations with respect to some people but not others.

The paradigm case of a special obligation is, for Hart (1955, pp. 183-184) as for all advocates of this model, a promissory or contractual obligation. A promisor, through a voluntary act of will, imposes upon himself an obligation that is peculiarly his own, not shared by the world at large, and his obligation is owed to a specific individual, the promisee, rather than to the world at large. Various other special responsibilities—of businessmen to their customers and employees (Atiyah, 1979), of professionals to their clients (Fried, 1974, p. 78), of all of us to our families (Hart, 1958, p. 104) and friends (Montaigne, 1580/1958, p. 137)—are, within this model, seen as more-or-less attenuated instances of this same basic promissory pattern.

Perhaps the most important practical consequence of analyzing special responsibilities as self-assumed obligations is to reinforce our intuitions about their restricted scope. After all, most of us voluntarily assume responsibility in any way whatsoever for only a very limited number of people. On this account, we have no special responsibilities (but only, at most, much weaker general responsibilities) that have not been voluntarily self-assumed.

The model I want to counterpose to that traditional one traces our special responsibilities to the peculiar vulnerabilities of specific others to our

actions and choices. It is their vulnerability, not our promises or any other voluntary act of will on our part, that imposes upon us special responsibilities with respect to them. The promissory obligations that figure so centrally in the traditional account are, on this analysis, just a special case of vulnerability. If I promised and others are depending on me in consequence, then I am obliged to do as I promised—not because I promised, but merely because they are depending on (i.e., are vulnerable to) me.²

Whereas the paradigm of self-assumed special obligations is the promise or contract, the paradigm for the vulnerability model is family responsibilities. Hart (1955, p. 187) himself admits the difficulty of assimilating these rights and duties to his model of self-assumed obligations. And well he should. You do not enter into families voluntarily. You do not ask to be born; you do not choose your parents or siblings. In the first instance, anyway, you make them no promises and extract none from them.

Even areas of family life that appear on first brush to be contractual turn out not to be completely so. The standard "marriage contract," for example, deviates crucially from the legal ideal of a contract. The authors of the *Second Restatement of the Law of Contracts* are at pains to emphasize that fact: "Although marriage is sometimes loosely referred to as a 'contract,' the marital relationship has not been regarded by the common law as contractual in the usual sense" because "many terms of the relationship are seen as largely fixed by the state and beyond the power of the parties to modify" (Braucher & Farnsworth, 1981, sec. 90; see further Pateman, 1981). Even if marriage partners were to write their own contract, there would be certain duties (such as the duty of support) that would be regarded legally as "essential features" of the marital relationship and which the two parties involved would be legally unable to alter, even by mutual consent. That, of course, violates a fundamental precept of contracting.³

To see how completely ill-suited a model built

²Hence, I basically have no obligation to keep a promise when the promisee is not depending on me in any way to do so. The only reason I then would is that everyone in society depends on the maintenance of the institution of promising, which is vulnerable to being undermined (however slightly) by my breach. That fact explains why the courts award damages, but only nominal ones, in cases where one party suffers no loss from the other's breach of contract. See Braucher and Farnsworth (1981, sec. 346) and, more generally, Atiyah (1979), Gilmore (1974), and Kronman (1981).

³Champions of the voluntaristic model might reply that the parties have at least voluntarily assumed their roles and the concomitant (albeit unalterable) duties.

around contractual or quasi-contractual notions of reciprocity is to the analysis of family responsibilities, consider just two of its more absurd conclusions. First, on that account, a child who lived at home until age 21 would have incurred a debt to his parents 16.6% greater than that of his sister, who left home at 18. But surely the children's obligations to aid their ill and aged mother depend merely on her present needs and their relative capacities to meet them, and not on their past performances. It would be absurd to suggest that the son who remained at home longer should pay 16% more, even though he is now earning 50% less than his sister. (If it really were a case of debt, notice, it would not be absurd: our business obligations are invariant with respect to our capacities to meet them, at least until we declare bankruptcy.) Second, it would be equally absurd to suggest that the debt children owe their parents might at some point be fully repaid, and the children relieved of any further obligation to help parents, however ill they may be. Once contractual debts have been met, any special relationship between creditor and debtor dissolves. The two parties then stand in exactly the same relationship as before the debt was incurred. That is simply not how families or friendships work. A favor and a return-favor do not cancel each other out and dissolve the relationship; instead, they strengthen it (English, 1979, p. 345; Heath, 1976, pp. 59-60).⁴

What most fundamentally underlies the reciprocal duties of family life—of spouses to one another, of parents to their children, of children to their aged parents—is the vulnerability of those parties to one another. The protracted dependency of the human infant is, biologists tell us, one of the most striking features of the species. Much vulnerability is material in nature. Wageless members of the family depend on the wage-earner(s) to bring home the bacon; children depend upon their parents to feed, clothe, and shelter them; in the absence of adequate collective provision, ill or infirm parents are often dependent upon their children for care of a very

material sort.⁵ But we must not focus exclusively upon these material forms of vulnerability. Much of the vulnerability, in family life and friendships more generally, is of an emotional sort. That fact provides a crucial link in the argument explaining why the material support in question has to come from the particular person it does (for example, the parent, child), instead of some unrelated other who might be in an objectively better position to provide it.⁶

One way to demonstrate the superiority of the vulnerability model would be to survey systematically those "special relationships" that we so readily acknowledge. Among the cases that might be discussed, in addition to the family responsibilities just mentioned, are the special responsibilities of businessmen for the safety of their employees and customers, of professionals (especially doctors and lawyers) for their clients, of friends to one another, and of beneficiaries to their benefactors. The aim would be to show that the vulnerability model can offer a plausible account of all features of all these special responsibilities, whereas the model of self-assumed obligations can at best account for only a few features of a few of them. Such a case-by-case demonstration is, however, beyond the bounds of the present essay and is conducted elsewhere (Goodin, 1985a, chap. 4) instead.

The best quick demonstration of the superiority of the vulnerability model is this. Imagine someone who is utterly helpless. The model of self-assumed

⁵I treat these as "social facts" which should, at least provisionally, be taken as given. In these and many other cases, people end up being vulnerable to certain particular individuals merely because society happens to assign those particular people responsibility for caring for them (for example, parents for children). The only reason that this social allocation of responsibility matters morally is, I would argue, because of the vulnerability and dependency that it engenders. If it is regarded as your job, and no one else will do it if you do not (and if furthermore it is a job that really ought to be done by someone), then there is a strong argument for you to do it. That is not to say that it should be your job, or that you (or we) should not try to get someone else to accept responsibility for doing it. It is merely to say that until and unless they do, you should keep on performing it. This is a topic to which I return in discussing collective responsibilities.

⁶Thus, although the courts are committed to doing whatever is in "the best interests of the child," children are not removed from marginally less suitable natural parents whenever marginally more suitable foster parents can be found, and the reason given (the risk of "separation trauma") points, just as my argument here suggests it should, to the emotional vulnerabilities of the child. See Mnookin (1973) and, more generally, Parfit (1984, p. 96) and Blum (1980, p. 56).

Although that may explain why marriage partners have special responsibilities, no such voluntaristic analysis can explain what they are; for that, a separate account must be given. The vulnerability model, in contrast, provides a unified account of both the fact and the content of those duties.

⁴Where families and friendships are concerned, reciprocal favors create bonds transcending the simple sort of trust that underlies long-standing commercial relationships, which amounts to little more than "trust to honor your debts and to keep your word" (Hardin, 1982; Macneil, 1980).

obligations says you have only such responsibilities with respect to that person as you voluntarily assume. You might agree to do all sorts of nice things for that person. It would be terribly kind of you to do so, and of course once you have assumed responsibilities it would be obligatory at that point for you to discharge them. But, within this model, you are initially under absolutely no obligations to agree to do all (or any) of those favors. You are morally at liberty to press your bargaining advantage to the hilt, and to exploit mercilessly the other's weakness. You can, and morally you *may*, force that person to agree to arrangements that are highly inequitable, to say the least. For examples, if any are needed, reflect upon the classic cases of "unconscionable contracts": peddling useless drugs to desperately ill patients at inflated prices, or charging \$1000 for a hamburger when you find yourself enjoying a monopoly on food at some disaster site.

Now, according to the model of self-assumed obligations, parties in strong bargaining positions like these would be violating none of their moral responsibilities by pressing their advantage to the hilt. The vulnerability model, in contrast, regards it as the height of immorality for them to exploit the other's weakness in this way. The same thing that would enable the stronger to drive a hard bargain with (and, within the model of self-assumed obligations, to evade altogether any responsibility for) the weaker would, within the vulnerability model, impose a heavy responsibility upon the stronger to look after the weaker. In such cases, the implications of the two models diverge clearly. And surely our considered moral judgments, backed up by all our "background theories," side with the vulnerability model on this score. As Green (1881, p. 372) remarked, "There is no clearer ordinance of that supreme reason, often dark to us, which governs the course of man's affairs, than that no body of men should . . . be able to strengthen itself at the cost of the other's weakness."

Protecting the Vulnerable

In spelling out the details of this vulnerability model of special responsibilities, I shall confine myself to three general remarks. The first concerns definitions of key terms: vulnerability, dependency, and responsibility. I use "vulnerability" and "dependency" interchangeably to refer to the following situation: *A* is vulnerable to *B* if and only if *B*'s actions and choices have a great impact on *A*'s interests. Here I equate "interests" with "welfare," following Barry (1965, p. 176; see also Reeve & Ware, 1983) in reading "*x* is in *A*'s interests" to mean "*x* increases *A*'s opportunities to

get what *A* wants." Protecting the vulnerable is thus morally desirable because, *ceteris paribus*, it is morally desirable that people's interests and welfare should be furthered.⁷

Central to the concepts of "vulnerability" and "dependency" is the fact that they are relational. You are always vulnerable *to* or dependent *upon* some individual or group of individuals who have it within their power to help or to harm you in some respect(s). This relational character of the concepts is an enormous help in deciding where to settle responsibility for rendering assistance. If vulnerability gives rise to moral claims, then those moral claims must be principally against those agents to whose actions and choices one is vulnerable. Thus, this might be characterized as a "directed needs" model. Saying merely that "*A* is in need" leaves unspecified who should be responsible for meeting those needs. Saying that "*A* is vulnerable to *B*" provides a ready answer to that question.

Vulnerabilities are also relative. *A* would be more vulnerable to *B* than to *C* if *B*'s actions and choices make a greater impact on *A*'s interests than do *C*'s actions and choices. Those to whom one is relatively more vulnerable have relatively greater responsibilities. Anyone to whom *A* is uniquely vulnerable (no one else will help if that person does not) has the greatest responsibilities of all.

Although primary responsibility falls to whomsoever is in the best (or, in the limiting case, the unique) position to protect the vulnerable, that does not relieve others of responsibility altogether. Those who could help, albeit not as well as those with primary responsibility, retain a residual responsibility to do so should the others default, and they also have a continuing responsibility to monitor the situation to see whether or not their

⁷My principle is more catholic than it might appear. Although the definition of "interests" is recognizably liberal, the slippage between what people want and what is objectively best suited to satisfying their desires allows substantial scope for Marxian notions of false consciousness to operate. Similarly, although my principle is welfare-consequentialistic in form, deontologists can embrace it just so long as the *ceteris paribus* clause is understood to include their cherished rights-based constraints on action. Notice also that my principle treats acts and omissions symmetrically. Glover (1977, p. 94), Goodin (1982b, pp. 14-15), and Bennett (1983) show that, once other morally relevant correlates are factored out, not helping when you could have done so is morally indistinguishable from causing harm when you could have avoided doing so. That argument applies with particular force to bearers of standard special responsibilities (e.g., bodyguards and firemen), and would by extension apply equally forcefully to all vulnerability-based responsibilities that I have here argued are analogous.

assistance is in fact required (Feinberg, 1970, p. 244).⁸

Let us consider next the definition of responsibility. There are various forms, among them: causal responsibility (you produced this result); moral responsibility (you are to blame for it); and task responsibility (it's your job). Often we slide carelessly from one sense to another. To employ a pun that has frequently been mistaken for an argument, it is commonly said that those who are responsible should be made responsible. That is to say, (task) responsibility should be settled on those who are (causally) responsible. If *A*'s actions have caused some unfortunate situation, then according to this rule it should be *A*'s job to correct it. Most especially, if *A* got himself into a jam, it should be *A*'s responsibility to get himself out of it.

My argument is that task responsibility and causal responsibility are separable, logically, and should often be separated in practice. On my analysis, how *A* got into his present state is irrelevant. All that matters now is who is best able to get him out.⁹ Sometimes—maybe even typically, for all I know—the person causally responsible for producing a state of affairs will be best able to reverse it. If so, then the pun will point us in the right direction. But in a great many cases, the agent best able to get a person out of a jam might well be someone other than the agent who got that person into it. Then, I would argue, task responsibility should be settled upon the former, even though that agent is in no way causally responsible. When the wake from a passing speedboat capsizes my sailboat, it must be the responsibility of other nearby sailors rather than the long-gone speeder to pull me out of the water.

That rule might, admittedly, tend to encourage improvident behavior. People could always rely on others to clean up their messes or get them out of jams they got themselves into. But providing appropriate counters to those perverse incentives is a practical rather than a moral matter. Incen-

tives, as Rawls (1971, sec. 48) and Feinberg (1970, p. 94) rightly emphasize, are nothing more than socially necessary bribes; they do not necessarily have anything at all to do with moral deserts. On this nonmoralized view of incentives, encouraging people to get others out of bad situations is, in principle, just as important as discouraging those people from getting themselves into those situations in the first place. Where the emphasis should fall, in any particular instance, is simply a technical optimization exercise: we should just choose the most cost-effective method, or combination of methods, for avoiding suffering all around. This typically leads us to assign responsibility largely (and, on some analyses, almost exclusively) to people who were in no way causally responsible for the situations they are now being asked to remedy.¹⁰ Furthermore, there are in any case clear limits on the use of incentives. They must be effective in accomplishing their desired goal; they must be no larger than strictly necessary to do so; and they must be no more costly than the goal itself is worth. All this combines to suggest that incentive considerations will not lead us to deviate often or largely from the course of action recommended by my principle of protecting the vulnerable.

The separate question of moral deserts remains.¹¹ It presents itself as a counterargument to my vulnerability model in two respects. One argument concerns those who find themselves in

⁸The incentive problem posed here is discussed more fully by economists concerned with allocating the costs of accidents between those whose negligence created a dangerous situation, on the one hand, and those with the "last clear chance" to avoid the accident, on the other. Wittman (1981) has shown that the optimal incentives there require us to impose the bulk of the charges on the latter party rather than the former one. This is consistent with my suggestion that task responsibility should be predominantly forward-looking (who can remedy the situation?) rather than backward-looking (who caused the situation?).

¹¹As a kind of hybrid between the incentive and desert arguments, there is a third proposition, which holds that it is our moral responsibility to protect the long-term interests of the profligate poor (or their or our successors, who would otherwise have to pay their bills) by creating appropriate incentives in the present to encourage them to do what they (or their or our successors) will at some future time wish they had done. But the same limits apply to this argument as the earlier incentive argument: it justifies incentives only insofar as they are necessary, effective, and entail less overall harm than benefit to the interests they are designed to protect. Thus, if the arguments of footnote 19 below are correct, the issue will simply not arise. Even if it does, this duty to be "cruel to be kind in the long term" might be counterbalanced by our duty not to be cruel in the short term.

⁹Notice an important asymmetry. When those with primary responsibility have acted, those with subsidiary responsibilities are relieved of any obligation to do so. But when those with subsidiary responsibility have, owing to the inability or especially the unwillingness of those with primary responsibility, been forced to act in their stead, that in no way relieves the latter of their obligations or excuses their defaulting.

¹⁰This saves my principle from the counterintuitive consequence that we have stronger responsibilities for protecting the rich than the poor, because, *ceteris paribus*, people are more vulnerable, the more they have to lose. But of course the more you have to lose, the better able you are to protect yourself from loss: the rich can protect themselves; it is the poor who need our help.

difficulty through their own improvidence: some would say that they had it coming, that is, they deserve whatever they got and it would actually be *wrong* for anyone to help them out of their difficulty. The other argument concerns those who have done nothing to cause another's distress but are (perhaps uniquely) in a position to help relieve it: some would insist that they have done nothing to deserve this burden, and that it would be wrong to impose it on them although of course it would be terribly nice of them if they chose to assume it.¹²

Both claims, however, extend the notion of "deserts" beyond its proper bounds. Take the second counterargument first. Incurring a duty or a responsibility is not necessarily, nor even characteristically, something that you deserve. More often than not, it is something that just happens to you. When walking along a deserted beach, you see a child floundering in shallow water. Nothing in your background or character suggests that you deserve to bear the burden of effecting the rescue. It would, however, be absurd to say that you are therefore morally at liberty to leave the child to drown.

The notion of "deserts" is ordinarily equally out of place in the first counterargument as well. By and large, the improvident should be regarded as foolhardy people whose recklessness is to be discouraged through appropriate disincentives, not as evil people whose wickedness deserves to be punished. In dealing with people in precarious situations, questions of fault, blame, and desert are simply out of place. When two victims of a traffic accident are brought into the hospital emergency room with similar injuries, surely they should be treated with equal care and attention, even though one caused the accident while the other was its innocent victim. The need, dependency, or vulnerability of the victims, not their deserts, is what dictates physicians' responsibilities (Goodin, in press).

The third and final general point about my vulnerability model is that the "agents" to whom these responsibilities fall might be either in-

dividuals or collectivities. Consider the problem of famine relief. Millions of people are starving daily. But realistically what could I do to help? There are millions of them and only one of me. In such cases it may make little sense to hold anyone individually responsible; maybe, in my terms, those in need are just not very vulnerable to any single other individual.

Such invulnerability would, on my argument, relieve us individually of any responsibility for helping with famine relief efforts. Excuses of that sort, however, typically serve merely to get us out of the fire and into the frying pan (Austin, 1956). The price of letting individuals off the hook in that way is to put collectivities there in their place. The starving people of the world certainly *are* vulnerable to the actions and choices of those of us in rich nations, taken collectively. Hence we collectively have heavy responsibilities in respect of them.

Collective responsibilities are laid, in the first instance, upon the group as a whole to organize and to implement a scheme of coordinated action to protect the vulnerable agent or agents' interests. But these group responsibilities ultimately devolve onto individuals constituting the group. Each member of the group is responsible, so far as that person is able consistently with that person's other moral responsibilities: 1) to see to it that the group organizes a scheme of coordinated action that protects the vulnerable agent or agents' interests as well as it can; and 2) to discharge fully and effectively the responsibilities allocated to him or her under any such scheme that might be organized, just so long as it protects the vulnerable agent or agents' interests better than no scheme at all would protect them.¹³

In many ways, making something a collective responsibility only complicates things. In one important respect, however, it simplifies them. Making something a collective rather than an individual responsibility removes any objection to the social enforcement of that responsibility. Individuals have many moral duties which, for one reason or another, we think society ought not compel them to discharge. Where the duty is a collective one from the outset, however, collective action is clearly appropriate. Then the collectivity

¹²The most famous counterexample of this sort is Thomson's (1971, p. 48) of a dying violinist, who can survive only if plugged into your circulatory system. My principle does indeed require you to give the violinist's interests serious consideration; but they may of course be outweighed by the other responsibilities, including responsibilities to yourself, on which you would have to default to save the violinist's life. In this way my principle can wriggle out of Thomson's counterexample. Here it is to the credit of my theory that it requires some squirming: they simply answer, "It's my body, and that's that," surely makes far too short work of what truly is a terrible moral dilemma.

¹³Philosophers (McKinsey, 1981) tend to recognize the latter duty but not the former; for more details see Goodin (1985a, chap. 5). There is obviously some scope for trading off these two duties for one another, with political action designed to alter the scheme being preferred to cooperation under the scheme whenever it is likely to be sufficiently effective in instituting a scheme that is sufficiently better at promoting the vulnerable agent or agents' interests sufficiently quickly to justify the short-term damage to those interests.

is fully justified in compelling each of its constituent parts to play its full, appointed role in discharging those obligations, duties, and responsibilities.

Broader Implications

The aim of this essay is to demonstrate that we have a broader range of social responsibilities than we traditionally acknowledge. What I hope to establish here is essentially a point of principle. The practicalities—how best to discharge these broader responsibilities—largely remain as open questions, although at the end of this essay I shall offer reasons for believing that the welfare state is a good way of discharging at least some of them.

The argument for broader responsibilities goes like this. We all acknowledge strong responsibilities toward our own families, friends, and certain others. Once we start examining the sources of those responsibilities, we discover there is nothing "special" about them. It is the vulnerability of the others, rather than any voluntary act of will on our part, that generates those responsibilities. There are many more people vulnerable to us, individually or especially collectively, than stand in any of the standard "special relationships" to us. If my analysis of the true basis of those standard responsibilities is correct, then we have strictly analogous (and, potentially, equally strong) responsibilities toward all those others as well. Aid to vulnerable strangers is thereby justified on the same basis as aid rendered to our own parents or children.

To see what all is at stake in this argument, its ramifications must be sketched, however briefly and unsatisfactorily. First and foremost, notice that state welfare services are essentially mechanisms whereby we attempt collectively to protect vulnerable members of our own society. They may or may not be the best mechanisms available for so doing, and I shall return to that issue presently. But for now, just note that the welfare state, as a device for protecting the vulnerable, can claim at least *prima facie* moral justifiability on my principle.

The vulnerability model, in its collective form, might also underwrite far stronger international obligations than are ordinarily acknowledged (Singer, 1972). People in very poor countries are enormously vulnerable to the actions and choices of people in very rich ones. Again, how we might best go about discharging these responsibilities is an open question and turns heavily upon various matters of empirical fact. It seems very likely, however, that the responsibility imposed on the

rich will turn out to be a collective rather than an individual one.¹⁴

Yet another potential application concerns our obligations to future generations. Intergenerational transfers, especially spanning several generations, are singularly one-way affairs. We can do whatever we like to our distant successors without fear of retaliation. Future generations are enormously vulnerable to the actions and choices of present people, both individually and especially collectively. The things we destroy or use up will be unavailable for them to enjoy. Their extraordinary vulnerability to our actions and choices seems to be the best basis for assigning strong responsibilities to present generations to provide for the further future.

Finally, this model might entail responsibilities to protect animals and natural environments. "Dumb animals," delicate coral reefs, and ecosystems more generally have proven enormously vulnerable to the actions and choices of human agents, both individually and especially collectively. Whether or not that gives rise to any responsibilities on our part to protect them depends on whether this constitutes "vulnerability" in the relevant sense. Earlier, "vulnerability" was defined in terms of the impact of our actions and choices on the *interests* of those who are affected. The question thus becomes one of whether animals and other nonhuman entities can have genuine interests. This is not the place to enter into that debate.¹⁵ It is enough to say that if (and insofar as) we see sentient creatures, ecosystems, or the natural environments upon which they depend as having "interests" at all, then their vulnerabil-

¹⁴Glover (1975) points out that, although none of us acting alone could eliminate world poverty, virtually any of us acting entirely alone could lift at least several starving Asians out of poverty. The essential flaw with individualistic programs such as "adopt-a-child," though, is that they work on a child-by-child basis; consequently they can end up doing as much harm as good, both for the putative beneficiaries and for those around them. To be sure our well-intentioned gifts have the desired effects, they must be set in the context of some reasonably comprehensive and well-integrated scheme for restructuring the recipient community as a whole. That requires a kind of coordination well beyond anything isolated donors—or even relatively large-scale voluntary charities—could reasonably hope to accomplish. Nozick (1974, pp. 266-267) makes just such a case for organized collective action.

¹⁵Feinberg (1980, pp. 159-206) persuasively argues that animals, at least, do have interests and hence interest-based claims; and since Routley and Routley (1979) are right to insist that the burden of proof must fall upon anyone who proposes to treat different agents' interests differently, animals' interests must at least presumptively enjoy the same status as our own.

ity will once again generate *prime facie* responsibilities on our part to protect them.

Of course, we will have to balance off one set of responsibilities against others. How much should we allocate to our collective responsibilities for social welfare, as against our private responsibilities for the well-being of our families and friends? How much to foreign assistance, as against domestic social services? How much to future generations, as against present people? How much to the protection of animals and environments, as against humans?

Some might regard these responsibilities as utterly incommensurable and therefore deny that any such balancing can ever be done. Recall Sartre's example of "the young man who is torn between remaining with his ailing mother and going off to join the Resistance. Sartre's point is that there is no way of adjudicating between these two strong claims on his moral allegiance through reason. . . ." But, as Taylor (1976, pp. 290-291) goes on to say, if there really were no bases for comparing the claims of kin and country, then the moral dilemma itself would disappear: the son should just "throw himself" one way or the other, as Buridan's ass in choosing between two equally good haystacks; since neither choice would be right or wrong, there would be no point agonizing over the choice. But that cannot be a correct analysis. We may find it hard to say which is the right choice, but we are quite confident that, whatever else, it is rightly to be regarded as a hard choice. That fact is conclusive evidence that the responsibilities cannot really be incommensurable, after all.

Here I suggest that the notion of "vulnerability," cashed out in terms of the notion of "interests," can not only account for the force of these dilemmas but can also help us resolve them. The dilemmas arise because discharging our responsibilities with respect to some of those who are vulnerable to us entails defaulting on our responsibilities with respect to some others who are also similarly vulnerable to us. Which we should favor depends, I suggest, upon the relative vulnerability of each agent. We must determine 1) how strongly that agent's interests would be affected by our alternative actions and choices, and 2) whether or not that agent would be able to find other sources of assistance or protection should we fail to provide it.¹⁶

¹⁶We also need to decide how to weight one agent's interests relative to another's. Where human beings are concerned, I am firmly inclined to weight everyone's interests equally (Goodin, 1975). I am just as firmly inclined to weight future generations' interests the same as our own (Goodin, 1982a). But where animals or in-

On balance, it seems likely that those relatively near to us in space and in time probably will be rather more vulnerable to us in these ways. Hence my argument is saved from the traditional *reductio* of requiring us to give everything we have to starving Asians, or always to be saving everything for the ever-receding infinity of future generations, or to have our own lives and projects constantly interrupted to serve others. My argument would seem to allow—indeed, to require—us to show *some* bias toward our own kind, however defined. But that bias must not be absolute. The vulnerability of others—be they needy fellow-citizens, foreigners, heirs or hares—to our actions and choices may well be sufficiently large, and how we (and perhaps we alone) can help sufficiently clear, to require us to give their interests some substantial weight in reckoning our own responsibilities. Charity may indeed begin at home, but morally it must not stop there.

Reducing Immoral Vulnerabilities

Up to this point, my argument has taken vulnerabilities and dependencies as given. I have argued that, if people are vulnerable to us, then it is our responsibility to protect them. Sometimes, however, it might be even better to try to eliminate the conditions making them vulnerable and dependent. A few of these conditions may be naturally fixed, inevitable and immutable. But most vulnerabilities will turn out to have been created, shaped, or sustained at least in part through certain social arrangements (Titmuss, 1958). In some respects, this is true of even those dependencies that would seem at first blush to be the most natural, such as those connected with childhood (Marks, 1975; Skolnick, 1975) or old age (Walker, 1980; 1982).

It is obviously immoral to hold people in an unnecessarily dependent status against their will. Moral theories built around notions of free choice, self-respect, or autonomy all yield that same conclusion. But in some cases people willingly do—and it seems morally and prudentially desirable that they should—render themselves vulnerable to a variety of other people for a variety of purposes. In personal relationships, love can plausibly be analyzed as putting yourself in one another's power. "A world in which no one had power over another, and in which no one was vulnerable, a world in which people could be moved to action by force or reason alone, would be a world without love, an inhuman world"

animate objects are concerned, I am far less certain what the appropriate weighting should be.

(Wilson, 1978, p. 315). In commercial relations, interdependence is the essence of trade and hence of all mutually profitable business ventures. Similarly in politics, interdependence forms the basis of all kinds of alliances and coalitions (Baldwin, 1980). Objectionable though we might find the particulars of any specific arrangement, there is surely nothing wrong with exchanges or alliances as such merely because they engender vulnerabilities of this sort.

Our moral theories must, then, allow for vulnerabilities, some of which are inevitable and some of which are self-selected. Yet at the same time we must recognize that such relationships are subject to abuse, and we must insist upon certain conditions to guard against that kind of abuse. The standard proposal is that, insofar as vulnerabilities or dependencies are within our power to create or alter or eliminate, they should be reciprocal or mutual. The real objection, various commentators observe, is not to dependency as such. It is instead to *unilateral* dependency, to *asymmetrical* power relations. That asymmetry is what creates opportunities for the strong to exploit the weak. Were each party equally dependent upon the other, there need be no fear of such behavior (Wilson, 1978, p. 315; see similarly Held, 1974; Pateman, 1981; Thompson, 1825; and Wollstonecraft, 1792).¹⁷

It is, however, wildly unrealistic to demand that dependencies or vulnerabilities be completely symmetrical, that is, that each depend on the other in exactly the same measure. That condition is satisfied in virtually no trading relation, in virtually no alliance, and probably in very few personal relationships. Neither is complete symmetry strictly necessary for a morally acceptable dependency relationship. There are, in fact, two acceptable alternatives.

The first is described by a pair of conditions. One is interdependency: each party must get something out of the relationship; or, alternatively, each must stand to lose something if it were terminated. The other condition is that neither party is so heavily dependent upon the other that the dependent party would cease to meet basic needs were the relationship terminated (Baldwin, 1980; Shue, 1980). The latter condition could be waived in cases of perfectly symmetrical vulnerabilities but becomes increasingly important the greater the asymmetry.¹⁸

¹⁷"Symmetry" must be subjectively defined: what one party gains from the relationship (or would lose were it terminated) must occupy as important a role in that party's overall life plan as what the other party gains or would lose plays in that other's.

¹⁸Symmetrical dependencies, even very great ones, admit of no abuse. If each party depends equally upon the

The second alternative would permit monopoly suppliers of needed resources to exist. What it would crucially demand is merely that they have no discretionary control over the disposition of those resources. *B*, who needs the resource, would still be dependent upon *A*, the monopoly supplier of that resource. But *A* cannot *exploit B's* dependency unless *A* can withhold the resource at will. And it is exploitable dependencies which we find morally most offensive.

An Application: The Welfare State

The *raison d'être* of the welfare state clearly is to discharge in part the responsibility I have here been discussing, namely, to protect vulnerable members of society. The early British initiatives—old-age pensions, health and unemployment insurance, minimum wage policies, and the like—were all aimed explicitly at "protecting the weakest and most vulnerable elements in society, such as the aged poor, the unemployed workman, the sweated worker" (Robson, 1976, p. 21). The subsequent history of the welfare state has similarly consisted in the increasing recognition of and response to "states of dependency" (Titmuss, 1958, pp. 42-43). Contemporary arguments for the expansion of the welfare state are all likewise couched in terms of dependency and vulnerability (van Lennep, 1981, p. 9; Australian Royal Commission on Human Relationships, 1977, vol. 1, p. 63).

Assuming my earlier arguments have proven persuasive, it is clear that we have a strong moral responsibility to protect those who are vulnerable to our actions and choices. The only remaining question is whether the welfare state is the best way of doing so. Of course, any particular welfare state may fail for reasons peculiar unto itself. It is important, therefore, to separate out and concentrate on what is generic to the welfare state as such.

For purposes of this discussion, I shall assume the defining features of the welfare state—and the features that must therefore be justified in any defense of it—are as follows. The welfare state necessarily entails *compulsory, collective provision for certain basic needs as a matter of right*. Welfare state provision is said to be compulsory and collective to distinguish it from private, voluntary charity; as distinct from more thoroughgoing egalitarian regimes, it provides only for people's basic needs; and as distinct from

other for the satisfaction of basic needs, then neither could exploit the other through a credible threat to terminate the relationship.

the old Poor Law, the welfare state allows beneficiaries to claim their entitlements as a matter of right.

The principal challenge facing any defense of the welfare state must surely be to show why protecting these vulnerable people should be a collective rather than an individual responsibility. There are basically two individualistic options available here. The first focuses upon the responsibilities of needy individuals themselves, while the second focuses upon the responsibilities of their families and friends.

Certainly it is true that people are even more vulnerable in even more ways to their own actions and choices than they are to those of the collectivity. For that reason, they should indeed bear primary responsibility for attending to their own needs. The state's responsibility is in the nature of a secondary, back-up responsibility, activated only when those with primary responsibility have failed to discharge it. Why they have failed, I have argued, is morally irrelevant: my analysis of responsibilities is, recall, forward-looking (asking only "Who is best able to help now?") rather than backward-looking (asking "Who is to blame for this situation?").¹⁹ All that matters is that these individuals are now at risk of failing to have their most basic needs satisfied. That in itself is quite enough to activate secondary responsibility on the part of others to protect them.

The second individualistic option is to admit any need for someone to bear second responsibilities in such a case, but to lodge those responsibilities with some other private individuals rather than with the state. Specifically, this approach suggests that if people are unable to provide for themselves, it should be left to their families and friends to do so. That, curiously, is what "self-reliance" has come to mean in contemporary social policy debates (Goodin, 1985b). No doubt families and friends do, on my account, have

strong moral responsibilities to provide assistance in such situations, and at its best, this system is no doubt preferable to state provision: material assistance from families and friends might betoken a deeper affection, in a way that state assistance necessarily cannot. The question is not whether families and friends should help out those in need. It is instead whether those in need should be forced to rely upon their families and friends for such support.

At this point, there is a strong case to be made for welfare-state provision as a kind of back-up to the back-up. An important part of this case is brutally empirical. One of the primary obstacles to an effective system of family responsibility for needy members is that needy people are ordinarily found in needy families, who are in no position to make any great sacrifices (Goodin, 1985b; Steiner, 1981, p. 114). There is also a principled objection to such policies, however, which would prove decisive even if these empirical facts were otherwise.

This principled objection harks back to the standards of an acceptable dependency relationship laid down above. Dependency might be acceptable if: 1) it were symmetrical; or 2) basic needs were not at stake; or 3) no one were to enjoy discretionary control over needed resources. *Ex hypothesi*, we are here discussing an asymmetrical relationship where the subordinate's basic needs are indeed at stake.

The only thing that could make such a relationship morally acceptable would be for the agent upon whom the subordinate depends not to have discretionary control over those needed resources. Where that agent is a person's family or friends, that condition cannot possibly be met: family and friends must inevitably enjoy discretionary control over resources that are, in the final analysis, still their own. Where the subordinate is dependent instead upon the state, however, it is possible to vest that person with a legal entitlement to assistance. Once such laws have been enacted, state officials (unlike families, friends, or private charities) lack any discretion in deciding whether or not to honor claims of needy petitioners (Goodin, 1985b; Smith, 1949, 1955; Titmuss, 1971).²⁰ The welfare state, defined as an institution which meets people's basic needs as a matter of right, is therefore a morally necessary adjunct

¹⁹Backward-looking considerations are relevant only insofar as, through incentive effects, they might have future consequences. But there is a substantial body of evidence suggesting that the poor tend to be poor planners and hence are largely impervious to incentives whether these are positive (e.g., incentives to slack off work when guaranteed a minimal income) or negative (e.g., incentives to avoid hazardous jobs); see Danziger, Haveman, and Plotnick (1981), Viscusi (1979), and Goodin (1985b). It is perfectly reasonable that the poor should be unresponsive to incentives: marginalist calculations are a costly business, and the poor have more pressing demands on their time and attention. Given that, however, restricting welfare benefits would probably amount to no more than bringing a gratuitous disincentive to bear on a group that is largely impervious to incentives of any kind.

²⁰Discretion disappears only after such laws have been enacted; state officials are left with discretionary control over what laws they enact. But it is the exploitation of dependencies that worries us, and it is in the administration rather than in the enactment of welfare policies that the greatest danger (although certainly not the exclusive one) of such exploitation arises.

to other more individualistic responses to the problems of vulnerability and dependency in our larger community.

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The Responsible Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974

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Most analyses of the 1974 congressional elections have failed to find significant effects for either Watergate or personal financial conditions, despite the prominence of both of these issues in the campaign. An alternative thesis argues that the effect was indirect, through the selection of better-than-usual Democratic candidates and weaker Republican contestants for House seats. Reanalyzing campaign finance data, we challenge this thesis and then move on to a different type of analysis from that which traditionally has been done in retrospective voting studies. With the use of the 1972-1974 panel of the Center for Political Studies, we examine separately the voting behavior of what V. O. Key, Jr. called "standpaters" and "switchers." The former are motivated primarily by party identification, with small Watergate effects. Our probit analysis for switchers, on the other hand, finds much weaker party identification effects, but, interestingly, much more pronounced Watergate and economic impacts. Furthermore, an analysis of the sample compared to the population of districts in 1974 suggests that a more representative sample would lead to even more pronounced impacts for Watergate and the economy than even we have found.

Richard M. Nixon resigned the presidency on August 9, 1974 and was succeeded by the nation's first unelected chief executive, Gerald R. Ford, who then pardoned Nixon for his role in the Watergate scandal. After the oil price shock that accompanied the Yom Kippur War in the Middle East, inflation hovered close to 11% while unemployment rose to 5%. Americans were clearly up in arms over the state of the economy: 50.2% of the respondents to the 1974 Center for Political Studies election survey cited inflation as the most important national economic problem, a 2.5-fold increase over just two years earlier (Kiewiet, 1983,

p. 84). At no time since these questions were first asked in 1958 had more than 20% of those surveyed been so concerned about inflation. Furthermore, 30.2% saw inflation as their most important personal economic problem in 1974, and an additional 7.1% referred to "declining real income." These two figures, when added together, represent a virtual doubling of such concerns from 1972 (cf. Kiewiet, 1983, p. 55). Indeed, the decline in real disposable per-capita income in 1974 (-2.3%) was the largest for any off-year since 1946 (Tufte, 1978, p. 25), when Democrats lost 56 House and 13 Senate seats and ceded control of the Congress to the Republicans.

It is hardly surprising, then, that the Democrats gained 49 House and four Senate seats in the off-year contests. Not since 1958, in the midst of a deep recession, had the president's party lost so many seats in the House; indeed, one has to go back to 1948, when the Democrats recaptured the Congress, to find greater Democratic gains than in 1974. The *New York Times* (1974) referred to the political terrain within its largest circulation area (New York, New Jersey, and Connecticut) as "wall-to-wall Democrats."

It would seem, then, that the 1974 midterm election would be easy to explain. Armed with the combined conventional wisdom of journalists and politicians, students of voting behavior also sought to demonstrate the effects of Watergate and the economy on the Republican debacle. But, with a single exception, these studies found either no effects or impacts so minimal that they were overwhelmed by traditional variables such as

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party identification. One study tried to put the intellectual carnage that the various survey-based analyses had wrought in some sort of order by proposing a novel thesis: It was the candidates, not the voters, who reacted strategically to the Watergate and economy syndrome (Jacobson & Kernell, 1981). We examine the claims of the journalists and politicians and assess the findings of the studies of the 1974 election. After critically analyzing the "strategic politicians" thesis, we develop a reconceptualization of the retrospective voting argument and then show that there were, after all, Watergate and economic effects in 1974.¹ Our approach is to study not only the vote itself, but also vote changes from 1972 to 1974, using the Center for Political Studies panel survey. Our findings should be reassuring because they no longer lead to a schism between voting studies and conventional wisdom: 1974 was marked by retrospective voting.

Watergate and the Economy

The conventional wisdom about the 1974 elections was summarized well by *Congressional Quarterly Weekly Report* (1974b, p. 3122) in its election post-mortem:

Old-fashioned economic issues, centering around inflation and unemployment, emerged as the major topic of debate. Close behind was another warhorse of political rhetoric—corruption, focusing on the Watergate scandals, President Ford's pardon of former President Nixon and alleged violations of campaign contribution and spending laws.

The punishment of Republicans was selective: "Nearly every House Republican beaten . . . was counted among the conservatives; the liberal and moderate Republicans generally had little trouble winning reelection" (*Congressional Quarterly*, 1974a, p. 3065). Republican members of the House Judiciary Committee who voted against impeaching Nixon were significantly more likely to be defeated in 1974 than those who voted for impeachment, even controlling for the members' ideology (Wright, 1977). Republican Senator Charles Percy (IL), who was not up for reelection in 1974, and Democrat Wendell Ford (KY), who

defeated Marlow Cook in that contest, both attributed GOP losses to the twin demons of Watergate and inflation. Republican National Committee chairman Mary Louise Smith also stated that "Watergate and the economy have combined to influence voters" (Cannon, 1974).

It thus appears that no great new theoretical insight is required to explain the outcome of the 1974 Congressional elections. Yet most of the attempts to assess the effects of Watergate and inflation have rung up "no sale." We are thus faced with a puzzle: How do we reconcile conventional wisdom with survey research?

Most systematic analyses of the 1974 midterm election, both those employing survey research and those using aggregate data, have produced no support for this scenario. Two analyses using the 1974 CPS surveys found virtually no Watergate effects and only small amounts of economic retrospective voting (Conway & Wyckoff, 1980; Miller & Glass, 1977). A Wisconsin study (McLeod *et al.*, 1977) also detected no Watergate effects. Kinder and Kiewiet (1979) found that personal economic grievances had little impact on vote choice in 1974, because most people attribute such complaints to their personal circumstances and not to governmental action. National business conditions, a form of collective judgment, also failed to yield significant coefficients. Only collective judgments of party competence, as Fiorina (1981) found, had a measurable impact on voters' decisions. Kiewiet's (1983, p. 105) more elaborate model of economic retrospective voting failed to yield any significant coefficients for either personal or national economic assessments for 1974; indeed, the coefficient for concern about inflation was precisely .001. Owens and Olson (1980), using aggregate data on vote shares for 1974 for 138 districts for which party registration figures could be obtained, found very little support for the supposed effects of either inflation or change in real per-capita income for the vote shares (or changes in vote shares) for incumbents of the president's party.

One might be tempted to respond that the most sophisticated study of retrospective voting in 1974 did find significant and large effects for both disapproval of the Nixon pardon and expectations about which party would best handle future economic problems in the congressional voting decisions for electors (Fiorina, 1981, pp. 164-167). Furthermore, attitudes toward the pardon clearly affected the voters' choice of party identifications in 1974, although alternative specifications yielded different conclusions about whether personal economic conditions or collective judgments on the state of the economy had a measurable impact on party identification choice that year (Fiorina, 1981, pp. 98-102).

¹To those who might doubt the roles of the economy and Watergate in the minds of the voters, it seems appropriate to recall the remarks of Rep. William Hungate (D, MO) of the Judiciary Committee during the impeachment hearings addressed to those who did not admit Nixon's implication in the scandal: "Some will say that it's not an elephant. It's a mouse with a glandular condition."

Strategic Voters or Strategic Politicians?

One might simply choose up sides and prefer Fiorina's specification. This would hardly be unreasonable.² However, Jacobson and Kernell (1981) have offered a "revised theory" of the effects of Watergate and the economy. Their thesis is that when a party's electoral fortunes appear to be bleak, it will have difficulty attracting good candidates to run for office. On the other hand, in anticipation of an electoral sweep, the opposition will be able to attract challengers with previous elective office to run against the presidential party's incumbents. In turn, these candidates will be able to attract more campaign contributions. Because it is the challenger's spending that makes the difference in the outcomes of congressional races and not the incumbent's, the combination of a well-funded campaign and highly visible challengers makes the incumbent's task in winning reelection much more difficult. The chief advantage that House incumbents have over their Senate counterparts is that they are so much better known than their challengers (Hinckley, 1980; Mann & Wolfinger, 1980). Jacobson and Kernell (1981, p. 31) demonstrate that the average vote received by both Democratic and Republican challengers without prior elective office has been from 4 to 11% lower than those who have held office from 1972 to 1978. Furthermore (p. 32), the percentage of Democratic challengers with experience for both Republican-incumbent and open seats in 1974 was considerably greater than it was for any other recent election, whereas the share of GOP challengers with prior office was markedly lower in 1974. Additionally, Jacobson and Kernell argue:

In the normal years, Democratic incumbents spent between 57 and 63 percent more than Democratic challengers; in 1974, they spent 22 percent less. Republican incumbents spent between 62 and 90 percent more than Republican challengers in the normal years, 293 percent

²There remains another possibility. Kramer (1983) presents an elegant argument that individual-level models of retrospective voting are misspecified and seeks to reconcile them with aggregate analyses. The latter studies consistently show much stronger retrospective voting patterns than do the microlevel ones, of which the 1974 results are but a special case. Kramer reviews many, but not all, of the conflicting findings. Much of Kramer's argument focuses on the difference between cross-sectional analysis of survey data and the time-series component of aggregate models. Our emphasis on vote change rather than the simple vote is consistent with Kramer's thesis, even if we do not use his elaborate model. (We are not aware of any attempt to apply his complex estimation procedure.)

more in 1974. . . . Campaign contributions—and therefore expenditures—were sharply responsive to perceived political trends in 1974. (1981, pp. 41-42)

A simple five-variable model including the party of the challenger, challenger expenditures, incumbent expenditures, the prior strength of the challenger's party in the district, and whether the challenger has held prior elective office accounts for 66% of the variance in aggregate vote shares in 320 congressional districts in 1974 (Jacobson & Kernell, 1981, p. 46). This thesis provides an alternative account of what went wrong for the GOP in 1974. It seems commonsensical, elegantly simple, and well supported.

However, a somewhat closer look at the evidence suggests that this explanation cannot fully account for the Republican debacle in 1974. First, the supposedly most important variable in the model, whether the challenger has held elective office, has the lowest standardized coefficient (.07) of the five variables. The variable with the highest *t*-ratio is whether the challenger is a Democrat. One does not need a very elaborate theory of voter responses to external conditions to predict that Democratic challengers would do well in 1974. Second, the campaign expenditure data can be given more than one interpretation. The comparisons made by Jacobson and Kernell and cited above are between Democratic incumbents and Democratic challengers, on the one hand, and GOP incumbents and hopefuls on the other. This assumes that only challengers' expenditures matter.³ However, the regression presented by Jacobson and Kernell shows that challengers' vote shares are significantly related to both their own expenditures and those of the incumbents they run against. Furthermore, both Jacobson (1978, pp. 114-134) and Ragsdale and Cook (1984) demonstrate that incumbents react to challengers' fundraising by seeking more of their own resources. Whereas Jacobson argues that challengers simply raise all the money they can and it is incumbents who must react, Ragsdale and Cook provide evidence that challenger expenditures are strongly influenced by incumbent resources in the previous election. Even if the direction of causality were clearly as Jacobson posits, this still would not imply that incumbents' resources can be discounted completely in evaluating the strategic politicians thesis. Thus, we believe that a reasonable approach is to com-

³This is the argument implicit in Jacobson and Kernell (1981, pp. 41-42). Even though it is not explicitly stated, the context of the comparison was elucidated to us by Jacobson.

Table 1. Ratios of Democratic to Republican Average Campaign Expenditures for the House of Representatives, 1972-1978

	Democratic Incumbent/ Republican Challenger	Open Seat	Republican Incumbent/ Democratic Challenger
1972	.603	.514	.366
1974	.691	.563	.421
1976	.588	.596	.331
1978	.604	.524	.338

Source: Computed from Jacobson and Kernell (1981, p. 41).

pare spending levels of candidates who run against each other: Democratic (Republican) incumbents with Republican (Democratic) challengers. We have recalculated Table 4.1 in Jacobson and Kernell (1981, p. 41) and report the results in Table 1.

At first glance the figures appear to be supportive of the Jacobson-Kernell thesis. Spending ratios for Democrats in 1974 jumped substantially across the board. Indeed, Democratic challengers running against GOP incumbents finally appeared to be somewhat competitive with respect to financial resources. The largest percentage increase from 1972 to 1974 came among Democratic incumbents. The ratio of 1974 to 1972 advantages for that group is 1.15. The corresponding figures for open seats and for races with GOP incumbents are 1.10 and 1.09. The Jacobson-Kernell thesis would lead us to expect a sharp rise in funding primarily for Democratic challengers facing Republican incumbents, but in fact the increase was across the board, which suggests that Democratic races were viewed as good investments by contributors regardless of the incumbency status in a particular district.

Ratios calculated for 1976 versus 1974 indicate that the Democratic advantage for 1976 was of roughly similar magnitude for seats with Democratic and Republican incumbents (1.18 and 1.16). The Democrats actually did comparatively better in their funding of open-seat races in 1976 than they did in 1974. Finally, if the Jacobson-Kernell thesis were correct, we would expect that the ratio of Democratic to Republican funding would decline from 1976 to 1978, when the incumbent party was considerably less popular. This happened only for open seat races, as Democratic incumbents actually increased their advantage over GOP challengers while Republican incumbents had proportionately fewer resources (albeit by a miniscule amount) than they did in 1976.

If voters responded only to the quality of challengers, well-funded Republican incumbents

and challengers with experience would have done as well in 1974 as they did in 1972, so that "Republican losses . . . would have been entirely self-inflicted. Presumably this would not happen very many times before politicians, who are not in the business of getting these things wrong, would discover that such strategies are badly flawed" (Kiewiet, 1983, p. 128). To corroborate the Jacobson and Kernell thesis, one would also need data on the prior background of state legislative candidates, because the Democratic sweep of 1974 was deep as well as wide. The Democrats took control of 15 additional state legislative chambers, losing only the Colorado Senate to the Republicans. One could also extend this argument all the way down the line—to city councils, even to sheriffs. Sooner or later, one has to reach a level of office for which there could be no conceivable prior elective position.

Yet, contrary to Kiewiet's argument, the alternative thesis that voters do respond to the economy (and to Watergate) is not quite so firmly established. Most of the evidence, including his own, is mixed. We suggest that the reason various studies have yielded conflicting assessments is because they are not asking the appropriate question. Studies of retrospective voting have almost all focused on cross-sectional analyses of why people vote the way they do. Did people vote Democratic in 1974 because they were concerned about either Watergate or the economy, on the one hand, or because they identified with the Democratic party, liked (disliked) Democratic (Republican) incumbents generally, or were closer to one party's candidate on the issues, on the other hand? These are interesting questions in their own right, but they do not address the issue of why the Democrats gained so many votes (and seats) from the Republicans in 1974 compared to 1972.

Put another way, why did so many people who either voted Republican for Congress or did not vote at all in 1972 vote Democratic two years

later? The focus of retrospective voting studies has been the 1974 vote, but the central question should be the vote change. Consider voters who had cast a ballot for a Democratic congressional candidate in 1972. Clearly they were a numerous group since the Democrats took 52.7% of the two-party House vote that year. Furthermore, they were very likely to be Democratic party identifiers as well, as most Republicans and independents (particularly those who also voted for Nixon) supported GOP congressional candidates (Jacobson, 1983, p. 137). How could such a voter punish the Republican administration for the twin devils of Watergate and inflation? Only by voting Democratic again in 1974! But because most of these voters are Democrats anyway, it would hardly be surprising to find that party identification would swamp the effects of such retrospective factors, as it does even in Fiorina's analysis.

The appropriate way to examine the impact of Watergate and the economy is to consider voting changes from 1972 to 1974. Fortunately, there is a panel conducted by the Center for Political Studies which can be used to examine just such questions. Key (1966) pioneered the study of vote changes, but his analyses depended upon people's recall of their previous ballot choices. Nevertheless, he found the electorate quite "rational" in presidential elections. Weatherford (1978) used the 1956-1958-1960 panel and found significant effects for vote changes from 1956 to 1958, controlling for party, at least for the working class.⁴ Only Brody (1977) has analyzed the panel for comparisons between 1972 and 1974, but his emphasis was solely on change in party identification.

He and Fiorina maintain that there is considerable variation in partisanship even with the use of contemporaneous expressions of party identification at various waves of the panel. Indeed, 10 to 15% of respondents reported shifts in party identification from 1972 to 1974 and "party identification responds to short-term electoral forces" (Brody, 1977, p. 30). Specifically, he found that people who were dissatisfied with either Nixon (as measured by the feeling thermometer) or with their family income were more likely than the doubly satisfied to change party identification—and the doubly dissatisfied were three times as likely to change identification as the doubly satisfied. But Brody neither examines the

impact of party-identification change on the vote nor presents a multivariate model, as does Fiorina (1981, chap. 5). We examine changes in the vote from 1972 to 1974 in the context of a more fully specified model. Whenever possible, our independent variables are not simply demographic or political constructs believed to affect the vote *direction*. Instead of examining voting change as a function of trust in government (or as a function of the Nixon feeling thermometer or voters' perceptions of the economy), we look at changes in trust in government, and the other relevant variables whenever possible. Some variables are simply not available in 1972 (the Nixon pardon question), whereas others do not need to be considered over time (party of incumbent in 1974). In general, however, to explain change, we believe that we must focus on change. We turn now to a specification of the models to be estimated.

Data Analysis

The data base, as indicated, is the panel survey of the Center for Political Studies taken in 1972 and 1974. We use four dependent variables. The first is a measure of movement toward the Democrats in 1974, coded from 1 (people who either voted Democratic or for an independent minor party candidate or did not vote in 1972, and who voted Republican in 1974) to 4 (people who either voted Republican, voted for an independent or minor party candidate, or did not vote in 1972, and who voted for Democratic congressional candidates in 1974). The two intermediate categories encompass voters who cast GOP ballots or Democratic ones in both elections (cf. Weatherford, 1978). We also include a simpler measure for all voters in the panel: congressional voting behavior in 1974. This is the traditional variable used in retrospective voting studies, and it will thus provide a benchmark for comparisons with the change variables. We do, however, restrict the number of cases to the 467 for which we have complete data for the first dependent variable. The third dependent variable, encompassing switchers and new voters,⁵ is derived from the first, taking only the two extreme values. The fourth, also derived from the first, includes only standpatters. The 229 cases in the sample residing

⁴The coefficient for the middle class was not significant. The coefficient for the full sample is not designated as significant in Table 3 (p. 930), but it has an *F*-ratio of 3.8, equal to a *t*-ratio of 1.95, marginally different from the conventional 1.96 used for the .05 level for a two-tailed test.

⁵Results only for switchers (excluding new voters) were virtually identical, so we opted for the slightly larger sample sizes obtained by including 1972 nonvoters. There was no theoretical reason to presume that the factors that mobilized 1972 nonvoters to select Democratic congressional candidates in 1974 were different from those that induced people who voted for the GOP in 1972 to switch.

in congressional districts in which there was no challenger are excluded.

Because regression analysis is not appropriate for categorical dependent variables, we use probit analysis (cf. Aldrich & Cnudde, 1975; Fiorina, 1981). However, in contrast to regression analysis, one cannot interpret probit coefficients in terms of amount of change in the dependent variable given a unit change in a predictor. Thus we must rely upon two alternative criteria. First, we can compare the z-scores for the predictors. Second, we use a technique developed by Wolfinger and Rosenstone (1981) which converts the estimated values of the dependent variable into probabilities for each case.⁶

The variables included in the analysis are discussed in the appendix. First, however, we discuss predictors that are not used. Although the literature on retrospective voting strongly suggests that collective economic judgments are far more important in individual voting decisions than is personal financial status (see especially Kiewiet, 1983), we were limited in the case of 1974 because only a post-election survey was conducted, and in 1972 the questions on government performance in managing the economy were asked of only half the sample. The results, however, indicate miniscule coefficients that fail to meet any reasonable significance level. None of the traditional demographic variables used in voting research (age, race, sex, occupation, income, education, marital status) reached acceptable levels of significance (at least .10). Finally, we wanted to examine changes in perceived change in financial conditions from 1972 to 1974 but were unable to do so because the overwhelming majority of respondents gave the same answer to the personal economic considerations in both years.⁷

The variables selected for analysis include: 1) the traditional party identification question,

measured in 1974, and encompassing only the three-point scale recommended by Brody (1977) and Fiorina (1981); 2) change in strength of party identification from 1972 to 1974; 3) change in trust in government from 1972 to 1974; 4) change in one's placement of Nixon on the feeling thermometer from 1972 to 1974; 5) expenditures for 1974 by Republican challengers; 6) whether the incumbent in the congressional district was a Democrat (cf. Fiorina, 1981); and 7) a dummy variable scored at 1 if a respondent either perceived his or her financial situation to be worse in 1974 or who disapproved of the Nixon pardon *or* both and at 0 only for people whose personal finances were not perceived as worse and who also approved of Ford's pardon of Nixon. Neither condition alone produced very many changes in partisan affiliation, and our initial work confirmed that there were only modest effects for either variable on vote changes. We thus followed Brody's lead and hypothesized that the relationship was more complex. Brody (1977) noted that changes in party identification were enhanced among people who both disapproved of the pardon and who had financial grievances. This variable should have positive coefficients. We expect negative signs for the two party-identification variables. Initially, negative coefficients were anticipated for the change in trust variable, because the events of Watergate might lead to a decline in trust in government. A similar direction is hypothesized for the change in the Nixon feeling thermometer and for GOP challenger expenditures. Finally, a positive sign was hypothesized for the variable "incumbent Democrat." Most straightforwardly, people who were already represented by a Democrat in 1974 would boost that member's vote share. Curiously, as would be predicted by the Jacobson-Kernell model, there was no significant effect for a similar variable for the Republicans *in any direction*.⁸

We are particularly interested in comparing the behavior of switchers and standpatters, as Key (1966) did. The equations for all voters is of some interest since it helps to uncover similarities be-

⁶Operationally, we take the estimated probit equation and derive predicted values for the dependent variable for each case, setting the variable(s) of particular interest first at the minimum value(s) observed. Then we use a routine provided by Steven Rosenstone to convert these predicted values (or z-scores) into probabilities for each case. The second step repeated the first except that the variable(s) of interest are set at the maximum observed value(s). The two probabilities are aggregated, and means are computed. The difference between the means for the maximum values and the minimum values indicates the net impact of the variable in terms of the probability of the direction of the vote (change).

⁷Similarly, we attempted to create two variables for the change in Nixon feeling thermometers from 1972 to 1974 to control for those whose earlier placements were above and below 50 respectively. However, the number of zero values was again extremely large.

⁸Unlike some of the other variables, incumbent Republican was not so strongly correlated with other variables that it should have been deleted on that basis alone. Our criterion for multicollinearity is that suggested by Farrar and Glauber (1967): the multiple correlation for any predictor with the remaining independent variables must be no higher than the multiple correlation between the dependent variable and the entire set of independent variables. On the other hand, money spent by Democratic challengers was highly correlated with the variable "incumbent Democrat." Yet it failed to be significant, even in models without the latter predictor.

Table 2. Probit Analyses for 1974 Voters on Congressional Elections

Predictor	All Voters (Four Category Rank Order)	All Voters (Dichotomy)
Party ID	-.409*** (-7.041)	-.857*** (-11.118)
Party ID change	-.042 (-.687)	.050 (.579)
Change in trust	.042 (1.265)	.057 (1.207)
Change in Nixon feeling thermometer	-.002 (-1.020)	-.0002 (.062)
Republican challenger expenditures	-.103*** (-3.742)	-.169*** (-4.464)
Pardon disapproval/finances worse	.243** (1.968)	.583** (3.329)
Incumbent democrat	.449*** (3.831)	1.026*** (5.935)
Constant	1.7603*** (8.927)	1.105*** (4.602)
\hat{R}^2	.21	.54
Percent predicted correctly		
Probit	81.1	81.1
Null	—	56.1
Spearman's rho	.42	—
N	467	467

Entries are unstandardized probit coefficients; z-scores in parentheses.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

tween these groups. Traditional voting research has stressed continuity, primarily through the prism of party identification. We would expect, however, that switchers would be far less influenced by the simple force of party—and thus also more likely to be affected by short-term factors such as Watergate and economic grievances.

Results of the Analysis

The estimated coefficients in the probit analyses are presented for all voters in Table 2, measured as both simple vote choice and as a ranked indicator, with the categories in the latter being 1 for new voters who voted Republican or for 1972 nonvoters or Democratic voters who voted Republican in 1974, 2 for consistent Republican voters, 3 for consistent Democratic voters, and 4 for new voters and for previous nonvoters or Republican voters in 1972 who voted Democratic in 1974. The results for switchers and standpatters are presented in Table 3. Despite some evidence that the four-category variable for all voters is not

strictly ordinal,⁹ the pattern of significance levels for the dependent variables in Table 2 is quite similar. Because the variables have different scales, however, the probit coefficients cannot be compared. What we find is that both sets of equations are heavily driven by party identification.

⁹Because we were not sure that the four-category vote change variable for all voters was strictly ordinal, we also ran a logit model (which does not assume ordinality) for this variable. Logit, however, is more difficult to interpret because it yields $(k-1)$ sets of coefficients where k is the number of categories in the dependent variable. Hence, instead of eight probit coefficients, we have 24 logit estimates. Briefly, the logit results suggested that only party identification and spending by GOP challengers were significant for all groups. Most errors in prediction occurred among switchers, not surprisingly. But the logit model yielded a much higher rank order correlation (.7) than the probit analysis did, which suggests that the variable is not strictly ordinal. However, the similarity in results for the two constructs for all voters in Table 2 suggests that comparisons can be made, if with some caution.

Contrary to what we expected, there are no significant effects found for party identification change, change in trust, or change in the Nixon feeling thermometer.

The Jacobson and Kernell thesis argues that campaign expenditures, particularly by challengers, would play a major role in vote choice. Republican challenger expenditures were strongly related to both variables, as was incumbent Democrat. The more money Republican challengers spent, the more votes they received. Thus, whereas Democratic incumbents had a larger advantage over their Republican challengers in 1974 than in other recent years (cf. Table 1), what money GOP upstarts did have apparently was put to good use.

However, this result runs directly counter to the Jacobson-Kernell thesis. *If anyone should have been more advantaged in 1974, it should have been Democratic challengers.* Yet spending by Democratic challengers did not yield significant coefficients for any of our four dependent variables, either in combination with expenditures by Republican candidates or when estimated by itself. Another variable designed to test the

Jacobson-Kernell thesis measures the change from 1972 to 1974 of Democratic to all expenditures for contested races in the sample. The logic of this formulation is straightforward. The variable measures the relative improvement in Democratic candidates' financial resources from one election to the next. Thus, linking it to vote change provides a measure of the elasticity of campaign expenditures by Democrats. The surge in Democratic coffers in 1974, as indicated by the Jacobson-Kernell data, should have a pay-off among voters. The variable did yield significant coefficients when considered alone, but not when expenditures of Republican challengers were also entered. When each was entered separately, the GOP challenger expenditure coefficient had consistently higher *z* scores.

The variable for Watergate and inflation does yield significant coefficients for both equations for all voters in the panel. In contrast to previous researchers, then, we have found that Watergate and the economy affected vote choice in 1974, whether we consider the simple congressional vote (as has traditionally been done) or a more complex vote change variable. Overall, the equation

Table 3. Probit Analyses for 1974 Voters on Congressional Elections: Standpatters and Switchers

Predictor	Switchers	Standpatters
Party ID	-.278** (-2.178)	-1.212*** (-10.956)
Party ID change	-.186* (-1.389)	-.167 (1.326)
Change in trust	.165** (2.116)	-.027 (-.399)
Change in Nixon feeling thermometer	-.004 (-.985)	.003 (.817)
Republican challenger expenditures	-.173*** (-3.478)	-.131** (-1.835)
Pardon disapproval/finances worse	.532** (1.835)	.623*** (2.495)
Incumbent Democrat	.657*** (2.357)	1.314*** (5.176)
Constant	.177 (.447)	1.677*** (5.073)
\hat{R}^2	.25	.70
Percent predicted correctly		
Probit	69.3	87.8
Null	57.9	55.4
N	140	327

Entries are unstandardized probit coefficients; *z*-scores in parentheses.

**p* < .10.

***p* < .05.

****p* < .01.

for the 1974 vote performs much better than that for the four-category vote change variable. The latter equation has an \bar{R}^2 of only .21 with a rank order correlation between actual and predicted votes of .42. For the simple congressional vote, the \bar{R}^2 is .54; 81.1% of the cases are predicted correctly, compared to a benchmark of 56.1% for a null model based upon assuming all voters would cast Democratic ballots. The estimated coefficient of determination is only .07 lower than Fiorina's (1981, p. 165) for the same dependent variable with a similar number of cases (454), but which uses 17 predictors instead of our seven. Indeed, our percent predicted correctly is even marginally higher than Fiorina's (80.6).

The greatest vote surge for the Democrats for both conceptualizations of all voters, other than party identification, came not for Democratic challengers but for Democratic incumbents. This result seems both partly reasonable and partly anomalous. On the one hand, it is very plausible that Democratic incumbents did do better in 1974 than in 1972. On the other hand, the failure to find corresponding gains for Democratic challengers (but cf. Fiorina, 1981, p. 162) leads us to inquire precisely where the votes that gave the Democrats 49 additional seats came from. There are three possibilities. First, we could find very different effects for switchers and standpatters. Second, it might be argued that the combined effects of Watergate and inflation, together with change in party identification as a mediating force, produced all the change needed to account for the behavior of switchers to Democratic challengers. Third, there is the possibility that these results tell us something about the nature of the sample. We shall investigate these possibilities after considering the results for switchers and standpatters.

The equations comparing switchers and standpatters in Table 3 suggest that there are some different patterns for these voters. The coefficients for longer-term party-related variables are much stronger, as might be expected, for standpatters. Party identification overwhelms all other predictors for standpatters, based upon the z-scores for the coefficients. It remains important for switchers, but far less so. This is not surprising, because standpatters voted the same way in 1972 and 1974 and presumably have more durable long-term party ties. Similarly, incumbent Democrat has stronger z-scores for standpatters than for switchers. On the other hand, change in party identification has the wrong sign for standpatters, but is significant for switchers at the .10 level. Republican challenger expenditures is another short-term party effect which is stronger for switchers, although it is significant for standpatters as well.

The pattern for Watergate-related variables from the probit equations is mixed. Initially, it appears that the pardon-disapproval and finances-worse construct has a stronger effect for standpatters. It is not surprising to find that traditional Democrats who stayed with their party in 1972 and 1974 would be affected by such concerns, but we would expect switchers to be motivated at least as much by these short-term issues which were so central to the public debate in 1974. When we examine the probabilities of voting Democratic in 1974, we shall find more support for this thesis. There are no significant coefficients for changes in the Nixon feeling thermometer. However, for switchers in particular there is a moderate correlation between this variable and the pardon-disapproval and finances-worse one; a coefficient significant at .10 is found when the latter is dropped in the equation for switchers. Finally, an insignificant negative coefficient is found for change in trust for standpatters, following the results for all voters. For switchers, a significant positive coefficient is obtained. A positive coefficient, however, should not be all that surprising, because trends in trust follow short-term political situations (Miller, 1974). Positive evaluations of the government ("the system worked") might well have emerged from the long trauma of Watergate. It seems eminently reasonable that voters would reward the Democrats, particularly those running for the institution that had forced Nixon from office, for their renewed faith in the system. Those voters who tossed out GOP incumbents for supporting Nixon may have very well thought that their Representatives were far more antisystem than the electorate (cf. Wright, 1977). Overall, the results for standpatters are stronger than for switchers. The respective \bar{R}^2 values are .70 and .25, with 87.8% and 69.3% of the cases predicted correctly. The difference in explanatory power of the equations is accounted for by the differential impact of party identification.¹⁹

All Voters, Switchers, and Standpatters

Because the probit coefficients have no ready interpretation, we need another way of comparing the results obtained for all voters, switchers, and standpatters. We begin with the fact that the predicted probit scores for each case are z-scores which can be converted into probabilities. For each voter, we obtain probabilities at the two extreme situations and these probabilities are ag-

¹⁹Unfortunately, the variables that allow us to assess dimensions of incumbency were not included in the National Election Studies until 1978.

Table 4. Probabilities for Predictors for the 1974 Congressional Vote for Democratic Candidates^a

Predictor	All Voters	Switchers	Standpatters
Party ID	.809/.265/ <u>.541</u>	.666/.471/ <u>.195</u>	.868/.191/ <u>.677</u>
Party ID change	.525/.586/ <u>-.061</u>	.754/.450/ <u>.304</u>	.496/.609/ <u>-.140</u>
Change in trust	.637/.497/ <u>.140</u>	.718/.386/ <u>.332</u>	.506/.552/ <u>-.046</u>
Change in Nixon feeling thermometer	.567/.564/ <u>.003</u>	.667/.457/ <u>.210</u>	.515/.600/ <u>-.085</u>
Pardon disapproval/finances worse	.598/.446/ <u>.152</u>	.622/.437/ <u>.185</u>	.581/.464/ <u>.117</u>
Incumbent Democrat	.693/.430/ <u>.263</u>	.676/.461/ <u>.215</u>	.683/.467/ <u>.216</u>
Republican challenger expenditures	.604/.064/ <u>.540</u>	.671/.029/ <u>.642</u>	.570/.198/ <u>.372</u>
Republican challenger expenditures—adjusted	.604/.257/ <u>.347</u>	.671/.204/ <u>.467</u>	.570/.410/ <u>.160</u>
Republican challenger expenditures—doubly adjusted/incumbent			
Democrat controlled	.727/.365/ <u>.362</u>	.755/.275/ <u>.480</u>	.696/.536/ <u>.160</u>
Four Watergate variables	.686/.470/ <u>.216</u>	.972/.205/ <u>.767</u>	.498/.581/ <u>-.083</u>

^aFirst entries reflect signs of probit coefficients for switchers and reflect predicted greater likelihoods of voting Democratic in 1974; the third entries are changes in probabilities and are underlined.

gregated and averaged across all voters in the (sub-) sample(s), as described in note 6 above. The results are displayed in Table 4. We use the signs of the coefficients for switchers to determine the direction of the impact of the probabilities. The entries in each column in the table are: 1) the average probability, given the values for each case on uncontrolled variables, that the voter would cast a Democratic ballot when the predictor is posited to be favorable to a Democratic vote; 2) a similar measure for the situation when the predictor is posited to be favorable to a Republican vote; and 3) the difference between 1) and 2), which is the marginal effect of the variable(s) in question on 1974 voting behavior.

The critical differences are found between switchers and standpatters. For the latter group, party identification clearly overwhelms all of the other predictors. Controlling for all of the other variables, a Democratic standpatter has a .868 probability of voting for that party in 1974, whereas a GOP standpatter has only a .191 probability of casting a Democratic ballot, for a difference in probabilities of .677. In contrast, a Republican switcher has a .471 probability of voting Democratic and the difference in probabilities, or the marginal impact of party identification on switchers, is only .195, less than a third of that found for standpatters. The variable incumbent Democrat has similar effects for the two sub-samples, however. What is most fascinating is the pattern of the four short-term Watergate-related variables: change in party identification, change in trust, change in the Nixon feeling thermometer, and the combined pardon-disapproval and finances-worse construct. For switchers, the marginal impact of the first two variables is over

30%. A switcher who moved from strongly Republican to strongly Democratic will thus be 30.4% more likely to vote Democratic in 1974 than one who moved the other way. But a standpatter whose party identification shifted strongly toward the Democrats would actually be 14% more likely to vote Republican than a standpatter whose allegiance shifted toward the GOP! The change in probability for the trust variable also moves in a contrary direction to that for standpatters, although the value is quite small (–.046).

Change in the Nixon feeling thermometer yields a marginal impact of over 20% for switchers, compared to a small negative change for standpatters. The pardon-disapproval and finances-worse variable does, as expected, yield positive impacts for both switchers and standpatters. Although the .185 values for changes in probabilities is the smallest impact observed for any of the seven predictors for switchers, it is still almost 7% greater than the change for standpatters. When the four short-term Watergate variables are considered together, their joint marginal impact on standpatters is barely negative (–.083), but strongly positive (.767) for switchers. A switcher with pro-Democratic values on all four variables will have .972 probability of casting a Democratic ballot in 1974, compared to a .204 probability for a switcher with pro-Republican values on all four variables—controlling for the effects of party identification and spending by Republican challengers. What is important here is not the magnitude of the effects for switchers, but rather the great difference in results for the two samples. The estimated joint impacts are “too high” for switchers because there are very few voters in the sample who have either uniformly pro-

Democratic or pro-Republican values on all variables: the highest zero-order correlation across any of the samples between any pair of Watergate variables is $-.22$ for the relationship between change in the Nixon feeling thermometer and the pardon and finances-worse variable for switchers. The results for standpatters must be tempered slightly because it simply goes against all logic to maintain that Democrats who were incensed at Nixon in 1974 took out their rage by being slightly more likely to vote for Republican candidates for Congress. Note first that the "reverse" Watergate effects for standpatters are small anyway and that they are certainly overwhelmed by an even deeper-seated dislike for Nixon encapsulated in the power of one's party identification.

Even as the many other factors of the Watergate year worked against Republicans, those minority party challengers who were well funded scored quite well among both standpatters and particularly switchers. Money may well have been the difference between simply losing, as did almost all Republican challengers, and humiliation. Standpatters in districts with well-funded challengers were 37.2% more likely to vote Republican than they were if the challengers spent nothing, whereas the difference was 64.2% for switchers. But the construction of this variable can readily lead to erroneous conclusions. First, there was one district in the sample in which a Republican challenger spent \$155,580; both a switcher and a standpatter resided in that constituency. But the next highest spending total was \$85,050 for switchers and \$68,040 for standpatters. The highest value, then, was clearly an outlier, and to simulate an outcome based upon every GOP challenger spending that amount of money is little more than an exercise of wishful thinking for the Republican congressional campaign committee in 1974. When we adjust the figures at the upper extremes, more reasonable results are found: .467 for switchers, still the largest marginal impact for this subsample, and .160 for standpatters.

However, there is an additional problem with this simulation. The lower bound of the variable is zero, which occurs whenever the race is not between a Republican challenger and a Democratic incumbent. Thus, to get a more accurate measure of the impact of Republican challenger expenditures, we must treat each race as if it involved such a contest. To do so, we first set the variable incumbent Democrat equal to one for all cases. Then, we must derive an alternative lower bound for expenditures. The data indicate that \$2000 is such a floor, because more than just a handful of candidates were bedeviled with this low level of money in 1974. Doubly adjusting the bounds and

controlling for incumbent Democrat only serves to show how robust the results are. The differences in probabilities barely change at all. However, the probabilities obtained at each bound do show a greater propensity for Democratic voting than the earlier constructs for this variable.

The results for all voters, which would be most directly comparable to previous studies, are once again generally a mixture of the findings for the two subsamples, but mostly closer to the standpatters. The two variables with the greatest marginal impact are party identification and Republican challenger expenditures. Close behind is incumbent Democrat. Nevertheless, our specification does yield noticeable effects for Watergate and the economy: the pardon-disapproval and finances-worse variable and change in trust each increase the probability that a voter will cast a Democratic ballot by approximately 15%. The joint impact of the four Watergate variables is a change in probability of .216. Only change in the Nixon feeling thermometer and change in party identification yield no measurable pro-Democratic impact.

The failure to find Watergate effects in previous studies can be traced largely but not exclusively to the failure to analyze switchers and standpatters separately. There are such effects for all voters, however, and this suggests that the question cannot be reduced entirely to differences in the subsamples. Specification is also a key issue, as are methods of analysis. The separate analyses for switchers and standpatters do not establish simply that there are Watergate effects, but rather that for some voters these effects may be much longer than for others—and, indeed, that for these variables the results for all voters may be driven by a relatively small subsample of voters.

The Nature of the Sample

Most of our results make sense in light of the conventional wisdom about the 1974 elections. However, in battles which seemed to highlight the success rate of Democratic *challengers*, we find that the variable "incumbent Democrat" was quite significant and had a considerable marginal impact as well. Did we miss something important about the 1974 elections, or is there another explanation for these findings? Earlier, we offered three possible explanations for how this variable might have considerable effects on voting behavior. First, there is the possibility that the voting behavior of standpatters and switchers was asymmetric with respect to incumbents and challengers. The probit equations suggested that this might indeed be the case, because standpatters

Table 5. District Outcomes and Panel Voters for Races Involving Incumbents with Major Party Challengers in 1974: Democrats

Incumbent's Percentage of Two-Party Vote	Percent of Contested Races with Democratic Incumbent			Percent of All Contested Races		
	All Districts	Panel Voters	Switchers	All Districts	Panel Voters	Switchers
60+	85.9	66.5	56.3	33.8	33.5	30.2
55-59.9	7.0	11.9	16.5	2.8	6.0	8.9
50-54.9	5.3	7.3	12.6	2.1	3.7	6.8
45-49.5	1.2	7.9	6.8	0.5	4.0	3.6
Below 45	0.6	6.3	7.8	0.2	3.2	4.2
Total (%)	100.0	99.9	100.0	39.4	50.4	53.7
N	171	302	103	171	600	192

Sources: Center for Political Studies, National Election Studies 1972-1974 Panel; *Congressional Quarterly Almanac* 1974, pp. 857-864.

had a much higher z-score for this variable than did switchers. However, our analysis of the marginal impact of "incumbent Democrat" yielded virtually identical changes in probabilities for the two subsamples. Our second explanation was that the equation for switchers might fully account for the surge for challengers. The R^2 value for the switchers' equation certainly seems capable of improvement. Thus, we must consider the third explanation: The nature of the sample led to the strong coefficients for incumbent Democrat.

Tables 5 and 6 report breakdowns of the distribution of the sample and the corresponding population (in terms of congressional districts) by incumbents' percentages of the two-party vote in 1974. Again, uncontested districts are eliminated; all 56 had Democratic incumbents. The remaining districts left as sampling possibilities numbered

171 with Democratic incumbents, 166 with Republican incumbents, and 42 open seats. However, the sample greatly overrepresented constituencies with Democratic incumbents: there were 302 voters residing in districts with Democratic incumbents compared to 191 represented by GOP incumbents and 107 in open-seat districts. The ratio of respondents residing in contested districts with Democratic incumbents to those with GOP members was 1.32 in the sample compared to 1.03 in the population.¹¹

¹¹These respondents (in Tables 5 and 6) include only voters. The *N*s vary from those in the equations estimated in Table 2 because we employed listwise deletion of missing data. It should also be noted that the congressional district was *not* the primary sampling unit in 1974. However, that does not obviate our claims. For

Table 6. District Outcomes and Panel Voters for Races Involving Incumbents with Major Party Challengers in 1974: Republicans

Incumbent's Percentage of Two-Party Vote	Percent of Contested Races with Democratic Incumbent			Percent of All Contested Races		
	All Districts	Panel Voters	Switchers	All Districts	Panel Voters	Switchers
60+	33.7	38.2	43.9	12.9	12.2	13.0
55-59.9	20.5	28.3	33.3	7.8	9.0	9.9
50-54.9	23.5	18.3	15.8	9.0	5.8	4.7
45-49.5	16.3	13.6	7.0	6.2	4.3	2.1
Below 45	6.0	1.6	0.0	2.3	0.5	0.0
Total (%)	100.0	100.0	100.0	38.2	31.8	29.7
N	166	191	57	166	600	192

Sources: See Table 5.

Not only did the sample include proportionately too many voters from districts with Democratic incumbents, but it also overrepresented districts with marginal Democratic incumbents and of the three majority party members who lost. The relevant data are in Table 5. Only 1.8% of Democratic incumbents lost in 1974, but 14.2% of the panel voters and 14.6% of the switchers were from such districts. When we shift our focus to *all* districts, Democratic incumbents who were defeated constitute a miniscule 0.7% of the population, but 7.2% of all panel voters and 7.8% of all switchers reside in these constituencies. In contrast, voters in Republican districts with incumbents who scraped by with less than 55% of the vote or who were defeated are underrepresented by substantial margins. The data are presented in Table 6. Although 22.3% of districts with GOP incumbents fell to the Democrats in 1974, voters in those constituencies constituted only 15.2% of the panel and just 7% of the switchers. Republican members who lost were four times as numerous across all districts as the percentage of switchers in those constituencies. There were, then, just four people interviewed who switched votes from 1972 to 1974 and resided in districts that tossed out Republican incumbents!

Given these sample biases, it is straightforward to understand why "incumbent Democrat" has significant coefficients in all equations estimated in Tables 2 and 3. There may be some attraction to bring even more votes to already successful Democrats, but the strength of the coefficients is surely overestimated. If this is the case, then it is likely that spending by Republican challengers, which would limit the vote-gathering possibilities by incumbent Democrats, is also estimated to be stronger than it should be.

The bias in the sample undoubtedly attenuates the coefficients for Watergate and economic grievances. Almost twice as many of the switchers (103 to 57) lived in districts with Democratic incumbents rather than Republican members. Thus, the sample underestimates the impact of throwing the rascals out. Someone who voted for a Republican challenger to a Democratic incumbent in 1972 and then cast a ballot for the incumbent two years later contributed less to the shock value of the 1974 election than did another voter who helped bring down a Republican incumbent he or

she supported just one election ago. Because the punishment of Republican incumbents tended to be somewhat selective—toward the more pro-Nixon, conservative members (cf. Wright, 1977), one can make a good case that the sample bias will seriously underestimate the effects of Watergate and economic complaints for switchers.

Conclusion

The politicians and the pundits did not miss the message of the 1974 congressional elections, as well as those for lesser offices. But it would be a mistake to argue that those contests were nothing more than a referendum on Watergate and the economy. For most voters, those who traditionally rely upon party identification and who cast ballots for the same party or candidate election after election, voting is a ritual. That ritual may very well have policy content and is likely to have some retrospective component as well. To paraphrase Key, standpatters are not fools either. But their voting behavior is driven by loyalties to party and candidate which stem from either deep-seated policy agreement or a more general affect that transcends most national political trends. The importance of such loyalties was never more apparent than in 1972, when Nixon scored a massive landslide but the Democrats retained control of the Congress.

Periodically, however, there are vote swings of such a magnitude that they change the balance of power in the halls of Congress. These are often, although not exclusively, mid-term contests (1946, 1958, 1966, 1974). The ripple effects of the 1974 election, like all landslides, occurred because of changes at the margin by voters (cf. Fiorina, 1977). The Democratic share of the two-party congressional vote rose just under 6% from 1972 to 1974. It is this change that commands our attention and that politicians and pundits follow. As they know, retrospective voting didn't start with Watergate. But neither did it terminate with the escalation of the Vietnam War in 1966.

The "uncovering" of Watergate and economy effects does not suffice to undermine the Jacobson-Kernell thesis about strategic politicians. If the coefficients on "incumbent Democrat" and expenditures by Republican challengers were more robust, they would effectively put that formulation to rest. At the least, one must now argue that there was an identifiable Watergate-economy effect in 1974 that, if anything, was underestimated. Given the reinterpretation of the Jacobson-Kernell expenditure data in Table 1 above and the identification of such effects for switchers, there is probably no need to reject the conventional wisdom about what the 1974 elections settled. The scope of the

an argument about the 1978 sample, the first to use the congressional district as the primary sampling unit, and its biases for analyzing Senate races, see Mann and Wolfinger (1980). The pro-incumbent bias on CPS samples is discussed by Eubank and Gow (1983) and Gow and Eubank (1984).

landslide was too broad and deep to require that strategic politicians be available everywhere to exploit Democratic victory hopes. However, it does seem very plausible to argue that such enhanced prospects did bring out the Democrats' best and brightest; we cannot divorce the argument about strategic politicians from their political environment. Just as Mayor Fiorello LaGuardia of New York claimed that he could win reelection "on a laundry ticket," many far lesser candidates even in 1974 benefited from their simple identification with the Democratic Party. Much of the evidence is anecdotal, but the "Watergate babies" who entered the Congress in 1975 included many amateurs (Granat, 1984).

Altogether, our results indicate that the Democratic surge in 1974 can be explained by a framework that recognizes vote switchers, contributors, and candidates as making strategic investment decisions. Switchers whose trust in government increased as a result of the Democrats forcing Nixon from office but who had either Watergate or economic grievances against the Republicans moved toward the Democrats in 1974. To the extent that the results for the variable "incumbent Democrat" are robust (cf. Fiorina, 1981, pp. 164-167), such voting behavior primarily rewarded sitting members of the majority party in Congress for opposition to administration policies. Because there were substantially more Democratic than Republican incumbents, this voting pattern merely reflects the opportunity structure available to voters. Democratic candidates across the board fared better relative to their Republican opponents in terms of campaign expenditures in 1974 compared to 1972. But this does not imply that contributors simply poured money into the campaign coffers of Democratic incumbents in 1974. Indeed, the average dollar amount available to those incumbents, not even adjusted for inflation, declined from 1972 to 1974 (Jacobson & Kernell, 1981, p. 41). However, the decline in contributions to Republican challengers was much more precipitous, so that Democratic incumbents scored the largest percentage gain in funds expended from 1972 to 1974. Simply put, contributors realized that most Democratic incumbents did not need substantial sums to win, even to win big, in 1974, and that money given to their GOP challengers might increase Republican vote totals but would not save such challengers from defeat.

On the other hand, Republican incumbents averaged almost \$30,000 more in expenditures in 1974 compared to 1972 and still far outdistanced their Democratic challengers in terms of funds available (Jacobson & Kernell, 1981, p. 41). But, relatively, Democratic challengers had far more funds in 1974 than two years earlier. Yet the

failure of Democratic challenger expenditures to reach significance in our equations suggests that there were larger macropolitical factors at work in accounting for vote change in 1974; if anything, the investment decisions of contributors reflected the perception that money would not suffice to counter national trends. Being a strategic politician in 1974 meant being a Democrat; a Democrat with widespread name recognition, either as an incumbent or a challenger with prior elective office, only compounded the difficulties faced by Republican opponents.

The task ahead is to determine whether party identification change can be modeled as an instrumental variable in the same way that Fiorina (1981) has conceptualized lagged identification in his formulation of retrospective voting. Brody (1977) has suggested that this ought to be the case, but the correlations we have uncovered so far between party identification change and other variables are all very modest. We shall pursue this issue in other articles.

There are larger theoretical issues raised by our analysis, ones that are not confined to the interesting but limited question of what drove voters in 1974. Are standpatters and switchers motivated by different factors in other elections as well? If so, which seems likely, what does this imply for the much-heralded decline in the effect of party identification on the vote? Might party identification be alive and well for standpatters (habitual partisans) but weakening for switchers? Might the weakening of party identification be tied to a larger number of switchers in recent years than in the past? The impact of change in party identification upon 1974 vote choice for switchers was quite strong and suggestive of how simple party identification might, for some segments of the electorate, be a reflection of short-term rather than long-term attachments. It might not be surprising to find that switchers behave differently from standpatters, but the magnitudes of the differences we have found suggest that it is inappropriate to use the same models to account for the voting behavior of these two very different groups.

Appendix

Change in trust. Ranges from -5 to +5. It is the difference between two summed indices based on V570 to V574 (1972) and V2229 to V2233 (1974).

Democratic incumbent in 1974. Incumbent Democratic representative in the district = 1; otherwise = 0. (Coded from Gary Jacobson's data.)

Change in Nixon evaluation. Difference between thermometer ratings in 1974 and 1972. Ranges

from -98 to +60. Computed from V2354 (1974) to V702 (1972).

Change in party identification. Change in the strength of party identification between 1972 and 1974. (Computed from V140, V2204. Ranges from +3 to -3.)

Party identification in 1974. 1 = Democrat; 2 = Independent, 3 = Republican (V2204).

Evaluation of Nixon's pardon. 1 = disapproval, 0 = approval (V2166).

Joint evaluation of personal financial condition and Nixon pardon. 0 = approve of pardon and personal financial situation same or better; otherwise = 1. (Computed from V2166, V2313).

Republican challenger expenditures in 1974. Reported campaign expenditures by Republican challengers to Democratic incumbents in 1974. 0 if Republican incumbent or open seat. (Jacobson data).

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Expectations and Preferences in Presidential Nominating Campaigns

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Survey data from the preconvention waves of the 1980 National Election Study are used to estimate the effect of expectations about who will be nominated on respondents' own preferences. The results confirm the conventional belief that bandwagons play an important role in nominating campaigns; at the same time, they suggest that the dynamics of the nominating process may be more subtle than simple bandwagon models would indicate. First, preferences are strongly and consistently projected onto expectations, making the relationship of central interest a reciprocal one. Second, the bases of candidate choice appear to change systematically with political circumstances. In close, volatile campaigns, support for bandwagon candidates (like George Bush in early 1980) is based largely on favorable expectations and on relatively general, diffuse political evaluations (e.g., "leadership"). By comparison, when expectations about the nomination are very one-sided, their impact on preferences approaches zero, and more specific, substantive political evaluations become increasingly important.

The meteoric rise to prominence of Gary Hart is the latest in a series of object lessons concerning the importance of momentum in the contemporary presidential nominating process. Like George McGovern, Jimmy Carter, George Bush, and John Anderson before him, Hart capitalized on better-than-expected performances early in the caucus and primary season to break out of the pack, attracting remarkably broad support from an electorate that had barely recognized his name a few months earlier.

A variety of journalistic and academic observers have documented the apparent impact of momentum in recent nominating campaigns.¹ However, most of these accounts have focused solely on the broad dynamics of the phenomenon; few provide any evidence, or even speculation, about the political character of momentum or about the nature of the individual-level behavioral mechanisms underlying the aggregate-level

phenomenon.² Thus, in the wake of Hart's remarkable success, pundits and analysts found themselves groping for answers to the same kinds of questions they previously asked about Carter, Bush, and others. How did he take off? Was his momentum based mostly on support for his new ideas, on strategic calculation, or on the pure excitement of his rush to the forefront of the race? What are the implications of his success for our understanding and evaluation of the institutions we use to select our presidential nominees?

As a first step toward answering these and similar questions, I propose and test a microlevel model in which the preferences of potential voters are determined in part by their expectations about who will be nominated. A "bandwagon" effect of this sort—however rational or visceral—would account for the main features of the aggregate-level phenomenon of momentum by causing a candidate whose chances of being nominated are improving to attract increasing support, thus improving his chances still further, attracting even more support, and so on over time.³ In this sense, my model provides a useful individual-level test of the importance of momentum in nominating campaigns. At the same time, my focus on the individual political behavior underlying the phenomenon of momentum makes it possible to embed the impact of expectations on preferences within a more general model of political perceptions and preferences in the preconvention period, and thus to specify more precisely than has

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¹For relevant analyses of recent campaigns, see Schram (1977), Witcover (1977), Matthews (1978), Aldrich (1980), Marshall (1981), and Bartels (1983).

²A notable partial exception was provided by Shanks and Palmquist (1981).

³For a mathematical analysis of the dynamics of this process, see Aldrich (1980b).

heretofore been possible the nature and role of "bandwagons" in the nominating process.

Data and Models

In 1980, for the first time, a CPS National Election Study provided extensive information about public preferences and expectations during a presidential nominating campaign. The first part of this multiwave survey consisted of interviews with more than 1000 respondents in late January and February 1980, after the Iowa caucuses but before the New Hampshire primary. It thus provides a useful source of information about individual attitudes and expectations during the crucial early phase of the nominating campaign. A second cross-section of similar scope conducted in April makes it possible to compare early attitudes and preferences with those prevailing well into the sequence of primaries and caucuses.

In both of these cross-sections, and for both parties, the joint distribution of expectations and preferences is strikingly consistent with the bandwagon hypothesis. Among Democrats and Independents, those who expected Carter to win the Democratic nomination were much more likely than other respondents to prefer Carter as the Democratic nominee.⁴ In the January-February survey, 63% of those who *expected* Carter to win also *wanted* him to win, whereas only 7% of those who did not expect him to win preferred him themselves. Similarly, in the April survey, Carter was the first choice of 52% of the respondents who expected him to be nominated, but of only 9% of those who did not expect him to be nominated. Among Republicans and Independents, those who expected Reagan to win were also substantially more likely to prefer him themselves; the proportions were 42% versus 9% in January-February and 45% versus 1% in April.

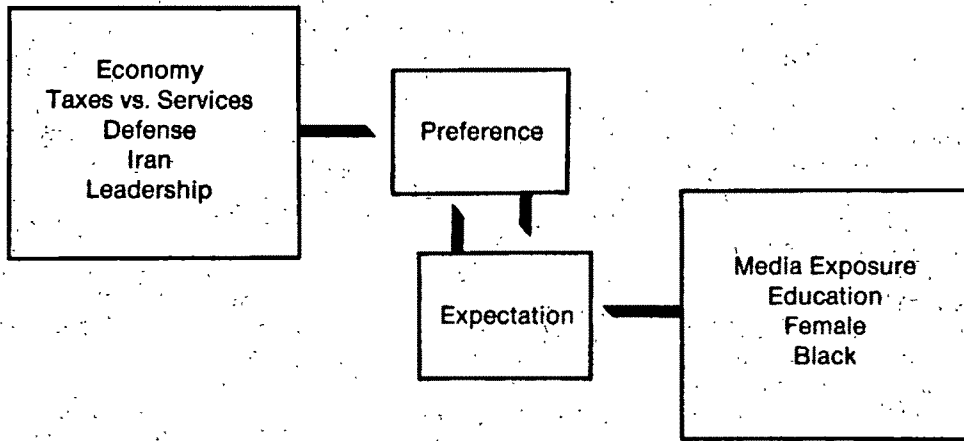
These congruences between expectations and preferences are certainly consistent with the hypothesis that expectations about who will win the nomination influence the preferences of potential voters during the primary season. Indeed, these bivariate relationships between preferences and expectations are as strong or stronger than the relationships between preferences and any of the usual range of variables used to explain preferences, such as

ideology, job ratings, issue positions, or personality evaluations. However, in estimating the causal impact of expectations on preferences, it is necessary to take account of an obvious alternative explanation for the relationship between the two variables: potential voters may first decide which candidate they prefer, and then project these preferences onto their expectations about the outcome of the nominating contest, using selective perception or wishful thinking to convince themselves that their favorite is doing well regardless of the real political situation. Such projection effects have previously been documented, not only for expectations about election outcomes (Berelson, Lazarsfeld, & McPhee, 1954, p. 289), but also for perceived issue positions of candidates (Berelson, Lazarsfeld, & McPhee, 1954, chap. 10; Markus & Converse, 1979; Page & Jones, 1979) and groups (Brady & Sniderman, 1983). These results are substantively important for the revisions they suggest in our inherited theories of political behavior. But they are also methodologically important: even if the phenomenon of projection was of no interest in its own right, we would still have to take it into account in order to estimate accurately other political relationships of interest.

In the present case, if we ignore the fact that primary season expectations are, in part, projections of the potential voters' own preferences, we are bound to overestimate the true strength of the reciprocal bandwagon effect of expectations on preferences. The only way to avoid this bias is to construct a model that explicitly incorporates the simultaneous reciprocal impacts of the bandwagon and projection effects. In the remainder of this article I develop just such a model, based on the assumptions that preferences are influenced by expectations (the bandwagon effect) and by comparisons of the competing candidates on the issues (broadly defined), and that expectations are influenced by preferences (the projection effect) and by social characteristics measuring political awareness. The causal relationship between expectations and preferences is thus allowed to work in both directions, with each of these central variables also determined in part by other variables that can be treated for present purposes as exogenous.

The specific variables included in my analysis of the 1980 data are shown in Figure 1 for Democrats and in Figure 2 for Republicans. The variables on the left side of each figure are those assumed to affect preferences, but not to affect expectations (except indirectly through preferences). The variables on the right side of each figure are those assumed to affect expectations, but not to affect preferences (except indirectly through expectations).

⁴Throughout this article, analyses of the Democratic campaign are based only on responses from Democratic identifiers, Independent Democrats, and "Pure" Independents. Analyses of the Republican campaign are based only on responses from Republican identifiers, Independent Republicans, and "Pure" Independents.

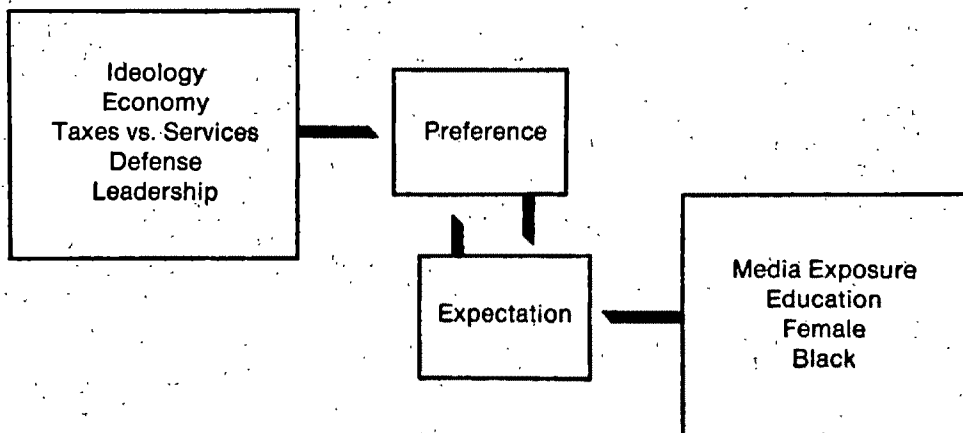
Figure 1. Causal Model for Expectations and Preferences: Democrats

Given the relative paucity of previous theoretical or empirical analyses of preconvention preferences, the exact specification of these models must unavoidably be to some extent a matter of guesswork. Additional exogenous variables could have been included (especially on the preference side of the models); where they would not have altered the results and interpretations presented here, I have excluded them.⁹ This par-

simonious approach seems appropriate because my goal is to investigate a specific causal relation-

⁹Many of the exclusions in the models estimated here are based on preliminary empirical experimentation. For example, education and race were dropped from the preference side of the model because their effects were negligible after controlling for issues, leadership, and

expectations. Several other variables (e.g., age, frequency of church attendance, attitudes about energy and the environment) were dropped from the analysis completely, because they appeared not to have distinct effects of sufficient strength to be worth retaining. On the other hand, the major argument against including a variety of the available "personality" evaluations is a theoretical one: these evaluations—when they exist at all—are unlikely to be exogenous, so that adding them to the models would complicate the analysis greatly without providing much promise of additional substantive insight.

Figure 2. Causal Model for Expectations and Preferences: Republicans

ship, rather than to construct a general explanation of political attitudes in the pre-convention period. In any event, the specifications in Figures 1 and 2 do tap the dimensions of political perception and evaluation most commonly stressed in accounts of the 1980 nominating campaigns (e.g., Harwood, 1980).

The usual method for estimating parameters in models like the ones proposed here is two-stage least squares. In this case, however, the situation is complicated by the fact that expectations and preferences are both measured as dichotomies; for example, Carter is named as the most likely nominee or not, and Bush is a respondent's first choice or not.⁴ These dichotomous responses presumably reflect underlying continua of expectations and preferences which, though unobserved, are the real objects of analysis. In order to estimate causal relationships involving these underlying continua, I have modified the usual two-stage least squares procedure by employing nonlinear logit models in both the first and second stages of the analysis. The first stage uses exogenous variables to construct purged estimates of each respondent's position on the underlying preference and expectations continua, and the second stage estimates the parameters of the structural model relating the underlying continua to each other and to the exogenous variables.⁵

Iowa to New Hampshire: Democrats

Having outlined a model of preferences and perceptions in nominating campaigns, it is now possible to return to the available data in order to estimate the magnitudes of the various causal effects posited in that model. My focus in this and the two subsequent sections will be on the determinants of expectations and preferences across a variety of campaign settings tapped by the 1980

study. Although the bandwagon effect of expectations on preferences is the causal effect of central substantive interest, my results will also shed some light on the role of political issues and evaluations in the development of pre-convention preferences, and less directly on the role of the media in generating and sustaining bandwagon phenomena.

Parameter estimates reflecting the determinants of expectations and preferences for Democrats in the post-Iowa survey are shown in Table 1. The estimates in the first part of the table correspond to the model on the left side of Figure 1. They indicate that preferences for Carter as the Democratic nominee were significantly influenced by evaluations of Carter and Kennedy on a variety of issues. To facilitate comparisons across issues, all of these evaluations were scaled to range from zero to one.⁶ Thus, the relative magnitude of the parameter estimates give a direct indication of the relative importance of different issues in determining nomination preferences. In order to interpret these magnitudes more directly, it is useful to calculate the change in the probability of preferring Carter as the Democratic nominee associated with each of the indicated second-stage logit coefficients. These changes in probability, each calculated for an idealized voter otherwise indifferent between the two candidates, are shown in the last column of Table 1.⁷

⁴The expectations question reads, "Here is a list of people who are possible choices for the Democratic (Republican) Party nomination for President this year. Who on this list do you think is *most likely* to win the Democratic (Republican) nomination?" (NES V153, V178). The preference question is, "Of all the candidates on this list, who is *your* first choice for the Democratic (Republican) nomination this year?" (NES V176, V209).

⁵The two-stage logit procedure serves here to approximate a more coherent but far more complicated maximum likelihood procedure based on the probit assumption that the underlying continuous distribution of expectations and preferences is bivariate normal. For discussions of the relevant methodological issues see Lee (1981) and Maddala (1983, pp. 242-252). The likely practical effect of the simplification employed here is to underestimate somewhat the standard errors of the second-stage parameter estimates.

⁶The original survey items used to generate the issue evaluations were of three types. Evaluations of Carter's handling of Iran were derived directly from a seven-point response (NES V999). Comparative evaluations of the candidates' abilities to "solve economic problems" and "provide strong leadership" were derived from comparative ratings on four-point items (V663, V664, V690, V691, V744, V745, V789, V790). Comparative evaluations of the candidates' positions on taxes vs. services, defense spending, and (for the Republicans) overall ideology were derived from relative distances of the perceived candidate positions from respondents' own positions on a seven-point scale (V944, V947, V954, V1081, V1082, V1083, V1085, V1089, V1114, V1115, V1116, V1118, V1122). In every case, the resulting issue evaluations were scaled so that zero represents the strongest possible preference for Kennedy, 1.0 represents the strongest possible preference for Carter, and .5 represents neutral or missing values. Thus, all of the issue effects in Table 1 (and subsequent tables) are in the expected direction.

⁷Because the logit model posits nonlinear effects, the actual probability change associated with each variable depends on a respondent's overall probability of preferring Carter. It is customary to evaluate the change in probability for a respondent whose score on each variable equals the sample mean, but this sample-specific rule of thumb makes it impossible to compare probability changes across surveys. Given my interest in discerning an intelligible pattern in the results from four

Table 1. Determinants of Preferences and Expectations for Carter, January-February (N=655)

	Parameter Estimate	Approximate Standard Error	Change in Probability
Preferences^a			
Expectation	.375	.162	.093
Leadership	1.543	.886	.368
Economy	3.082	.753	.647
Taxes vs. services	1.906	.882	.443
Defense	1.834	.865	.429
Iran	1.226	.441	.297
Intercept	-5.535	.805	-
Expectations^b			
Preference	.661	.077	.164
Media exposure	.750	.471	.185
Education	.0559	.0377	.014
Female	-.340	.214	-.085
Black	-.704	.259	-.174
Intercept	.778	.469	-

^a-2 log likelihood = 667.4; 76.8% correctly classified.

^b-2 log likelihood = 567.9; 78.0% correctly classified.

The estimates in the second part of Table 1 correspond to the model on the right side of Figure 1. They indicate—not unexpectedly, given the political situation at the time of the post-Iowa survey—that highly educated Democrats and those most exposed to the news media were most likely to expect Carter to be the party's nominee. In addition, Carter was more frequently named as the expected nominee by men than by women and by whites than by blacks.¹⁰

These results are consistent with the idea that expectations are, in part, a reflection of political

awareness, at least when the political environment provides reasonably clear and consistent signals about who is likely to win. But expectations are more than just beliefs based on what people are *told* will happen; to some extent, they also reflect what people *hope* will happen. This projection effect is captured by the parameter estimate for preferences in the expectations equation. Respondents who wanted Carter to win the nomination were significantly more likely than others to expect that he would, in fact, be nominated, even after controlling for differences in the social characteristics of these two groups. A one-unit change on the underlying continuum of preference apparently produced a change of more than sixteen percentage points in a respondent's probability of expecting Carter to be nominated. More concretely, an exogenous increase of one percentage point in an undecided respondent's probability of preferring Carter apparently produced an increase of .661 percentage points in her probability of expecting Carter to win the nomination. Preferences thus appear to have been a powerful determinant of expectations during the early part of the nominating process.

By way of comparison, we can return to the first part of Table 1 to assess the reciprocal impact of expectations on preferences. The parameter estimate, .375, suggests that an exogenous increase of one percentage point in an undecided respondent's probability of expecting Carter to be nominated produced an increase of a little more than a third of a percentage point in her probability of preferring Carter as the Democratic nominee. Although only a little more than half as

separate campaign contexts, it seems preferable here to adopt a less descriptive but more consistent baseline, evaluating the change in probability for a respondent with a .5 probability of preferring Carter. Note, however, that this is precisely the respondent for whom, under the assumptions of the logit model, the change in probability associated with each variable is greatest. Thus, the changes in probability shown in this and subsequent tables should not be interpreted as average effects for each variable in the sample at hand, but as maximum potential effects for respondents at the point of greatest uncertainty.

¹⁰My measure of media exposure is a scale constructed from survey responses concerning exposure to political conversation, network television news, news magazines, newspapers, and radio news (NES V12, V22, V24, V26, V28). Education is measured by years of formal schooling (V1483). The effects of race and sex, captured by dichotomous variables for blacks and females (V1701, V1702), presumably reflect differences in exposure and attention to (mainstream) political news media which persist after controlling for education.

big as the projection effect, this bandwagon effect is big enough to have very significant political consequences. Elsewhere (Bartels, 1983, pp. 191-193) I have estimated that Carter's victory in the Iowa caucuses in 1980 increased his probability of being nominated by about 12 percentage points, and that a single week's results later in the primary season increased his probability of being nominated by as much as 20 percentage points. If these changes in probability can be translated directly into changes in subjective expectations, the results presented here suggest that Carter's primary and caucus successes may have increased his overall popular support by from four to seven percentage points in a single week.

Iowa to New Hampshire: Republicans

In many respects, the Republican contest in the month before the New Hampshire primary provides the most interesting political circumstances of the 1980 nominating campaign. George Bush had just scored a startling upset in the Iowa caucuses and was suddenly "the hottest property in American politics" (Cannon & Peterson, 1980, p. 137). Bush himself, in his campaign speeches, referred frequently and gleefully to "Big Mo," and not without reason: in a period of ten days, Bush gained more than 20 points in the polls, surpassing Reagan as the first choice of Republicans for their party's nomination. The first wave of the 1980 CPS National Election Study was perfectly timed to provide a detailed picture of expectations and preferences during this heady month, and thus of a bandwagon almost comparable in

magnitude (although not in duration) to Carter's in 1976 and Hart's in 1984.

What were the ingredients of Bush's sudden popularity? The logit estimates in the first half of Table 2 provide some indications. First, it is interesting to note that most of the "issues" included in my analysis had substantially smaller impacts on support for Bush among Republicans than on support for Carter among Democrats. The only exception to this pattern involves the most general (and least substantive) of the "issues"—leadership. Both the pattern and the exception suggest that Bush's support was more diffuse, and less firmly grounded in specific political perceptions, than Carter's. In this respect, the results conform to the stereotypical view of bandwagons like Bush's as being non-ideological—even, to some extent, apolitical—in character.

The other interesting difference between Bush's support and Carter's is that expectations had a much greater impact on support for Bush than on support for Carter. On the Democratic side, an exogenous increase of one percentage point in an undecided respondent's probability of expecting Carter to be nominated increased her probability of preferring Carter by about a third of a percentage point. On the Republican side, the corresponding increase in the probability of preferring Bush was more than a full percentage point—a powerful translation of expectations into preferences. It is not hard to understand how bandwagons like Bush's begin to roll when perceptions of success have such a dramatic impact on the preferences of potential voters.

Table 2. Determinants of Preferences and Expectations for Bush, January-February (N=490)

	Parameter Estimate	Approximate Standard Error	Change in Probability
Preference^a			
Expectation	1.176	.203	.286
Leadership	1.340	1.281	.323
Economy	-.316	1.282	-.079
Taxes vs. services	-.084	1.012	-.021
Defense	1.191	1.147	.289
Ideology	.496	1.118	.123
Intercept	-1.607	1.113	—
Expectations^b			
Preference	.632	.140	.157
Media exposure	1.178	.611	.286
Education	-.0549	.0598	-.014
Female	-.524	.236	-.130
Black	4.046	1.242	.766
Intercept	.140	1.124	—

^a-2 log likelihood = 379.2; 84.1% correctly classified.

^b-2 log likelihood = 469.8; 78.2% correctly classified.

Table 3. Determinants of Preferences and Expectations for Carter, April (N=620)

	Parameter Estimate	Approximate Standard Error	Change in Probability
Preferences^a			
Expectation	.087	.117	.022
Leadership	3.754	.696	.735
Economy	.462	.766	.115
Taxes vs. services	2.632	.906	.577
Defense	1.963	.811	.455
Iran	2.114	.408	.484
Intercept	-5.944	.693	-
Expectations^b			
Preference	.595	.096	.148
Media exposure	.927	.555	.228
Education	.1314	.0417	.033
Female	-.800	.279	-.197
Black	-1.194	.297	-.290
Intercept	.996	.548	-

^a-2 log likelihood = 654.5; 72.6% correctly classified.

^b-2 log likelihood = 407.4; 87.3% correctly classified.

Given the impact of expectations on preferences for Bush in the early phase of the 1980 campaign, it seems reasonable to inquire into the determinants of these expectations. Some evidence is provided by the results in the second half of Table 2. Three points are worthy of notice. First, the projection effect, measured by the parameter estimate for preference, is virtually identical in magnitude to the corresponding projection effects among Democrats in both the early and late primary season surveys. This similarity provides some indication of the persistence of projection as a behavioral phenomenon across a variety of political settings.

Second, media exposure seems to have played a powerful role in the development of expectations that Bush would win the Republican nomination; indeed, the estimated effect of media exposure on Republican expectations is more than 50% greater than the corresponding estimated effect of media exposure on Democratic expectations at the same point in time. It is worth recalling that Bush's objective chances of being nominated were, despite his success in Iowa, certainly no better than Carter's at this stage in the campaign. Thus, the greater impact of media exposure on expectations on the Republican side is a reflection not simply of a less ambiguous political reality, but also of the general emphasis and specific interpretations placed on that reality by the media to which potential voters were more or less heavily exposed.

A third, related, point is that education appears to have had a *negative* effect on Republicans' propensity to perceive Bush as the most likely nominee in the month after Iowa. This is the only

instance, of the four investigated here, in which media exposure and education had counteracting rather than reinforcing effects. Although the parameter estimate for education is too imprecise to provide definitive evidence, it is tempting to speculate that, in this instance, the kind of media exposure instrumental in increasing respondents' perceptions of Bush's chances was primarily *un-critical* exposure to the media's month-long fascination with Bush, his campaign, and his "Big Mo" in the wake of his Iowa victory.

Bandwagon and Projection Effects Later in the Primary Season

In order to test the persistence of the relationships linking expectations and preferences in nominating campaigns, I have replicated the analyses of the post-Iowa survey described above, using the separate cross-sectional survey conducted in April 1980. By that time many of the primaries and caucuses were over, and there was considerably less objective uncertainty than there had been in February that Carter and Reagan would be their parties' nominees. Given these changes in the political situation, one might expect either the bandwagon or projection effects, or both, to be less powerful in April than in February.

The results of the replication, shown in Tables 3 and 4, are in keeping with the changes in the political situation in the intervening months. On

Table 4. Determinants of Preferences and Expectations for Reagan, April (N=439).

	Parameter Estimate	Approximate Standard Error	Change in Probability
Preferences^a			
Expectation	.007	.146	.002
Leadership	.868	1.137	.214
Economy	2.136	1.067	.488
Taxes vs. services	1.019	.875	.249
Defense	2.572	.906	.567
Ideology	1.888	.816	.440
Intercept	-4.908	.798	-
Expectations^b			
Preference	.801	.196	.198
Media exposure	1.841	.568	.430
Education	.1174	.0504	.029
Female	-.565	.292	-.140
Black	-.644	.522	-.160
Intercept	.183	.669	-

^a-2 log likelihood = 535.5; 67.9% correctly classified.

^b-2 log likelihood = 335.7; 85.2% correctly classified.

the Democratic side (Table 3), evaluations of Carter's handling of the Iranian crisis and more general evaluations of the candidates' relative "leadership" qualities gained substantially in importance as determinants of nomination preferences as the crisis wore on from February to April. At the same time, perhaps because the rate of inflation began to recede from its January peak of 20% and public attention began to turn from the economy to foreign policy, the effect of comparative economic evaluations on nomination preferences declined significantly. Finally, the bandwagon effect almost disappeared, with an exogenous increase of one percentage point in an undecided respondent's probability of naming Carter as the most likely nominee adding less than one-tenth of a percentage point to her probability of preferring him herself.

On the Republican side (Table 4), comparative evaluations of the candidates with respect to the economy, defense, taxes vs. services, and ideology all increased substantially in importance as determinants of candidate preferences.¹¹ The only

comparative evaluation to decline in importance between the two surveys was the one involving leadership. In addition, for Republicans as for Democrats, the bandwagon effect so evident before New Hampshire had virtually disappeared by April. By this point in the campaign, apparently, respondents in both parties were relying more heavily on substantive political perceptions—and much less heavily on strategic considerations or momentum—than in the earliest weeks of the campaign.

The determinants of expectations in the second part of Tables 3 and 4 also reflect changes in the political situation between February and April. In each party, as it became more and more obvious that Carter and Reagan would be nominated, media exposure and political awareness became more powerful determinants of respondents' expectations. Among Republicans, the contrasting effects of exposure and education in the month after Iowa had become congruent by April, suggesting that the media's interpretation of the race may have changed sufficiently to make sophisticated respondents more, rather than less, likely to accept it. However, the increasing importance of objective determinants of expectations did little to reduce the importance of projection; even in April, with both races for all practical purposes decided, from 60 to 80% of any exogenous change in preferences was projected onto expectations about who would be nominated. This result provides further evidence of the resilience of projection as a psychological mechanism, even in situations where objective cues about the shape of political reality are quite powerful.

¹¹By April, Bush was so far out of the race (only 3.4% of the Republican and Independent respondents still expected him to be nominated) that Reagan had clearly become the sole focus of the Republican campaign. For this reason, I analyzed preferences and expectations for Reagan, rather than for Bush, in the April survey. In order to confirm that the comparisons between the two periods made in the text do not depend crucially on which candidate is the focus of analysis, I also analyzed preferences and expectations for Reagan in the earlier survey; the results were consistent with those reported above.

Patterns and Implications

Having analyzed the determinants of expectations and preferences in a variety of campaign settings, I turn in this section to a discussion of how my results fit together, and of what they suggest about the nature and role of momentum in the presidential nominating process.

From the political standpoint, the candidate preferences of potential primary voters are obviously of key importance, and my analyses shed some light on the determinants of these candidate preferences. In almost every instance, comparative evaluations of the candidates on the issues (the economy, taxes and services, defense, the Iranian crisis) had strong effects on potential voters' preferences. These effects would not be surprising, were it not for the occasional claims of campaign observers (e.g., Gopoian, 1982) that issues play only a minor role in primary voting. The evidence presented here indicates that issue perceptions do matter, particularly in stable contests involving well-known candidates.

What about less stable contests involving less well-known candidates? This case is the more interesting one if our goal is to understand the role of momentum in the nominating process. My analysis of support for Bush at the apogee of his challenge provides a fairly clear picture of the distinctive political character of bandwagon phenomena. If I were to construct a typical profile of bandwagon support, it would include three major elements: unusually weak effects of specific issue perceptions, relatively strong effects of more diffuse perceptions involving general qualities like leadership, and very strong positive effects of expectations on preferences. Thus, it appears that supporters flock to the candidate with momentum mostly because he is new, exciting, and getting a lot of attention, and that they bolster this diffuse support with more specific, reasoned political judgments only later (or, if the candidate fades, not at all).

Under what kinds of political circumstances do these bandwagon phenomena occur? Recent experience suggests that they are most common when unexpected successes by little-known candidates disturb the stability of a nominating campaign; certainly, Carter in 1976, Bush in 1980, and Hart in 1984 all fit comfortably within this general pattern. In principle, we could construct a continuum of political contexts defined by several related features of nominating campaigns, including the number and political stature of the competing candidates, the number of primaries and caucuses that have already occurred, and the apparent closeness of the race. It is not completely clear how these contextual factors fit together in determining the importance of momentum. For-

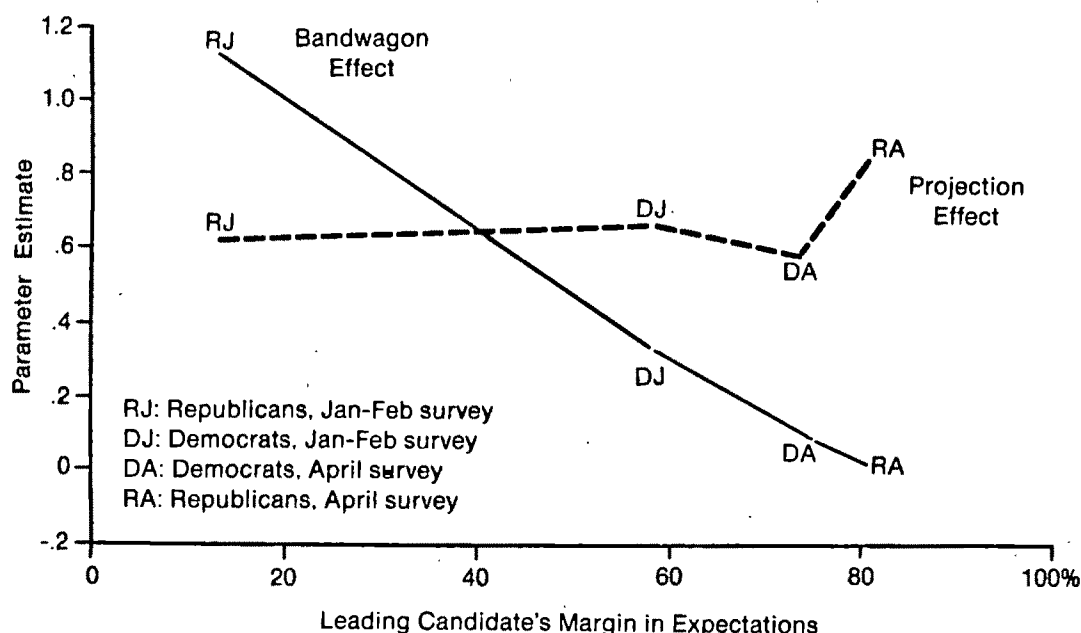
tunately, they tend to covary in actual campaigns: as the primary season progresses the field is winnowed, surviving candidates tend to become better known, and expectations about who will be nominated tend to become less problematic and more widely shared. Thus, it is possible to characterize different campaign situations fairly straightforwardly on the basis of simple indicators, without doing gross violence to the underlying political reality.

For example, it is possible to measure the preponderance of expectations for the frontrunner in each of several different campaign situations. The greater the frontrunner's margin in perceived chances of being nominated, the less volatile the situation, and presumably the less closely that situation approximates the bandwagon conditions typified by the emergence of Carter, Bush, and Hart. When applied to the four separate campaign situations tapped by the 1980 data (Democrats between Iowa and New Hampshire, Republicans between Iowa and New Hampshire, Democrats in April, Republicans in April), this simple criterion successfully distinguishes the situation prevailing in April, when both parties' nominations were practically locked up, from the more fluid situations prevailing in January and February. In addition, it successfully distinguishes the volatile Republican situation after Iowa, when Bush was rapidly vaulting into public prominence and into a narrow lead in the polls, from the much less volatile situation on the Democratic side at the same time, when both candidates were already very well known and their relative standings in the campaign already seemed fairly stable.

Once we begin to distinguish campaign situations in this way, it is possible to explore systematic variations in observed political behavior across different political contexts. In the present case, the most interesting such variation is in the importance of the bandwagon effect from one setting to another. I have already indicated above that the magnitude of this effect varied substantially from the early Republican campaign (where it was very large and positive) to the April campaigns in both parties (where it was essentially zero). In Figure 3, this variation is graphically related to the volatility of each campaign situation, measured by the leading candidate's margin in expectations of being nominated. The relationship is strong and clear: the bandwagon effect is most important in settings where the nomination is perceived to be very much in doubt, and least important in settings where one candidate has established a clear predominance.

By contrast, it is clear from Figure 3 that the magnitude of the projection effect of preferences on expectations is much less variable. In one instance, among Republicans in April, the estimated

Figure 3. Variation Across Campaigns In Bandwagon and Projection Effects



projection effect is noticeably larger than in the other three instances; but this difference does not correspond to any obvious political feature of the campaign and may simply reflect the stochastic nature of parameter estimates. The most reasonable interpretation of the general pattern of these results is that projection is a quite persistent behavioral phenomenon, varying little in impact across the range of campaign settings represented in the 1980 data.

The persistence of the projection effect across a range of campaign settings may make it more or less interesting as a topic for detailed analysis. But in either case, the danger involved in ignoring the phenomenon of projection is easily illustrated by comparing the estimated effects of expectations on preferences presented above with similar estimates based on a simpler model in which projection plays no role. In this simpler model, preferences are determined by issue evaluations and expectations, as before, but expectations are treated as exogenous. The results of the comparison, shown for Democrats and Republicans in the pre-New Hampshire survey in Table 5, are striking. In each case, the effect of expectations on preferences is grossly overestimated by the simpler model—on the Republican side by a factor of two, and on the Democratic side by a factor of seven. The problem in each case is that the portion of the correlation between expectations and

preferences actually due to projection is wrongly attributed to the bandwagon phenomenon in the simpler, misspecified model. In the presence of substantial projection effects, models that do not allow for reciprocal causation simply cannot be trusted to provide meaningful results.¹²

Given a more realistic model that takes due account of projection, it does seem possible to estimate the reciprocal influence of expectations on preferences in a meaningful way. The results presented here suggest that this bandwagon effect can play a major role in the dynamics of nominating campaigns. Under circumstances like those prevailing in the Republican party in early 1980 (and in the Democratic party in early 1984), expectations appear to be translated very powerfully into preferences, making it possible for candidates with momentum to generate new support at a prodigious rate.

¹²A similar problem presumably plagues the analysis of "electability" as a determinant of preferences among party activists offered by Stone & Abramowitz (1983). Although one of their models does treat electability as an endogenous variable, Stone & Abramowitz ignored the possibility that perceptions of electability may be determined partly by personal preferences. To the extent that such a projection effect exists, an analysis like Stone & Abramowitz's will tend to overestimate the importance of electability as a determinant of preferences.

Table 5. Estimated Bandwagon Effects with and without Projection (January-February)

Effect of Expectations on Preferences among	With Projection	Without Projection (Misspecified Model)
Republicans	1.176 (.203)	2.535 (.285)
Democrats	.375 (.162)	2.571 (.364)

My analysis also provides some suggestive evidence about the nature of bandwagon support and about the mechanisms by which it is generated. Presumably, once some political event triggers the process, the behavior of the news media provides a critical link between political reality and the perceptions of potential voters. Horse race coverage, polls, and projections provide a steady barrage of information about who is winning and who is gaining ground.¹³ The impact of media exposure on expectations reflects the results of this barrage. More important, the relative diffuseness and lack of substantive political grounding of bandwagon support reflects what the media do *not* provide with nearly as much volume or enthusiasm at this stage in the campaign: specific information about the candidates' qualifications to be president.

Finally, my analysis provides some indication of the limits of the bandwagon phenomenon as an explanatory variable. Most of the formal models of the dynamics of the nominating process (Aldrich, 1980b; Bartels, 1983, chap. 4; Brady, 1984, pp. 49-55) suggest that expectations and preferences should drive each other in a continuing upward spiral, with successful candidates doing progressively better and others falling by the wayside as the campaign proceeds. But the three most notable bandwagons in recent nominating campaigns—Carter's in 1976, Bush's in 1980, and Hart's in 1984—all appeared to slow during the course of the primary season. The results presented here reflect a similar slowing at the individual behavioral level: although bandwagons were much in evidence immediately after the Iowa caucuses, particularly on the Republican side, they had all but disappeared by April.

Only further investigation will make clear whether these results indicate a more general pattern. But if they do, they put the dynamics of the presidential nominating process in a new light.

The bandwagon phenomenon focused on by earlier analysts, important as it obviously is, may not necessarily lead in the real world to the kinds of outcomes predicted by our simple models. Rather than doing better and better (or worse and worse) in an unbroken cycle, candidates may reach plateaus of support determined in part by their political skills and circumstances.¹⁴ Why? One reason may be that the media, the public, or both tend to tire of the horse race and, eventually, turn their attention to less volatile, more substantive considerations (or simply stop paying attention to the campaign altogether).

The dynamic properties of such a process (and their political implications) would depend crucially on a variety of behavioral and institutional parameters. It might be that momentum stops mattering only after one candidate is so far ahead that the race is, for practical purposes, over. In that case there would be little comfort in the fact that attention turns from the horse race before the horses are back in the stables. On the other hand, and more benignly, momentum might shape the nominating process not by propelling one candidate willy-nilly to the nomination, but by creating a framework in which genuine intraparty competition takes place, winnowing out many of the contenders, giving others unforeseen prominence, but eventually confronting party voters with a meaningful substantive choice.

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¹³For a useful description of the content of media coverage during the 1980 nominating campaign, see Robinson, Conover, & Sheehan (1980). Also relevant is the earlier analysis by Matthews (1976).

¹⁴Starting from somewhat different substantive considerations, Brady (1984, pp. 39-49) constructed a mathematical model of the nominating process exhibiting dynamic properties consistent with this outcome.

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Communications

ON ARTICLES

Comment on The Problem of Strategic Voting under Approval Voting (Vol. 78, December 1984, pp. 952-958)

Niemi (1984) contains egregious errors and radically biased misinterpretations. We begin with two errors, one of omission and one involving an internal contradiction that is unrelated to approval voting.

1. Niemi cites a number of results for voters with dichotomous preferences from our first article on approval voting (AV) (Brams & Fishburn, 1978), claiming that "all of these results depend on the existence of dichotomous preferences, a contrived and empirically unlikely assumption." This is not true. In a subsequent article (Fishburn & Brams, 1981) and in our book (Brams & Fishburn, 1983, chap. 3), we showed that the election of a Condorcet candidate does not depend on all voters' having dichotomous preferences. Indeed, other readily interpretable sufficient conditions *guarantee* the election of a Condorcet candidate when voter preferences are not dichotomous.

For example, we proved that whenever any other common nonranking voting system (e.g., plurality voting [PV] or PV with a runoff) guarantees the election of a Condorcet candidate when voters use admissible (i.e., undominated) strategies, so does AV, but not vice versa. Thus, AV dominates these other systems with respect to a Condorcet "guarantee criterion." If a candidate is not guaranteed election when voters use admissible strategies, computer simulations by Bordley (1983) and Merrill (1984, in press) show that approval voting, on the average, does very well in electing Condorcet candidates, compared with ranking as well as nonranking systems. We give several empirical examples in our book that support this theoretical result (Brams & Fishburn, 1983), as do Merrill (1981), De Maio and Muzzio (1981), De Maio, Muzzio, and Sharrard (1983), Nagel (1984), and Chamberlin, Cohen, and Coombs (1984).

To be sure, the assumption of dichotomous preferences is the only necessary and sufficient condition for the assured election of Condorcet candidates when voters use admissible strategies. But to ignore our copious results on the election of Condorcet candidates under AV as well as other voting systems is to leave the reader with the false impression that we take into account only "a

contrived and empirically unlikely" case. This seems to us a serious error of omission.

In fact, we have empirical evidence that indicates dichotomous preferences are rather common, especially in three-candidate elections. However, even if dichotomous preferences were uncommon, we emphasize that many of our results are not limited to the dichotomous case.

2. Result 12 involves an internal contradiction: Niemi has voters designate some candidates as "approved," the others as "disapproved," and then shows that sophisticated approval voting may entail voting for some disapproved candidates. But this apparent contradiction occurs simply because the goal of sophisticated voting—to obtain preferred outcomes through the successive elimination of dominated strategies—is inconsistent with imposing constraints on the choice of certain strategies (i.e., those that involve voting for disapproved candidates).

Emphatically, this contradiction has absolutely nothing to do with approval voting. Designating only voters' first choices under PV as "acceptable" (to use a different word from "approved"), for example, leads to the same contradiction: it may be optimal for some sophisticated voters under PV to vote for unacceptable candidates.

Thus, the contradiction is internal in the sense that the constraint is incompatible with strategic behavior. It is not peculiar to approval voting but holds generally for voting systems. To avoid such a contradiction, but still retain the notion that only certain candidates are considered "approved" or "acceptable" by some voters, one could assume these constraints of voters are common knowledge (among all voters) in a game of complete information, which is the usual informational assumption made about voter preferences under sophisticated voting. Then sophisticated calculations could be made, given knowledge of these constraints and the presumption that they will be adhered to, and involve no internal contradiction.

We turn now to three significant misinterpretations of our results.

1. The claim of Result 1 is that "AV almost begs the voter to think and behave strategically." Speaking normatively, it is not clear to us why voters should *not* have to consider their choices carefully before they vote, especially when these are sincere. But quite apart from this issue, we were careful to distinguish in our original article

(Brams & Fishburn, 1978) "sincerity" from "strategyproofness" (when a voter has a unique sincere, i.e., dominant, strategy) precisely to indicate that sincere strategies are not necessarily strategyproof.

The important point, though, is not that AV is vulnerable to strategic voting—virtually all systems are—but how it compares to other systems with respect to encouraging an "honest expression" of voter preferences. We claim it is more sincere and strategyproof than other non-ranking systems; perhaps Niemi has in mind other criteria that make AV appear less favorable in promoting an honest expression of preferences. It would be helpful to know what these are so that general comparisons could be made with other voting systems.

2. Results 2-5 for sincere AV are correct, but the impression the reader is left with is highly misleading. If one substitutes PV for AV in all these results, plus Result 1, the statements would still be true.

Moreover, if one drops the phrase, "In the absence of dichotomous preferences," for Results 2-5 (Result 6 does not include this qualification), the statements would also be true for PV. In other words, AV is not only *not* worse than PV in all these situations, but it is in fact better because these alleged "undesirable features" do not hold for AV—but do for PV—in the dichotomous as well as nondichotomous cases.

3. As with the earlier results, Results 7-11 for sophisticated AV (except for Result 8, which is inapplicable to PV) obtain for sophisticated PV without the qualifying phrase, "In the absence of dichotomous preferences." (Result 12 also includes this qualification, but as we pointed out earlier, it contains an internal contradiction not tied to AV.) Hence, at least as far as comparisons with PV are concerned, AV is *uniformly superior* with respect to Niemi's so-called problems.

This conclusion is supported by new studies, published or completed since our book appeared, including the computer simulation results for ranking as well as nonranking systems cited earlier (Bordley, 1983; Merrill, 1984, in press), relative-efficiency results for sincere voting (Hoffman, 1983), and a median-voter Nash equilibrium result, when there are more than two candidates, that does not hold for PV (Cox, 1985). This latter result is game-theoretic, and thus in the spirit of sophisticated voting, although Nash equilibria are more robust than sophisticated equilibria because they are not dependent on the assumption that all other voters act sophisticatedly by eliminating dominated strategies.

Conclusion. We do not mean to imply by our response that AV is perfect—no voting system is—but rather that it has, in our opinion, far more

attractive properties than any other practical system of which we are aware. It is unfortunate that Niemi chose to attack this system without considering how other systems, on a comparative basis, do on his own dozen or so "negative" criteria, as well as on more positive criteria—including "simplicity" and "easiness"—that have been proposed (e.g., see Nurmi, 1983).

We have no doubt that the debate about AV will continue—as indeed it should, because this election reform represents a radical departure from traditional ways of aggregating individual preferences into social choices. For the interested reader, this debate takes on a more empirical flavor in the exchange between Arrington and Brenner (1984) and Brams & Fishburn (1984).

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Reply to Brams and Fishburn

Let me first treat what Brams and Fishburn regard as my errors.

1. Brams and Fishburn think I leave a "false impression" of approval voting. That is ironic, because one of the reasons I wrote the article is to counteract the false impression they have created of approval voting. Although Brams and Fishburn admit that it is not perfect, the general impression one gets from their work is that approval voting contains only virtues and no faults. If I have gotten across the point that approval voting—like other voting systems—has flaws, I will have accomplished one of my main goals.

2. With respect to my result 12, I am quite happy to agree that the constraint I imposed (approving only one's first choice) is incompatible with strategic behavior. I am happy to agree, that is, as long as the reader realizes that *if* an individual approves only his first choice (but can order the other two choices), then a) sophisticated approval voting need not select a Condorcet candidate (my result 7); and b) the individual may have to accept a less preferred alternative than if he behaved strategically.

Now I turn to what they call misinterpretations.

1. In the second paragraph under this point, Brams and Fishburn again apply the reasoning that I think is so misleading. In simplest form, the problem is this. They ask, does approval voting encourage an "honest expression" of voter preferences? Their answer: Approval voting encourages sincere behavior. But that response does not answer the question they asked. I grant that approval voting is more frequently sincere in the technical sense. But sincerity as used with approval voting is not equivalent to honesty. (Or, if you like, one might say that it is equivalent to a very limited form of honesty.) The point is that under approval voting one can be strategic and at the same time be (technically) sincere. Thus, for example, you can calculate whether you are more likely to get what you want by voting for *a* or for

ab—even if you much prefer *a*—but you are regarded as still behaving sincerely (if your preference order is *abc*). So will approval voting encourage more honest expressions of preferences? I don't think anyone can really be sure. I have shown that under approval voting (and plurality voting) voters do not simply express preferences irrespective of political circumstances (Niemi & Bartels, 1984). Those results suggest that approval voting may not encourage honesty any more than plurality voting, but, of course, they do not fully answer the question. In any event, my main point in the article was to stress that the greater sincerity of approval voting does not answer the question about how honestly voters express their preferences.

2. I agree that all of what I said applies to plurality voting as well. My point is simply the reverse. Do not forget that most things that affect plurality voting also affect approval voting.

3. Comparisons of voting systems are useful, but simulations do not answer all of our questions. Simulations often assume random distributions of individuals, no cooperation whatsoever, and sometimes optimal voting strategies. Although valuable—and I have done them myself—they do not tell us how people would actually vote. Nor do they tell us whether or not people will be happy with the outcome and think that the process leading to the outcome was a fair one.

Let me conclude with a short story that says something (to me at least) about the way people might react to approval voting. It involves taxes and schools, something about which individuals have very strong opinions. For this reason, I think that it is a good test of how people would react to an approval voting situation and whether or not they would be happy with an approval voting outcome.

Our local school board has outlined five proposals for dealing with the problem of an aging school: 1. Only state-mandated repairs, such as removal of asbestos; 2. Renovations beyond those state-mandated, but no "moving of walls"; 3. Major renovations, including gutting portions of the school; 4. A new school for grades 9-12; 5. A new school for grades 7-12. A chemist on our faculty (having seen the review of *Approval Voting* in *Science*) called me and suggested that were a vote to be taken on these five alternatives, it should be by approval rather than plurality voting. His concern was that if there were a plurality vote, all of those concerned primarily with taxes would vote for alternative 1, whereas the "pro-school" advocates would split their votes among proposals 2-5. Under approval voting, the pro-school group could vote for more than one alternative, and presumably one of the

last four proposals would beat the minimal-repair alternative.

If faced with such a vote, however, how would the pro-school faction in fact behave? Suppose you most favored building a new school for grades 9-12? Should you vote for both new school projects in order to ensure a new building even though you do not really want seventh and eighth graders going to school with ninth through twelfth graders? Should you vote for the major repair option as well since you are convinced that current facilities are inadequate? Should you even vote for the minor repairs option in order to avoid the state-mandated-only alternative from winning? Or should you only vote for your first choice lest you do yourself in by also giving votes to alternatives you are less happy with?

When the voting is completed, would you be happy with the results? Suppose the new 9-12 school lost by a narrow margin to the major repairs or the 7-12 option? If you (and presumably some others) voted for one of those alternatives as well as for the 9-12 school, was the winning alternative really the most preferred? (In part, how you would feel would probably depend on what information was provided about the vote—e.g., vote totals only, approval votes themselves, or preference order data—a topic I have written about elsewhere; Niemi, 1984.)

Would it be better to hold a plurality vote among the five alternatives? Probably not. However, given the known problems of plurality voting, it is not likely that such vote would actually be taken. Rather, the alternatives would be discussed at great length, with some compromises attempted. Then, a single alternative would probably be proposed for a vote by the district. Not a perfect solution, of course, but one that at least involves a great deal of input before the vote. The danger is that advocates of approval voting will convey the impression that their method so smoothly solves the problems inherent in conflict situations that decisionmakers will mistakenly avoid the process just outlined in favor of an approval vote.

So that Brams and Fishburn do not misunderstand me again, I hasten to add that they have nowhere said that approval voting solves all multi-alternative conflicts. But the impression one gets from their strong advocacy is that it will solve all or almost all such problems. The possibility that it will erode the give and take of politics before a vote—along with the difficulties of knowing whom to vote for and whether to be satisfied after the vote—are some of the major problems involved in approval voting.

Postscript. Footnote 9 in my article should be deleted. Dan Felsenthal and Zeev Maoz of the University of Haifa found counterexamples of

what Gretlein and I thought was a general result. Nothing else in the article is affected.

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Rejoinder to Niemi

We respond to Niemi's "short story," which is quite independent of our critical remarks, with a very short comment. In the innumerable situations in which protagonists are unwilling to compromise their differences, or candidates refuse to bow out of elections gracefully, are we to deplore their behavior as simply bad form? For better or worse, this is the "politics" that Niemi wants to preserve. We want to preserve the ability of voters to cope with precisely those situations in which politicians do not practice self-denial and, for the convenience of voters, reduce voter choices to easy binary ones.

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Comment on Wilson (Vol. 77, December 1983, pp. 895-910)

"To test the extent of corporatist interest group politics in France" (p. 896), Frank L. Wilson presents and analyzes data derived from interviews regarding the frequency and perceived efficacy of various forms of interest group activity vis-à-vis the state. The data are interesting in many respects, but it is difficult not to be troubled by the manner with which Wilson handles their presentation and analysis. To begin with the former, the data are presented in a methodologically curious fashion which serves to understate the frequency and perceived efficacy of formal, corporatist-style interactions between recognized/authorized interest groups and the state. The findings reported in Tables 3-6 include the results of interviews not only with the principal officially recognized groups, but also with what Wilson terms "small dissident groups regarded as 'unrep-

representative' by the government" (p. 900). Wilson acknowledges in a footnote that "20.4% of those interviewed reported that their groups were not members of the Economic and Social Council" (p. 898), and one can safely infer that many if not all of the groups excluded from the ESC were also excluded from many important committees and were denied formal access to government officials as well; indeed, he notes in another passage (p. 900) that some of the groups in his sample held seats on no committees at all. Needless to say, the inclusion of data related to these unrecognized groups must have decreased the percentages of interviewees reporting that corporatist-style interactions with the state are frequent or effective. One can only wonder why Wilson did not disaggregate his data, separating those for the recognized groups (the obvious focus for a test of the extent of corporatization) from those related to the unrecognized groups, so as to gauge accurately the degree to which the recognized groups rely on and value corporatist-style interactions with the state and to demonstrate the manner in which the behavior of those groups differs from that of the "unofficial" groups.

Even though the percentages of Tables 3-6 are skewed in the manner discussed above, they seem to provide evidence of a significant degree of corporatization in terms of the central criteria listed in Wilson's theoretical framework. He states that one of the "most prominent features" of a neo-corporatist pattern of interest group politics "is a set of statutory institutions to accommodate direct contacts between government officials and representatives of authorized interest groups" (p. 896). In a corporatist system, one thus expects to find "well developed, frequent" institutionalized group-state contacts, whereas "under pluralism, statutory contacts between interest groups and government officials are less frequent or nonexistent" (p. 897). How well developed and frequent are such contacts in the French case? Wilson notes that the French system features an "unbelievable quantity" (p. 900) of committees/councils, and he reports that "the form of interest group action most listed as undertaken 'often'" by the group representatives in his sample "was a method linked with corporatist forms of interest representation: participation in the activities of government committees" (p. 898). It strikes me as difficult if not impossible to square these statements with Wilson's conclusion that his "survey of group action in France suggests that the predominant pattern is pluralist" since the "most frequent activities . . . were formal and informal contacts between separate interest groups and political or administrative officials" (p. 907). Somewhere between pages 898 and 907, frequent participation by recognized groups in government committees

has been transmogrified from a definitive feature of a corporatist system to evidence of the predominance of pluralism; this system of "interest group pluralism" does indeed seem to have its "peculiarities" (p. 907).

A careful reading of the article enables one to see how Wilson resolves, at least to his own satisfaction, this apparent contradiction: A system of interest group politics can be categorized as essentially pluralist, despite the presence of a profusion of statutory bodies which are the principal focus of activity for a set of privileged groups, if most group leaders rate "the multi-partite committees, clearly corporatist in form, as *ineffective*" (p. 907, emphasis added). "The presence of corporatist-type structures does not mean the prevalence of corporatist patterns of interest group politics when governments *fail to give any powers* to these bodies . . . neither conservative nor leftist governments have shown any inclination to grant *real power* to the various committees they have proliferated" (p. 907, emphasis added). No French government, Wilson alleges, has been "disposed to grant *any of its authority*" to interest groups; the state has always, he repeats, "*refused to give these bodies decision-making or other powers*" (pp. 908-909, emphasis added).

But is it true that the leaders of the recognized interest groups all view participation in government committees as ineffective? Is it true that governments have granted no decision-making power or authority to any of the committees? These would seem to be absolutely crucial questions, and one would expect to find answers to them in an article that purports to rely on hard evidence rather than mere impressions. Unfortunately, Wilson does not answer these questions satisfactorily. Nowhere does he present a table showing how the various group leaders rate the significance of participation in these committees on a scale from "very ineffective" to "very effective." The only table that comes close to addressing this issue is Table 5, which reports that only 1.1% of the interviewees rated "participation in government committees" as the "*most effective means of action*" (p. 899, emphasis added). The findings of this table are reinforced by selected quotations on pages 900-901, virtually all of which downplay the importance of institutionalized contacts. For several reasons, however, the evidence cited fails to clinch Wilson's argument that the committees are almost universally viewed as ineffective. First of all, he notes that "69% of [farm representatives] rated committees as rather effective or very effective and not one farm spokesman said that the committees had no or very little policy impact" (p. 901). This passage is quite intriguing, because it seems to undermine

the utility of Table 5; after all, the farmers viewed their committees as effective *despite the fact that not a single farm leader rated committee participation as the "most effective" means of action*. The clear implication is that a significant if smaller percentage of leaders from the other sectors may have rated committee participation as at least rather effective. Second, Wilson (1983) fails to cite some relevant data that he presented in another piece. It strikes me that his categorical dismissal of the committees is inconsistent with his assertions (in Wilson, 1982, p. 192) that 82.3% of the group leaders in his sample were usually or sometimes "consulted . . . in advance on legislation affecting their members," that "of those consulted, most felt that the government was usually sincere rather than formal in seeking their opinions on legislative proposals," and that 76% of the interviewees "claimed to be very effective [or somewhat effective] in influencing government policy." The incorporation of these findings into Wilson (1983), it seems logical to assume, would have forced Wilson to draw a more measured conclusion. Third, leaving data aside for the moment, one must question whether the major interest group leaders of France—who can be assumed to be at least minimally rational actors—would devote so much of their limited time and resources to committee participation if they perceived such activity to be totally ineffective.

Although many of the statutory bodies may indeed be a source more of frustration than influence for interest groups, given that officials sometimes do ignore group inputs, Wilson's contention that no French government has "shown any inclination to grant real power to the various committees" (p. 907, emphasis added) is insupportable. In Ehrmann's words, "Many times the authoritative decision is made entirely on the basis of the group's suggestions, so that in effect administrative functions are parceled out to socio-economic forces" (Ehrmann, 1983, p. 205). In fact, some respected observers have argued that the committees constitute a "threatening phenomenon," as recognized groups have "take[n] over administrative functions, and assume[d] . . . certain prerogatives of the State" (Lavau, 1958, p. 82; see also Kuisel, 1981, p. 259). The powers devolved by Center-Right governments to the co-management bodies of the agricultural sector were so significant that the first Minister of Agriculture of the Mitterrand era felt moved to declare: "It is necessary to end the confusion between the role of professional organizations and that of the state. The former must negotiate and contest if they feel it necessary; the state must make the decisions" (Keeler, 1985a). Along similar lines, the "power of decision" regarding commercial development which the

Royer Law of 1973 accorded to commissions staffed in large part by representatives of shopkeeper's organizations was significant enough to be denounced by some as an abdication of state power and responsibility (Keeler, 1984b). In addition, the degree of power that conservative governments enabled representatives of big business to wield, through privileged committee posts as well as formal and informal contacts, was sufficient to inspire a respected observer to remark that it amounted to a "dilution" of "the once-sacred distinction between the private and public sectors in France" (Suleiman, 1978, p. 230). None of this evidence, which seems to contradict flatly the argument that the "aloof" French state has never taken the statutory bodies seriously, is dealt with by Wilson.

In his analysis of the significance of French statutory bodies, as well as other aspects of group-state relations which limitations of space prevent me from discussing here, Wilson exaggerates the extent to which France should be viewed as situated near the pluralist end of the pluralism-corporatism continuum. As many interest group theorists have argued in recent years, it is necessary to disaggregate our analyses and acknowledge that "different interest sectors . . . may be organized and relate to the state in quite different ways" (Schmitter, 1977, p. 14). Most available evidence, including Wilson's data, is consistent not with the conclusion that "France seems to be an exception to the purported trend toward corporatism" (p. 908), but rather with a conclusion that interest group politics have been less fully and evenly corporatized in France than in many other West European polities. Corporatization has been limited in the labor sector by both political and organizational factors, but corporatist group-state relations have developed strongly in the agricultural sector and at least moderately in the business case.

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Reply to Keeler

After accumulating social science data there is then the difficult task of deciding how to present that data in a concise yet thorough fashion. Inevitably, one cannot present all the figures nor disaggregate them in all the ways one would like to do. One must try to minimize distortion and bias in making the selection of the data and of the ways in which to present them. Then comes the equally subjective task of analyzing and interpreting the data.

In his comment, Keeler suggests that I may have presented my data in such a way as to skew the results in order to support my contention that there is little or no corporatist interest group-government interaction in France. Specifically, he complains about my failure to eliminate those respondents whose groups do not belong to any statutory government committee or council in calculating Tables 3-6. My sample, however, was designed to include a representative cross-section of all groups trying to influence politics, whether or not they were deemed representative by the government. To have excluded the responses of respondents from groups not on government consultative committees would have distorted the overall pattern of group activity. In fact, the number of respondents whose groups did not sit on government panels amounted to only 10.1% of the sample, and their exclusion or inclusion would

not have affected the pattern presented in my tables.

Keeler contends that my presentation of the data understates the group representatives' assessment of the efficacy of committees in the policy process. My own feeling from the open-ended as well as closed questions is that even those groups heavily involved in government committees are very reserved in the judgments of the effectiveness of these corporatist bodies. Table 1 presents some data not in my article and is drawn from a question asked only those respondents whose groups did take part in official committees. With the exception of farm spokesmen, it indicates the diverse and unenthusiastic reactions of my respondents to committees as a means of influencing policy.

Keeler then goes on to argue that I misinterpret other data in order to downplay the extent of corporatism in France. He points to some of my statistics that he believes demonstrate corporatism: the large number of official committees, the frequency with which my respondents claimed to participate in such committees, and finally the group leaders' perceptions of their overall effectiveness in influencing policy. But corporatism requires more than simply the presence of such bodies. They must in fact determine policy and, with the exception of farm leaders, virtually none of the group representatives claimed that the committees did that.

The exception of agriculture deserves some elaboration. The quantitative sections of my article showing the farm leaders' positive views of their action in committees were based on interviews in 1979. However, as I note in the article, when followup interviews were done in 1982, farm representatives claimed that these committees had no policy impact. Interviews in 1984 confirmed the virtual dismantlement of whatever corporatist relationship may have existed between farm groups and the conservative government under the Socialists. The "corporatist" bodies still exist, and they continue to meet although not as regularly as before. But as one senior farm leader told me in 1984, they are very formal and no longer have much influence on policy. This is

Table 1. Judgments on the Effectiveness of Committees (%)

	All	Business	Farm	Union
Quite effective	23.0	23.3	69.2	13.6
Effectiveness varies	21.8	26.7	15.4	22.7
Some but not much influence	19.5	20.0	7.7	18.2
None or very little influence	32.2	23.3	0	45.5
No influence but valuable for other reasons	3.4	6.7	7.7	0
N	87	30	13	22

significant because Keeler apparently agrees that corporatism requires the presence of institutionalized group-state contacts. Institutionalization, it seems to me, must at a minimum mean that the relationship would survive changes in government or group leadership. I contend that the easy and near-complete dissolution of the corporatist features of the relationship between the agriculture community and the new Socialist government proves the French government's mastery of such contacts. When it chooses to work with the farmers or other groups because it shares their viewpoint or wants their electoral support, it involves those groups in the policymaking process; when it does not so choose, it can and does exclude them. Thus corporatist bodies are not the signs of an entirely new system of interest group-government interaction. These and other presumably corporatist forms of interest group-government interaction are simply some of a panoply of avenues of influence that French groups may resort to within what remains a complex pattern of interest groups politics.

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Reply to Calvert and Wilson
(Vol. 78, June 1984, pp. 496-497)

We are responding to Calvert and Wilson's (1984) critique of our article (van de Kragt, Orbell, & Dawes, 1983) which first came to our attention when it appeared in the *Review*.

Calvert and Wilson take issue, first, with our use of the term "public good" to describe the bonus we offered our subjects. We don't wish a terminological quibble, but our bonus was characterized by both *joint supply* (one person getting \$10 did not prevent another from getting it) and by *impossibility of exclusion*. Of course, it was a "step level" good with some specified number of contributions being needed before provision, not an *n*-prisoners' dilemma such as the one offered in Calvert and Wilson's tangential table. But the logic of the minimal contributing set we offered depended on this step level characteristic. We attempted to be quite explicit that our model was not intended as a solution to linear dilemmas. If Calvert and Wilson wish to define a public good by the existence of a dominant strategy to defect, then that is their choice. But, we believe, their preferences hardly detract from the interest attached to the problem of providing a step level good that is jointly supplied and non-exclusionary.

Second, they argue that the minimal con-

tributing set is not a "solution" in the sense "of being sufficient, merely by its existence, to drive participants to provide public goods optimally." Of course not. As Calvert and Wilson correctly saw, there are two equilibria in this game, one the Pareto-efficient "all contribute" and another the deficient one of "nobody contribute." We were at some pains to point out that a belief among set members that other set members would not contribute (for *whatever* reason) would make withholding contribution to their personal benefit. Contributing is not a dominant strategy; there are two equilibria; hence, neither is a necessary "solution." (In contrast to the Pareto-optimal equilibrium, the inferior one is the maximin solution—agreement or no agreement.) But the empirical result is that our subjects choose the Pareto-optimal one.

Calvert and Wilson also raise an "important epistemological issue." They point out that much work has been undertaken by economists and political scientists using the utility-maximizing model of participants and, it seems, they dislike our intrusion of "extra features" into explanations of behavior. Our model, *that members of an MCS will contribute because their contributions will be necessary (i.e., critical) for their own enjoyment of the public good*, is surely within the utility-maximizing tradition. The point is that free riding is impossible. What is not impossible is wasting one's contribution (if for *whatever* reason someone else does not contribute). The only "extra feature" beyond the standard utility maximization principle introduced by us is the hypothesis that this possibility of loss has no effect on behavior. That was our interpretation of the finding that the minimal contributing set mechanism (which might better be labelled the *designated minimal set mechanism*) works. That's standard utility analysis. When outcomes that have non-zero probability have no effect on behavior (and surely the probability that someone doesn't contribute is nonzero), their utility is considered trivial. Our finding demonstrated that within our context the possibility of free riding influences behavior, but the possibility of loss doesn't. The findings of these studies alone may be interpreted by assigning trivial utility to the possibility of loss.

In claiming that the MCS is not sufficient "merely by its existence" to ensure optimal provision, Calvert and Wilson make two elementary misreadings of our article. First, they argue that "the structure of the experiment . . . facilitates the *enforcement* (their italics) of such an agreement." Most decidedly it did not. Subjects made their decisions in strict anonymity, knowing they were anonymous; they were paid individually in a separate room from the room in which the experiment was carried on; prior subjects were well clear

of the general area before subsequent ones were released from the experiment room and went to the payroom; finally, subjects knew in advance that prior subjects would be clear of the area. This was all made clear in our article.

Second, Calvert and Wilson argue, communication is crucial to optimal provision. Because communicating groups were never compared to noncommunicating ones, that inference cannot be drawn from the article. It turns out that Calvert and Wilson made a good guess. We have run 11 groups (nine players each) in which no communication was permitted and in which a minimal contributing set of five players was designated via a random drawing. The mean incidence of contributing was 83.6%, which is not 100% but compares favorably to the 48.8% when no contributing set was designated (van de Kragt et al., in press). Evidently eliminating the opportunity to free ride does contribute substantially toward group success even when the contributing set is not designated via discussion, but group success is short of the 100% observed in the experiments when it was specified via discussion. What role is discussion playing, therefore?

A further condition offered individual members of the MCS the bonus (regardless of what they personally did), if the other four contributed, with the result that *not contributing became a dominant strategy*. With the MCS again specified by a lottery, we observed a mean of 40.0% contributing—which does not reach the 96% contributing when discussion was permitted (five groups in each case)—but which compares favorably to the 28% when no MCS was specified (again five groups). Depending on one's perspective (is the glass half empty or half full?), the first finding might be taken as supporting predictions from the assumption of self-interest or not supporting them, but the fact that people contribute at all when their payoffs are independent of their own contribution suggests strongly that something beyond self-interest is involved, something greatly enhanced when discussion is present. Perhaps it is role designation, or perhaps increased concern for group welfare or generalized normative concerns. We are presently testing hypotheses phrased in such terms. But our point here is that such "extra features" do seem necessary to explain our later empirical findings and that theorists should not shrink from considering them.

We interpret our findings so far as strong evidence that decision rules harnessing self-interest to group welfare improves things greatly. But we also interpret them as saying that "extra features" are necessary for a full explanation of what happens. We know, for example, that an enforcement procedure is *not* a necessary "extra

feature," contrary to what Calvert and Wilson assert.

Any disinclination to think about "extra features" seems to us more evidence of a religious disposition than of a scientific one.

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Comment on Carson and Oppenheimer (Vol. 78, March 1984, pp. 163-178)

I doubt that many of us who use the ADA or similar voting index to measure the personal ideology of congressmen are entirely happy with those measures, but the discipline is clearly better off if researchers continue using that type of measure rather than the one proposed by Carson and Oppenheimer in the *March Review*.

Carson and Oppenheimer point to two types of problems in using the voting-based indices of ideology: 1. overestimating the impact of ideology on voting—because voting-based indices reflect not only personal ideology, but also other variables, such as party, and 2. confusion of public and personal ideology—because the representative, in order to get reelected, may hide his true feelings.

Neither of these "problems" justifies a shift to the suggested measure. The first, overestimation, should never be a problem using voting-based indices. The second, confusion of public and personal ideology, cannot be resolved by using the Carson and Oppenheimer measure, and a shift to that measure will create linguistic confusion in the discipline.

Overestimation of the impact of ideology is never a problem if the researcher uses control techniques that allow other variables to account for as much variance as possible before correlating the voting index with the voting behavior.

For example, if one is worried that the ADA index reflects party identification, one can run separate correlations for members of each party between ADA and the dependent variable, or one can run a partial correlation between ADA and the dependent variable controlling for party (and for any other variables of concern). Neither of those techniques could "over-count" ideology. In fact, the influence of personal ideology is *underestimated* by those techniques because personal ideology may have been one of the determinants of party identification. By looking separately at each party, or by letting party account for as much of the variance as possible before looking at the relationship between ideology and the vote, one removes some of the variance in voting that could be attributed to personal ideology.

Confusion between public and personal ideology is a problem, but one that cannot be resolved short of hypnotic investigation of each representative. Certainly we cannot resolve the problem by defining personal ideology to be uncorrelated with public behavior and memberships (as Carson and Oppenheimer do). Such a definition of personal ideology can only lead to linguistic validity problems. As used by others in the discipline (and by the public in general), the term "personal ideology" does not refer to a measure uncorrelated with party. In fact, many may assume that personal ideology is a prime factor in selecting a party. A measure, such as the one proposed, which classifies Cochran as the most liberal U.S. senator—because he is more liberal than other Republicans from Mississippi—will simply spread linguistic confusion.

The authors suggest this defect in their article—twice on p. 173 they note that we are not used to thinking of ideology in this way—and, in their conclusion, they say "we now identify those who go farther to the left of their constituents as the most liberal."

What Carson and Oppenheimer call personal ideology, others call ideological discrepancy. In their recent paper at the Midwest Political Science Association meetings, Covington, Fleisher, and Bond define ideological discrepancy as "the degree to which [representative's] voting records are too liberal or too conservative relative to the type of district they represent." In that paper, ideological discrepancy is measured using residuals from a regression relating district characteristics, party, and region to ADA.

There is just one theoretical model in which personal ideology and ideological discrepancy would be one and the same. That model assumes that representatives almost always vote the wishes of their constituents in an attempt to get reelected, and that any (usually minor) deviation from those wishes must be the result of personal ideology.

There is no empirical support for such a model. Most current studies show that constituency has little impact on representatives' voting. Therefore, personal ideology must be distinguished from ideological discrepancy.

If any more proof is necessary that constituency has limited effect on representatives' voting, it is found in Table 1 of Carson and Oppenheimer. There they use, in their terms, a "kitchen sink" full of constituency indicators (32 in all) to predict voting behavior, and still get an R^2 of only .54 to .57. Thirty-two columns of random numbers would have done almost as well (I got .44 for 1980). There is no empirical justification for assuming the constituency-influence model, and therefore no justification for treating personal ideology and ideological discrepancy as one and the same.

Ideological discrepancy is a variable of great interest to political science—but it should not be confused with personal ideology. There is no benefit in encouraging researchers to substitute a measure of the former for a measure of the latter. It is confusing to call Cochran a liberal; it is not confusing to say that he is more liberal than his state and party would suggest.

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Comment on Carson and Oppenheimer (Vol. 78, March 1984, pp. 163-178)

I was intrigued by Carson and Oppenheimer's recent effort at estimating the personal ideology of political representatives. I was especially interested in the table showing the ideological scores of U.S. senators for 1979-1980. As they suggest, the table contains quite a few peculiar rankings, especially for a number of Democratic senators, usually thought of as liberals, who appear among the more conservative members of that body.

I have no basic quarrel with their approach; in fact, it makes good sense to control for the regional and constituency characteristics of elected officials to establish personal liberalism scores. I see no problem either in throwing almost everything imaginable into the regression equation to predict ADA scores. As they indicate, their purpose is to minimize the sum of squares of the residuals to produce scores that take account of every conceivable influence that might contribute to a senator's personal ideology. I do have a quarrel, however, with the inclusion of party in the equation.

As the authors make clear, they see party as a potentially major influence pushing a legislator either to the left or to the right. One of their ex-

amples mentions Weicker, the Connecticut Republican, whose party if not state push him to the right, but whose ADA scores of 72 and 68 reveal him as a strong liberal at odds with constituency preferences. Without closer examination, this makes sense. But let's consider another example or two to see what their technique produces.

Take the senators from Oklahoma—Boren and Bellmon. Somewhat surprisingly they have *identical* ADA scores for both 1979 and 1980, the years shown in the table. The two obviously share the same regional and constituency characteristics. Yet one (Boren) is considered a conservative (his score of $-.40$ ranks him as the 21st most conservative member), whereas Bellmon with a score of $.26$ appears to be a liberal (counting from the other direction he shows up as the 23rd most liberal senator). This gap of 53 ranks, based on identical ADA scores, apparently arises solely from the difference in party among the two. Or, consider the Texas example. Republican Tower (with ADA scores of 6 and 11) appears more liberal than his Democratic colleague Bentsen, who has ADA scores of 39 and 26. Again, the difference is obviously party. Would those knowledgeable of Texas politics really agree that Tower is more liberal than Bentsen? I don't think so.

This is not to argue that partisan differences are unimportant. To the contrary. I think Carson and Oppenheimer's inclusion of party in their prediction equation does not do justice to partisanship. As they admit, their technique "puts strong liberal labels on Republicans who are liberal." In fact, of the 41 Republican senators, 24 are found in the "liberal" column (59%). Is this as it should be? If Republican office holders across the country, or in the Senate, are generally more conservative than Democrats, which is what the regression equation indicates, why do we find a disproportionate number of Republican senators labeled as liberals? I think it would have been preferable to omit party from the equation and then perhaps test separately whether or not Senate Republicans are more conservative than Senate Democrats. This could be simply done, of course, using a dummy variable for party in a correlation with ideology or as a regression predictor of ideology. Then one could make an independent judgment about the effects of party. If this were done, I suspect that most Republicans in the Senate would be found among the ranks of the conservatives, where they belong. And such anomalies as Tower's more liberal score than Bentsen's would be avoided.

Granted that a residualized measure of a representative's personal ideology will likely yield some anomalous results, and no doubt partisan influences affect a senator's voting record, but to include a measure of party in an equation to

predict ADA scores is guaranteed to generate ideological scores that obscure the effect of party and produce scores that are less useful than might otherwise be the case.

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**Comment on Carson and Oppenheimer
(Vol. 78, March 1984, pp. 163-178)**

The recent article by Richard Carson and Joe Oppenheimer, "A Method of Estimating the Personal Ideology of Political Representatives," (1984) attempts to create a more statistically useful measure of ideology than the more commonly used ADA ratings. But although what they do is statistically rigorous and justifiable, what they are left with, I fear, is not a measure of personal ideology, at least not if we consider ideology to be, as they do, "the usual convention of ideology along the liberal-conservative dimension" (p. 164, note 5).

The problem, it seems to me, is that although what they do makes statistical sense, from the point of view of measuring ideology it is not substantively reasonable. What they want to do is to create a measure that is free from the problem of multicollinearity with other factors such as party and constituency, and to do this they use a technique that allows them to "remove" the effects of these other factors and leaves us with a residualized measure of ideology. The difficulty with this technique is that the problem they are trying to resolve is not simply one of statistical collinearity, it is one of substantive collinearity as well.

Party and ideology are related. Demographic or economic (i.e., constituency) factors and ideology are related. There is no getting around that. Democrats are more liberal, on average, than Republicans; that is one of the reasons they become Democrats. To attempt to isolate completely which is the "liberal" influence and which is the "Democratic" influence is impossible, and to construct a measure of "liberalism" that is entirely independent of party is not possible. The residualized measure that they construct may be measuring something, but it is not liberalism. Any valid measure of ideology in contemporary America will be collinear with party and with certain demographic features of society. This presents statistical headaches, as we get unreliable estimates of coefficients in regressions using these measures. But I for one am afraid there is nothing that can be done about that.

One look at their table of "ideological scores" of U.S. senators (p. 174) shows the difficulties of

using their measure. They note a number of "seemingly anomalous results" (p. 173), but I would contend that any instrument that has Strom Thurmond and John Stennis as two of the ten most "liberal" senators and Abe Ribicoff and Paul Tsongas as two of the ten most "conservative" senators is simply not measuring what it is supposed to be measuring. Liberal Democrats do not become "conservative" simply because they are Democrats from "liberal" northeastern states, and conservatives do not become "liberal" simply because they are southern Republicans.

Furthermore, a closer look at the authors' results and applications shows that they do not really solve the problem of separating the effects of party, constituency, and ideology. What they do simply shifts the problem from one of unreliable estimates to one of uninterpretable coefficients. For example, in discussing their final table the authors explain how using the ADA measure of ideology "decreases" the effect of party on labor issue votes (p. 177). The implication is that by looking at their measure we can get a more accurate measure of the impact of party. However, this is not true. As they make clear, the coefficient for "party" when using their measure of "ideology" is actually a coefficient for the effect of party *plus the aspects of ideology which are collinear with party*. Looking at their figures cannot tell us how much of that coefficient results from party, and how much of it is that portion of ideology which is related to party (and not, therefore, part of their measure of "ideology"). We are, then, left with the same problem as before. We cannot entirely separate the effect of the dependent variables because they are related to each other.

The authors finish by noting the "practical" significance of their measure. "It would permit . . . interest groups to assess which incumbents are 'friendly' and ought to be sup-

ported" (p. 177). By their standards, then, liberals should give money to Jesse Helms (he is more liberal than Gary Hart by their measure), and conservatives should help Bill Bradley (the most conservative man in the Senate) (p. 174). I for one would rather look at ADA ratings.

The intent of the authors is admirable. It would be nice to have an instrument to measure ideology which was not collinear with party and all of the other demographic characteristics they control for. It would make our job a lot easier as we could easily discover the independent effects of each of these factors. Unfortunately, social science is often, necessarily, a bit messy. We do have variables that are in part inseparable, which often leaves us with somewhat unreliable coefficient estimates. I am afraid that this is one of those circumstances. No amount of statistical manipulation can make that problem go away.

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A Reply to Our Critics

Our critics raise questions about the following aspects of "A Method of Estimating the Personal Ideology of Political Representatives": 1) our use of the term "personal ideology" and, indeed, the concept itself; 2) technical aspects of our estimation of personal ideology, and 3) anomalies in our

Table 1. Original and Revised IDCC Scores with Actual and Predicted ADA Scores

	Decile of IDCC (1 = most conservative)		ADA Ratings Actual (mean 1973-1980)		Predicted ADA Rating (mean 1973-1980)	
	Old	New	Even Years	Odd Years	Even Years	Odd Years
Bradley (D-N.J.)	1	1	72	68	88	86
Cochran (R-Miss.)	10	10	22	5	7	2
Hart (D-Col.)	6	9	67	81	73	78
Helms (R-N.C.)	7	5	7	1	6	3
Ribicoff (D-Conn.)	1	5	71	78	76	84
Stennis (D-Miss.)	10	3	6	5	36	33
Thurmond (R-S.C.)	10	5	5	2	11	4
Tsongas (D-Mass.)	1	2	89	74	93	91

rankings of the 1980 personal ideology of U.S. senators. We will consider each in turn.

As noted repeatedly in our article, "personal ideology" refers to the legislator's predilection to vote for or against particular bills statistically independently of the political constraints imposed by constituency and party. We argued that the more commonly used ADA measure (a summary voting statistic) is unable to measure the role of a legislator's ideology independently from the role of constituency and party. When the other components of the ADA measure are factored out by our residualization technique, however, it is possible to estimate the uniquely personal factor. The resulting measure demonstrates no more or no less than which legislators vote more or less liberally than one would expect given their party and constituency.

Because, as our critics point out, personal ideology has a lay meaning, it is difficult to keep the technical and common usage separate. We agree and now suggest the use of a more cumbersome but hopefully less confusing label for the concept: "ideological deviation from constituent/party characteristics" with the acronym IDCC. This formulation underlines that our measure is of ideology relative to party and constituency.

When Bernstein relates ideology to a congressman's choice of a party, he adopts a different definition of ideology from the one measured by IDCC. Given our focus on predicting congressional behavior, the role of ideology in determining party choice is not central to our concerns. He is correct that our measure does not allow us to deal with this issue.

Morgan raises the issue of whether or not ideology should be relativized to party; that is, should party be included in the equations used to estimate IDCC? This depends on the researcher's objectives. We see the usefulness of Morgan's point in some estimation situations and, even more clearly, in developing an index of how inherently liberal or conservative particular districts are without regard to the particular incumbent's party. Usually, however, the researcher intends to use party as a predictor of votes along with ideology. In these cases, party should be included in the equation used to estimate IDCC so that IDCC does not contain the influence of that variable. More generally, any predictor variable which the researcher plans to use in this manner should be in the IDCC equation.

Bernstein's statement that: "Most current studies show that constituency has little impact on representatives' voting" is too strong and the issue is not yet resolved in the literature. Certainly, a dominant theme in much of the literature (and most of the *economic* literature) on voting behavior is that members of Congress simply vote

the interest of their constituents in order to get re-elected and exhibit no independent ideology of their own (c.f. Peltzman, 1984, and the references in our article). We invite the reader to examine the recent article by Kalt and Zupan (1984) which develops a theory very similar to ours, which they apply to congressional voting on strip-mining legislation.¹

The technical issues are raised mainly by Bernstein. He suggests that there are better techniques (especially partial correlations) for handling the multicollinearity problem and for controlling for the unwanted influence of other variables. Although there are multicollinearity problems with most of the voting model estimations we have seen, the major purpose of the IDCC method is to recover an inherently unobservable variable which then can be used in estimating voting models (c.f. pp. 166-169). Partial correlation coefficients are not a solution to the simultaneity or the latent variable problems. They are difficult to interpret in any but simple situations, and their use is inappropriate in structural models with even a moderate number of variables.

Bernstein's second technical point is that our estimates of two equations in our Table 1 are little better than he was able to obtain by regressing 1980 ADA scores against 32 random variables. Bernstein failed to note the number of observations in Table 1. He reports an R^2 of .44, which he compares to our reported R^2 s in Table 1 of .57 and .54. However, our regression had 483 observations, owing to our method of stacking the different senate years. Because what matters is the ratio of estimated parameters to the number of observations, we obtained no R^2 greater than .1 when we used Bernstein's random number approach with 483 senators. Moreover, as was unfortunately unclear in the text of our article, we always used adjusted $R^2(\bar{R}^2)$ in order to correct for the ratio of estimated parameters to observations.²

Finally, what of the anomalies? In our article we suggest the anomalies in the table result from the particular specification of the IDCC estima-

¹We note here that Kalt and Zupan (1984) independently saw a similar need to divide ideology into two components, one due to constituents' interests and one due to deviations from constituents' interests. They also independently developed a method of estimating personal ideology which can be shown to be a special case of the more general method proposed in our article.

²The \bar{R}^2 from Bernstein's random example is only .18—less than half of the R^2 he reports—whereas our \bar{R}^2 s are not much lower than our R^2 s due to the large number of observations. We also note that the F-statistic corresponding to Bernstein's R^2 is just barely significant at the .05 level, whereas the \bar{R}^2 from our equations is significant at better than .0001 level.

tion equations we use to illustrate the method and are not a product of the method itself. We also noted several ways in which that specification could be improved. Because these anomalies trouble our critics, it may be helpful to explore the results of new estimations which incorporate several of the suggestions made in our article.

In the original equations, all senators were constrained to respond to the same forces in the same way. Further, their IDCC values were based on a single session of Congress. This did not take into account the advantage of averaging across multiple sessions and ignored the likelihood that in the same district a Republican incumbent might be responding to different segments of the voters than would a Democrat incumbent because they have different coalitions. Our subsequent work uses average IDCC scores for 1973-1980 and allows for representatives to react differently to their constituency depending on their party, region, and size of their winning margin. For example, in a given district with $x\%$ blacks, our original equation forced every representative to react in the same fashion to this element of their constituency (and to all the others). Thus, in our new equation, the coefficient on the percent black for legislators outside the South is negative for Republicans, indicating a shift to the right as the percent black in a district increases. For the Democrats the coefficient is positive, indicating a shift to the left as the percent black increases.

Table 1 displays our original IDCC scores, our new IDCC scores, and the ADA scores for senators cited as anomalies by us in the article and by our present critics.³ In each case the shift is in the direction of the score expected by those who viewed the original scores as anomalies. Ribicoff, for example, moves from being among those senators who are the most conservative, relative to their constituent-party (and electoral closeness) constraints, to a decile ranking that indicates that he is voting no more or no less liberal or conservatively than average. Similarly, Helms and Thurmond now have IDCC scores that show them to be reflecting their party and state constituencies instead of deviating from them in a strongly liberal direction. Hart moves to a substantially more liberal position, and Stennis moves from being very liberal to voting to the right of his constituents and party.

The scores for each of the three nonshifters—Bradley, Cochran, and Tsongas—have plausible

explanations. Each was first elected to the Senate in 1978. Their IDCC scores therefore reflect their votes for only a single session. In each case, their ADA scores for that session were quite different from the scores predicted by our equation, thus earning them a berth among those with extreme IDCC scores. An examination of the ADA scores for their third and fourth years in office, however, shows that each shifted much closer to their predicted scores. Bradley's ADA scores for 1981 and 1982, for example, are 90 and 100; Cochran's were 10 for both years; Tsongas' were both 95.

This shows two things. First, for an overall understanding of a legislator's relationship to the pressures from his party and constituents his party-constituency voting constraints it is preferable, as we suggested in our article, to average IDCC scores over a number of years to avoid the influence of outliers. Second, the IDCC measure is a useful way to track the changing voting patterns of legislators over time. In the session immediately before his tough 1980 senatorial reelection campaign Hart, for example, shifted from his earlier liberal IDCC decile scores (e.g., 10 in 1975-1976 and 9 in 1977-1978) to 4, a much more conservative IDCC rating.⁴

The remaining cases are two pairs of senators from the same state; Bentsen (D) and Tower (R) from Texas and Boren (D) and Bellmon (R) from Oklahoma. In these cases what we have are not anomalies, but misunderstandings of the nature of the IDCC measure. Morgan asks the question, "Would those knowledgeable of Texas politics really agree that Tower is more liberal than Bentsen?", which he answers with a no and we agree. This, however, is not the question we sought to answer. Ours takes the form of: "Is Bentsen as liberal as his party and constituents would like or allow him to be?", a question that we would have to answer negatively if Bentsen's predecessors as Texas senators, Lyndon Johnson and Ralph Yarborough, are considered. The Oklahoma case with Fred Harris is similar.

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Resources for the Future

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³Most of the senators' IDCC ratings did not experience large changes. However, owing to an error in our data source, the parties of Senators Leahy and Stafford from Vermont were reversed in our original article, a circumstance that affected their IDCC ratings.

⁴Thus the change in Hart's rating noted earlier is due to averaging, whereas those of Helms, Ribicoff, and Thurmond are primarily due to changes in equation specification. The change in Stennis's IDCC score is due to both factors.

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Philip Roeder, "Do New Soviet Leaders Really Make a Difference? Rethinking the 'Succession Connection' "

Joseph A. Schlesinger, "The New American Political Party"

Duncan Snidal, "Coordination versus Prisoners' Dilemma: Implications for International Cooperation and Regimes"

E. Spencer Wellhofer, "Two Nations: Class and Periphery in Late Victorian Britain, 1885-1910"

Forthcoming in December

The following articles have been scheduled for publication in the December issue.

James E. Alt, "Political Parties, World Demand, and Unemployment: Domestic and International Sources of Economic Activity"

Jonathan Bendor, Serge Taylor, and Roland Van Gaalen, "Bureaucratic Expertise versus Legislative Authority: A Model of Deception and Monitoring in Budgeting"

Henry E. Brady and Paul M. Sniderman, "Attitude Attribution: A Group Basis for Political Reasoning"

BOOK REVIEWS

American Government and Politics

Nomination Politics: Party Activists and Presidential Choice. By Alan I. Abramowitz and Walter J. Stone. (New York: Praeger Publishers, 1984. Pp. xiv + 158. \$25.95.)

No distinction is more familiar in the party activism literature than that between "purist" and "pragmatist." For the most part, purists have been depicted as zealots ready to sacrifice electoral victory for ideological consistency. Several scholars have warned of the harm such types could do in the event they took over one or both of the major parties.

Are purists really so single-minded? Abramowitz and Stone contribute to the growing evidence of greater pragmatism among purists than originally thought. Their brief but important book offers a plausible model of how Democratic and Republican activists in caucus-convention states weigh perceived candidate chances in the general election against ideological agreement with candidates in deciding whom to support for the presidential nomination. Electability proved a much better predictor of activist choice than ideological congruence, but together these variables accounted for more than 85% of Democratic and Republican endorsements.

This finding is doubly interesting because big majorities in both parties subscribed to purist principles (as measured by stock but weakly correlated items). Purists evidently had taken over that part of the nominating process observed by Abramowitz and Stone, but most gave ground on ideology when necessary to embrace a more electable candidate. In sum, lip-service to purist ideals should not be taken too seriously. Most activists may want their view of good public policy to be enacted, but they recognize the importance of first winning office.

The data base for this study is enormous if non-random. Aided by numerous others, the authors collected 17,628 completed questionnaires from approximately 35,000 Democrats and Republicans attending 11 state conventions during the spring of 1980 for the purpose of selecting delegates to the presidential nominating conventions. Abramowitz and Stone ably use this statistical bounty to compare Democrats and Repub-

licans, as well as candidate supporters within each party, on socioeconomic backgrounds, group affiliations, and issue stands.

Beyond quibbling with the general wording of certain questionnaire items, I have two criticisms of this study.

First, the authors use "utility" and "ideological satisfaction" interchangeably. Anticipating attacks from this quarter, they marshal strong correlations between points on their liberal-conservative scale and stands on specific issues. Even so, utility remains an empty vessel into which ideological proximity to candidates has been poured. Respondents never were directly asked what they expected to get out of supporting one candidate over another. An item or two of this type would have given utility at least a chance of conceptual independence, and the resulting data might well have reinforced the authors' approach.

Second, the analysis of activist decision making is basically static. By April, 1980, when Abramowitz and Stone first began collecting questionnaires, Reagan's nomination was all but certain and Carter's seemed almost as inevitable. Perhaps such widespread realizations figured in the scant support given Bush and Kennedy in the huge but late Abramowitz-Stone sample. What could an activist expect to derive from supporting an obviously doomed candidate, however close the ideological fit? Now it is unreasonable to fault the authors for not conducting earlier surveys, but a few recall questions to pinpoint the timing of support would have aided the analysis. At what point in the process did purists become more pragmatic about the candidates?

My own experience attests to how many good ideas come to mind *after* the data have been collected, and my criticisms are hardly fatal to this generally sound study. The Abramowitz-Stone model should be replicated in future research, in primary as well as caucus-convention states. It should be modified to take account of campaign dynamics such as the winnowing of candidates, and it should be tested in early contests where seven or eight candidates compete.

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On What the Constitution Means. By Sotirios A. Barber. (Baltimore: Johns Hopkins University Press, 1984. Pp. viii + 245. \$17.50.)

Sotirios Barber has written an excellent book that seeks to present a general theory of "what the Constitution means." The book is really a statement of what is necessary for what Barber calls a "constitutional attitude" to exist in this nation. In order for there to be a constitutional attitude, the nation must do the following: engage in a continuous process of self-critical, aspirational, reaffirmation of the powers of constitutional government (chap. 4), constitutional rights (chap. 5), and constitutional institutions or rules such as separation of powers (chap. 6). This reaffirmation is not a homage to the law, judges, commentators on the law, nor to a contemporary rights or process theory of constitutional choice or one best moral position. Rather, constitutional rights, powers, and institutional norms are evaluated in the light of the goal of insuring that we, all of us, "give and exchange reasons for the ways in which we want to live with each other."

This quest requires that we do the following: honor rights as the most important constitutional obligation; reject self-interest as a basis of choice; oppose balancing as a means to decide rights and the nature of enumerated powers and institutions; continually seek a good society in congruence with the constitutional principles to which we aspire as a nation, knowing there is never an end to this process; and reject blind support for the Framers' policy wants and precedent. We are asked to view enumerated powers and the separation of powers doctrine, with checks and balances, not merely as means to stop abuse of power or rights violations but as praiseworthy in themselves, as an affirmative call to govern ourselves by claims to right reason.

The book is a devastating attack on the way modern constitutional theory and practice is done. Barber rejects: legal realist approaches to what the law is; self-interest as a way to talk about legal conflicts; both narrow interpretivism and wide-open noninterpretivism; consequentialism in the law; the Constitution as pure process values or proceduralism; balancing First Amendment rights with fears of a national emergency; acceptance by the Supreme Court and Congress of pretextual explanations of powers that don't fit the constitutional purpose of the enumerated powers (such as the commerce clause justification for public accommodations legislation); neutrality rather than the affirmative substantive responsibilities of Congress under the Civil War amendments; and judicial self-restraint and the failure of Congress and the president to take constitutional issues seriously.

Based on the above premises, Barber offers a superb analysis of why it is correct to allow abortion choice in the early months of pregnancy but not to consider such choices as a traditional right. He argues for abortion choice because those who oppose it have only come up with reasons that would stop the process of reaffirmation, the search, in its tracks while using religious-based moral ideas that run counter to rights premises in the First Amendment. Likewise, states rights ideas and leaving the allocation of choices on questions of race, gender, and ethnic equality to states are not favored, for they would result in the rejection of national constitutional standards in favor of state and local parochialism, thereby stopping the process of seeking the best society and constitutional values.

This book is a useful contribution to scholars and teachers of constitutional law, judicial politics, and the political philosophy of constitutionalism and the liberal state. Although it is an overstatement to call this work a general theory, it is excellent at creating a picture of how we can get away from the narrowness of the more legalistic means-ends conceptions of the Constitution and judicial choice and process and rights theories that dominate modern constitutional theory. It is also a forceful statement of the drawbacks of failing to take seriously such questions as whether institutions follow enumerated powers and institutional rules such as the separation of powers. It is a call to do so, to have a continuing national debate over the nature of our society and the type of constitutional principles and theory that is required so the debate will never end.

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Feeding Hungry People: Rulemaking in the Food Stamp Program. By Jeffrey M. Berry. (New Brunswick, N.J.: Rutgers University Press, 1984. Pp. xiii + 182. \$22.50.)

Hunger in the United States is a problem which grasps our attention periodically. A recent report by the Physicians Task Force on Hunger in America (*Hunger in America: The Growing Epidemic*, 1985) says hunger has reached "epidemic proportions," with up to 20 million Americans going hungry at some time each month. The physicians report that the problem is getting worse: Lines grow at soup kitchens and food banks, infant mortality rises, and there is widespread testimony about malnutrition. The physicians report that lack of food is not the cause of hunger. The experts attribute hunger to the lingering effects of the 1980's recession combined with

Reagan administration budget cutbacks in food and nutrition programs.

It is within this context that we read Jeffrey Berry's illuminating case study of rulemaking in the food stamp program. Berry begins with a cogent introduction to the regulatory context of social policy. Although we have had many studies of government's economic regulatory activities, Berry breaks new ground by examining social regulation. In particular, Berry looks at the development of issue networks (as replacement for the traditional "iron triangles" in public policymaking) and the role of administrative discretion. His case study is based on elite interviews with lobbyists and with major congressional and executive branch participants in food stamp policymaking, spanning 20 years from the early 1960s to the early 1980s.

Part I details the history of the food stamp program and the development of its regulations. The first chapter provides historical background. Food stamps were originally tried during the New Deal and the Second World War. Berry outlines the revival of the program in the 1960s, a period in which program administrators enjoyed great autonomy. Chapter 2 discusses the rise of hunger as a political issue in the 1960s, with the central role of the media. In the third chapter, Berry notes the increase in allotments and the decrease in purchase prices, largely through administrative rulemaking, during the Nixon administration. The fourth chapter discusses the changing fortunes of food stamps in the three most recent administrations, with major attention to the Reagan administration cutbacks and the role of interest groups in the rulemaking process.

The second part of the book places the case study in the larger context of legislative, administrative, and interest group behavior. Chapter 5 examines congressional involvement in administrative rulemaking. Berry's findings challenge many assumptions. Over the last 15 years, Congress has intervened more extensively in food stamp rule-making and has taken an active oversight role. The actions of many key congressional persons (Leonor Sullivan, Robert Kennedy, Jesse Helms) cannot be explained by traditional incentives. The final chapter treats the administrator's environment and administrative discretion. The chapter is especially good on detailing the rise of the hunger lobby and the impact of administrative guidelines.

In a brief epilogue, Berry notes the rise of hunger in the 1980s. He feels this is ironic, because government programs (especially food stamps) were, on balance, successful in reducing hunger in America in the 20 years before the Reagan administration budget cutbacks.

Overall, this is an excellent book. Berry teases

our curiosity on some key points. For example, he presents a fine discussion of the rule-making process and bureaucratization (pp. 77-80). But, exactly when and how did things get so formalized so fast? *Feeding Hungry People* is, however, a lucid, readable, and informative book: It would be appropriate for courses in administrative law, public policy, and public administration if the publisher makes a paperback edition available.

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A City in the Republic: Antebellum New York and the Origins of Machine Politics. By Amy Bridges. (New York: Cambridge University Press, 1984. Pp. xi + 210. \$29.95.)

The emergence of machine politics in New York and elsewhere, Amy Bridges suggests in this excellent book, was not solely the result of the ethnic conflict attendant to the massive migration into American cities in the antebellum period, for political machines antedated immigrant dominance of urban populations. Rather, this characteristic form of city political organization was the inadvertent result of the simultaneous presence of two powerful trends, one economic, the other political; one pervasive throughout the western world, the other particular in this period to the United States. The universal economic development was industrialization. The unique political one was democratization. As a consequence of democratization, and in contrast with the European experience, "the very first generation of the industrial working class had the vote in the United States. This meant that conflicts that were elsewhere social were here political" (p. 8).

This politicization of conflict connected with the fundamental alteration in the nature of work transformed the New York City polity, as Bridges demonstrates, from "mutualist" to "militant." In the earlier eighteenth century, city partisanship was not class-based; leadership in both major parties was by a business elite imbued with a communitarian ethic. Reflecting this ethic, the role of government included both economic regulation and relief for the poor pursuant to a notion of the common good. In contrast, by the mid-nineteenth century individualism and *laissez faire* were ascendant. City government became less a regulator and more a deliverer of basic services formerly made available through cooperative efforts. And the jobs created to provide these services allowed the separation of the urban from the national political system and provided the resource base for the political specialist—the boss—who came to mediate the relationship between the polity and

the economy on the basis of a partisanship rooted in a new congruence between class and ethnicity. Completing the "characteristic antagonism" with which we are now so familiar, reform then emerged in reaction to the excesses of this system.

In elucidating her major themes, Bridges provides a striking discussion of nativism as less a xenophobic phenomenon than a transitional one reflective of the political resistance among artisans to economic change and the consequences of that change for their social and political roles. Also interesting and encouraging for those who both believe in the desirability of political competition and who seek a renewed role for parties in contemporary politics is the struggle described here by both Whigs and Democrats to sustain broad-based coalitions across class lines in a highly competitive urban political environment.

This volume is less a work about New York politics than about machine politics, with New York as an illustrative example. Yet those interested in contemporary New York politics will find here such familiar themes as efforts by upstate, Albany-based Republicans to bypass city government and gain control of service and patronage and fears expressed by reformers that the city's solid mechanics (read "business") would be "driven away . . . by unjust taxation, causing Westchester and other places to thrive" (p. 138).

This is a brief yet complex and closely argued volume. The research is meticulous. Bridges is comfortable with both statistical and documentary materials and imaginatively uses and combines evidence from various sources—the census, church records, and voting roles. Bridges is obviously comfortable with quantitative techniques yet judicious in her reliance upon them. And she takes care to regularly connect her findings to the secondary literature. This volume is a significant contribution as both a work of scholarship and as an exploration of the roots of modern American urban politics.

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Union Power and American Democracy: The UAW and the Democratic Party, 1935-1972.
By Dudley W. Buffa. (Ann Arbor: University of Michigan Press, 1984. Pp. vii + 268. \$27.00.)

This book is about more prosaic matters than its title would indicate. Buffa describes how the UAW's relationship to the Michigan Democratic party has changed. Previously, the union respected a division of labor between the two

wings of the labor movement and followed a policy of noninterference in party affairs. Now the UAW dominates the internal affairs of the state party. Buffa also traces the fortunes of the Michigan Democratic party from 1935 to 1972. Much of this material is trivial except to aficionados of Michigan politics, but in an unassuming way, Buffa's party history presents a microscopic view of the strains that tore apart the New Deal coalition. Unfortunately, this is done so unobtrusively that this reading of the book threatens to give it more credit than it asks for.

Buffa argues that in 1947 the passage of anti-labor legislation at the state and federal levels piqued the interest of the UAW in the Michigan Democratic party. The UAW, however, was content to let the Michigan CIO take charge of rebuilding the party and making it a vehicle for the support of New Deal legislation. The state labor organization would represent the interests of labor to the state party. But in 1968 the UAW left the AFL-CIO and thus needed to forge its own links to the state party. In addition, the late sixties found the state party adrift and wracked with conflict. As blacks demanded more leadership positions, and policy conflicts broke out between the liberal and white working-class wings of the Michigan Democratic party, the UAW asserted itself. In 1972 the UAW dominated the state party convention, had its slate of candidates nominated for public office, and in 1973 installed a state party chairman that would do its bidding.

Buffa's view of these events is top-heavy. His argument is based on policy and personal conflicts among the state party leadership and the position the UAW leadership took in these debates. Buffa's institutional perspective on state party and labor politics may stem from his experience as assistant to the Michigan Speaker of the House and as a representative to the state party on the staff of Senator Philip Hart. Through these positions, Buffa was familiar with the labor and state party leadership, and he bases his argument on their account of events. But this aggrandizes the role of the leadership and diverts our attention from what is happening below the surface. For instance, there is little here on the problems the UAW encountered politically instructing its own membership. Buffa indicates that on broader issues than those of direct benefit to labor, "organized labor found it almost impossible to convince the members that the union leadership had any business entering the political arena" (p. 79). But he does not inquire as to how the leadership tried to overcome this condition, which limited their power and political maneuverability. When Buffa says white working-class voters in the suburbs supported George Wallace in the 1972 Democratic primary, it is as if none of these were

UAW members who supported a candidate the UAW rejected. Buffa does not examine the possibility that the closer the UAW leadership moved to the state party, the more the political gap between the leadership and membership increased. His focus on the machinations of the state party and labor leadership is too one-sided to capture this dynamic.

A less important error is his estimation of the power of a state AFL-CIO president. Buffa says Michigan AFL-CIO president August Scholle "wielded power over those below him in the union hierarchy" (p. 28) when, in fact, state AFL-CIO presidents have no authority over any labor people in their state except their immediate staff. Scholle was certainly among the more vigorous and colorful state AFL-CIO presidents, but whatever power he held was at the sufferance of affiliated local union leaders whose activity he could not direct.

Buffa succeeds in his coverage of personal and political struggles within the Michigan Democratic party. How much this enlightens us on the dynamics of union power and American democracy is still unclear.

ALAN DRAPER

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Refugee Scholars in America: Their Impact and Their Experiences. By Lewis A. Coser. (New Haven, Conn.: Yale University Press, 1984. Pp. xviii + 351. \$25.00.)

This remarkable book represents an enormous investment in detailed learning and could only have been written by someone who, like Coser, is both a highly astute sociologist and himself a product of the historical processes that the book describes. Coser's central aim is to chronicle and evaluate the impact on American intellectual culture of central European refugee intellectuals who came here during the years between Hitler's rise to power and the end of World War II. It is very much a book of intellectual history: Coser rejects so-called objective measures of intellectual influence such as citation indexes and content analyses because, he believes, "a crucial dimension gets lost in an approach that neglects human agency, the interaction of human beings in the process of intellectual exchanges" (p. xi). Instead he opts for intellectual-cum-sociological biographies of a huge number of writers in seven major fields. In psychology and psychoanalysis, he deals, among others, with the Gestalt theorists Wolfgang Kohler, Kurt Koffka, and Max Wertheimer; with psychoanalysis, Charlotte and Karl Buehler, Bruno Bettelheim, Erich Fromm, and Karen Horney. The chapter on sociology and

social thought deals with the Frankfurt School writers, the New School for Social Research, Paul Lazarsfeld, Alfred Schutz and phenomenological sociology, and Karl Wittfogel. The chapter on economics and economic history centers on Ludwig von Mises, Karl Polanyi, Jacob Marschak, Alexander Gershenkron, and various behavioral theorists. The chapter on political science and political theory has sections on Hannah Arendt, Franz Neumann, Leo Strauss, Karl Deutsch, Eric Vogelstein, and Hans Morgenthau. The literature chapter deals mainly with Hermann Broch, Thomas Mann, and Vladimir Nabokov, although there is a more general discussion of refugee writers and the problems they faced in New York and Hollywood. An especially wide-ranging chapter on the humanities deals, among other things, with Roman Jakobson, Erwin Panofsky, and other refugee art historians; such literary scholars as Erich Auerbach, Leo Spitzer, and René Wellek; historians Felix Gilbert, Hajo Holborn, Hans Rosenberg, and Paul Kristeller; and with Werner Jaeger and the impact of European refugees on American classical scholarship. The final chapter, on philosophy, is dominated by Rudolph Carnap and the Vienna Circle, Aron Gurwitsch, and Paul Tillich. This catalog (by no means complete) should indicate the breadth and texture of the book, for although Coser is not attempting definitive intellectual biographies of his subjects but rather is focusing on the particular issue of their European heritage and its American impact, he exhibits a staggering control of literature over a huge diversity of fields, and it would be unjust not to take judicious note of this impressive fact.

Like all methods of doing intellectual history, Coser's has its shortcomings. The price of his remarkable breadth is exceedingly uneven treatment of some writers, and often tantalizingly little about the effect of the intellectual's refugeehood on either his or her own intellectual evolution, or on his or her impact on American intellectual culture (e.g., the section on Leo Strauss). It is not, in short, a book from which experts in the various fields can expect to learn much new about refugee scholars in those fields, and indeed its superficiality in areas one knows well can be quite irksome. But it remains a mine of anecdotal information and a source of much enlightenment.

A more serious criticism is that there is astonishingly little in the way of synthetic assessment of the various writers and the commonality of their refugee experiences. The reader's expectations in this regard are aroused by Coser's principles of selection: He excludes French intellectuals who, by and large, returned to Europe after the war; he excludes the natural sciences and the arts; he excludes those, like Ernst Cassirer, who

died shortly after coming here, as well as refugee scholars who received their major education in America; and he concentrates almost exclusively on European scholars who had successful careers in America. Leaving aside questions that could be raised about these principles of selection (it could be argued, for instance, that European cultural influences were transmitted at a host of less visible levels), one would think that Coser would venture some generalizations deriving from the features his group shared in common. Yet there is no concluding chapter, and the 13-page introduction tells us little unexpected.

We discover that refugee scholars frequently could not satisfy their high aspirations, at least initially; that they often found the relatively low status ascribed to the professoriat in America difficult to accept, as did they the not-infrequent fact of lower positions in the status hierarchy than was the case for indigenous scholars of lesser achievement; that anti-Semitism was a problem; that younger scholars tended to adapt better; and that sometimes whole fields of achievement were not even acknowledged. We find that they were "most influential in areas of study where they filled a perceived need not previously met, or in fields in which they encountered an already established tradition to which they felt affinity" (p. 9). There is some brief discussion of Franz Neumann's sociology of types of refugee intellectuals, but the only substantive claim is that people fell into one or another group "not due to happenstance," but rather due to the "roots in the variant positions of these men and women in the social networks in which they were enmeshed and the social positions they variously occupied in America" (p. 12). This reader would have liked to see a good deal more in the way of explicit filling out of this kind of claim in the body of the work and a concluding assessment of the significance of his refugees' collective experiences.

In short, although the claim on the dust-jacket that this is a "vaulted gallery, crowded with superb word-portraits and vignettes" is amply justified by the book's contents, readers hoping for more will be disappointed.

IAN SHAPIRO

Yale University

Presidential Primaries and Nominations. By William Crotty and John S. Jackson III. (Washington, D.C.: Congressional Quarterly Press, 1985. Pp. xii + 251. \$8.95, paper.)

This overview of the process of selecting presidential nominees is largely a routine compilation

of existing literature and available empirical findings. Its primary use would seem to be as an undergraduate text. Even in that role, however, its usefulness is limited by a lack of incisiveness in analysis or writing; it provides only a basic background on the presidential nomination process as the basis for class discussions.

The book is organized in three sections totaling nine chapters. Part 1 outlines the historical succession of nomination systems through the party reforms of the 1960s and 1970s to the mid-eighties. Part 2 deals with participants in primaries/caucuses, convention delegates, candidate strategy, campaign finance, and the conventions. Part 3 is a single, brief chapter on proposals for further reform.

The book covers a large number of relevant and appropriate topics. But they are seldom fleshed out sufficiently, their relevance is often unclear or underdeveloped, and sections seldom fit cleanly together, even within chapters. Handling of normative questions is particularly weak, with arguments from both sides often followed by a statement that readers will of course want to make up their own minds based on their own values.

One general problem with the book is the frequent failure to place the analysis in an appropriate political context. This is particularly troublesome in the authors' treatment of representation. They make the standard distinction between demographic and attitudinal representativeness, but the two are analyzed separately with almost no attention to the linkage between them. There is no real discussion of the interests or demands of the various social groups mentioned, and criteria for selecting groups for attention are often unclear. Crotty and Jackson do point out that "young people in general are not a coherent group with shared long-term interests" (p. 109), but they fail to realize that explicit attention to the question of who constitutes such a meaningful group and how such groups are related to each other is necessary to decide which groups to study in the first place. The discussion of quotas for minorities and women, for example, fails to make clear why *these* were the groups within the party whose representation became politically important. The disembodied tone of the discussion becomes clear in a much shorter section on the Republicans in which reform is approached in terms of increasing access of precisely the same groups as were discussed for the Democrats. Explicit concern with attitudinal representation deals largely with ideological self-placement of delegates, which the authors note shifts from year to year. However, failure to place this in the context of the internal politics of the party—for example, such as the nature of the Carter presidency and of the Kennedy challenge to it in 1980

—makes it a rather weak (if accurate) conclusion.

Chapter 6 is a particularly disappointing discussion of voting behavior and candidate strategy. The authors deal with individual vote choice in nine pages, referring very briefly to concepts such as candidate image, ideology, and issues without defining them clearly or analyzing them in any detail. The evidence is almost entirely anecdotal. The discussion of candidate strategy is a checklist of relevant factors, the role of which is highly underdeveloped. A conclusion is that "personality and ideology have importance only relative to other factors and the field of candidates" (p. 152). The chapter ends with a comment that "we have barely touched the complexity of presidential nomination campaigning" (p. 156). The chapter should have been the central one of the book where assorted themes could be brought together and focused; instead, it is the weakest in the book.

The book tends to be less than incisive in confronting normative questions. Chapter 4, for example, which deals with primaries and caucuses as alternate delegate selection procedures, includes as comments late in the discussion that "considerable controversy rages over the issue of whether caucuses do a better job of nomination as compared to the primaries" (p. 97). Furthermore, "there are no definitive answers to these controversies. Trade-offs must be made in accepting either position. There are some advantages to mass participation and there are some advantages to elite-dominant decision making in the nominating process. The American solution is often to compromise" (p. 100). One might have hoped for analysis to focus on precisely what the controversy consists of and on what, exactly, the trade-offs are.

DAVID G. LAWRENCE

Fordham University

Problem Definition in Policy Analysis. By David Dery. (Lawrence: University Press of Kansas, 1984. Pp. xiv + 145. \$19.95.)

The recommendations that emerge from policy analysis hinge on the way the policy problem is framed in the first place. Yet the nature of policy problems is elusive. David Dery explores that mystery much as his teacher, Aaron Wildavsky, did in *Speaking Truth to Power* (Little, Brown, 1979).

His main point, like Wildavsky's, is to attack "rationalist" notions that problems are somehow "given" and analysis is confined to the selection of means to attain them. The choice of problems

in fact hinges on politics, many definitions are possible, and each implicitly points towards certain remedies. Problems and solutions thus are chosen together, not separately; "problem definition is a framework within which certain interventions are considered—and indeed defined—as solutions" (p. 5). When policies fail, the sensible course is usually to recast goals, not reform programs. "Policy analysis consists, not of replacing ineffective behavior with efficient means to the same ends, but of learning from experience what we should prefer" (p. 20).

However, the choice of problem is not entirely subjective. According to Dery's "interventionist perspective," a true *policy* problem cannot be a dilemma with no apparent answer, like urban crime or congestion. The problem must have practicable solutions, and their overall benefits for society must exceed their costs. Admittedly, this will seldom mean net benefit in Pareto terms. Policy problems are usually "complex," involving conflicting values; a political decision to serve some and hurt others must be made. But in some overall sense, a policy problem must offer "opportunities for improvement."

In this connection, Dery endorses the Berkeley view that implementation failures are not scandals to be rooted out. They arise because the proponents of programs favor one set of values, their opponents another. The route to better implementation is political learning and problem redefinition, not tougher-minded administration.

The main impediment to learning is what Dery calls the "handicapping force of organizations" (p. 117). Problem definition typically occurs "within policy-making arenas" (p. 117) dominated by bureaucracy. Agencies defend established definitions because existing programs are linked to them, while redefinition might mean new programs controlled by others. The "enlightenment model" of analysis, accordingly to which analysts lead policymakers by an educative process, is usually naive. To have influence, experts must be prepared to engage in "persuasion and contestation," not just research (p. 118).

Dery writes clearly, and occasionally with wit. He succeeds in conveying the ineffable way policy problems already look toward solutions—the sense in which "the problem is itself a model" (p. 7). Some of his discussion is illustrated through reference to youth and other social programs he has studied in Israel where he teaches. Most of the book, however, is an extended commentary on the existing literature on problem definition. It is an exercise more in criticism than research. Thus, it is broken up with references to other authors and digressions about them. This makes it difficult for the uninitiated to follow. The organization is also repetitive, with successive

chapters saying much the same thing in similar ways.

This book should be of interest to those involved in problem definition, a field it covers with authority. Readers distant from that subject, however, will probably wonder how much Dery has really added to Wildavsky's fundamental insight, that ends and means in policymaking are, in a sense, the same. An in-depth case study of problem definition and redefinition in one of the programs Dery mentions might well have taught us more.

LAWRENCE M. MEAD

New York University

Political Women: Current Roles in State and Local Government. Edited by Janet A. Flammang. (Beverly Hills, Calif.: Sage Publications, 1984. Pp. 320. \$28.00, cloth; \$14.00, paper.)

This collection is an important contribution focusing attention on women's political involvement in state and local politics as a complement to more readily available analyses of the national legislature. With a careful but persistent feminist tone throughout, the book's contributors sustain theses that women are transforming traditional political concepts, female officials have (or develop) distinctive politics, parties remain problematic for women, and there is political significance in women's initial inclusion in government (if even as tokens) and as a majority of voters at the polls.

The transformation of political concepts as a result of women's participation in state and local politics is a focus of Diane L. Fowlkes's perceptive analysis of how women's political activism is based on "extrinsic" accounts of connectedness outside the political actor, as well as traditional "intrinsic" definitions locating agency internally. Marianne Githens provides an interesting assessment of how differential background factors—such as socialization and situational constraints—affect the legislative performance of women in state politics, thereby introducing new sets of policy concerns. Martha A. Ackelsberg illuminates how the traditional split between the politics of work and the politics of community is challenged by women's issues, which necessarily require an integration of both spheres. Edmond Constantini and Julie Dairs Bell present provocative findings about male/female motivational differences in the context of state legislative politics and public policy formation.

The development of distinctive politics and policies in response to women's issues is illustrated by Anne Wurr's unusually fine case study

about how women in Santa Clara County, California organized a Support Network, educated establishment police officials, and eventually instituted policies at the local level to ensure greater police effectiveness in protecting women from domestic violence. Denise Antolini argues that local female officials offer perspectives to the policymaking process directly derivative of a growing liberal and feminist orientation.

The role of political parties and the difficulties faced by women to mobilize this resource for their political ambitions is discussed by Flammang, who evaluates how women's groups perform party-like functions at the local, grass-root level, which in turn critically assist development of public policies in legislatures at the state level. How female officials develop successful political strategies outside traditional sources of party support is presented in Carol Mueller's excellent analysis of collective strategies employed by the now "critical mass" of women in state legislatures, thereby illuminating intricacies of developing and sustaining new political coalitions by and for women. Janet Clark et al. present fascinating data on the electoral prospects of women candidates in different electoral environments and in relation to measures of voter hostility and incumbency.

The significance of women breaking through political barriers is impressively documented by Beverly B. Cook in an examination of the state and local court system in relation to women's access to the judicial branch of government. David B. Hill considers the thesis that the relatively smaller number of women in state senates can nevertheless be disproportionately effective as cue givers to others in the policy formation process. Emily Stoper provides an excellent overview of state and local policies in relation to national politics on the subjects of abortion, child care, divorce, and welfare—or, the politics of motherhood—pointing to the unique problems and solutions necessarily of concern to women. Ellen Boneparth contributes an insightful overview of the relation and significance of state and local politics to the national level in an age claiming the value of "dual federalism."

As can be discerned from this brief synopsis, this volume covers a wide range of topics. One cautionary note is in order concerning the organization of such diverse contributions. Sections do not necessarily correspond to stated themes and sometimes are misleading: The section, "Female Officials in State and Local Governments," in fact, does not contain any chapters dealing with local government (only state). More important, although chapters exhibit individual strengths, systematic conceptualization relating chapters to each other, and a formal

assessment of distinctive features of state versus local levels of government in relation to women's issues and women's participation in elected elites, remain largely undeveloped. However, with these reservations aside, *Political Women* is recommended as a long-overdue set of readings filling an important literature gap.

EILEEN L. McDONAGH

Northeastern University

The Costs of Federalism: Essays in Honor of James W. Fesler. Edited by Robert T. Golembiewski and Aaron Wildavsky. (New Brunswick, N.J.: Transaction Books, 1984. Pp. 330. \$29.95.)

This *festschrift* for James W. Fesler provides a wide variety of perspectives on federalism, broadly defined. As with so many contemporary essay collections, there is no single theme or conceptual framework to unite these analyses. Although the book's title implies a focus on the "costs" of federalism, none of the essays addresses this issue explicitly. Some of the essays do discuss consequences which their authors view as costs, yet there is an equal interest in federalism's benefits, and some essays treat federalism as a dependent variable in considering the conditions that make it possible. Nor do the essays assume a single conceptualization of federalism. In his introductory essay, Wildavsky offers an umbrella definition of federalism as "the diverse organizational elements of modern pluralist democracy" (p. 4) which is necessary to encompass the variety of subjects discussed. The essays range from Nelson Polsby's analysis of the prospects for American pluralism, which does not (except in the essay's title) mention federal structure, to Fred Greenstein's essay on President Eisenhower's views on administrative delegation, to Carolyn J. Tuohy and Robert G. Evans' discussion of decentralized health planning in Ontario. Although there is no unifying theme in this volume, these original essays are of high quality and taken together or individually will be of interest to political scientists with divergent concerns.

The weaker of these essays are in a section titled "A Plurality of Conceptions" (pp. 21-69), which includes the Polsby essay mentioned above and contributions from Theodore Lowi and Wildavsky. Lowi argues that the historical absence of a socialist movement in the United States is largely a consequence of the federal structure, but his claim to the originality of this thesis is undermined by his own quotations from Madison's *Federalist*

#10. Wildavsky's essay, "Federalism Means Inequality," is full of intriguing insight but suffers from a fuzzy conceptualization of equality defined largely through the familiar distinction between equality of "opportunity" and "result." He fails to explain why his conclusion that "federalism and equality of result cannot coexist" (p. 68) applies only to "result." Federalism's role as a support for segregation in the South suggests otherwise.

The best of the volume's essays offer empirical descriptions of centralization/decentralization issues in contemporary regimes. Two describe recent developments in American intergovernmental relations: First, Donald Kettl offers a model of the segmentation of intergovernmental politics between congressional preoccupation with distribution, issue network dominance of substantive ends, and third-party dominance, often in the form of nonprofit organizations, of service delivery. Second, David Caputo traces the impact on American cities of first the expansion then the reduction of federal aid to the cities. In an essay on campaign finance reforms, Herbert Alexander argues that recent efforts by both the Republican and Democratic parties to increase centralized fund raising through the party organization are not likely to alter significantly the highly decentralized character of campaign funding sources. A comparative perspective on these American developments can be found in Alfred Diamant's elegant description of the French Socialist government's program of administrative decentralization. He says these reforms, although grounded in incremental evolution in the postwar period, are dependent on "continued Socialist political control at all levels of government" (p. 162), an increasingly problematic situation.

Rounding out the book are three proposals for reform: One is Garry Brewer's proposal for "termination" as the missing ingredient in the recent industrial policy debate. Second, Golembiewski suggests a comprehensive shift from functional to areal organization in the federal bureaucracy. And, finally, James E. Swiss offers practical recommendations for improving administrative efficiency in the food stamp and AFDC programs.

In sum, the diversity of topics discussed in this book is its virtue, and perhaps a reflection of the man in whose honor it was written. In his concluding essay, Golembiewski emphasizes Fesler's ability as a teacher to "seek value from multiple orientations and perspectives" and to "encourage us to follow our methods of choice, while inspiring us with his standard of excellence" (p. 299). These abilities have clearly born fruit in the work of his students presented in this volume demonstrating that diversity, a principle benefit of

federalism, has also been a benefit of Fesler's legacy to political science.

WILLIAM E. HUDSON

Providence College

Knowledge and Discretion in Government Regulation. By Ted Greenwood. (New York: Praeger Publishers, 1984. Pp. xii + 283. \$34.95.)

Trained originally as a physicist, Greenwood has specialized in science and technology policy during his career in political science. He therefore brings an interesting perspective to government regulation concerning risk assessment of health or safety standards.

This book is based in part on interviews with "numerous" public and private officials focused on the EPA hazardous air pollutant program and the OSHA occupational health program. It is a revision of a 1981 report conducted under contract with the Office of Science and Technology Policy. The various chapter notes indicate a substantial review of the literature, relevant statutes or regulations, and major court cases. Therefore, the book is clearly an interesting and distinctive contribution to the science policy literature and helps extend that literature to issues of government regulation.

Its objective is, by "studying the interaction" between knowledge and discretion, to generate "a set of observations and an analytical framework useful for understanding and analyzing any public policy arena where science or engineering knowledge plays an important role" (p. vii). A central issue is that "the boundary between knowledge and discretion in regulation is fuzzy and their interaction is complex" (p. 221).

Knowledge means the state of scientific and engineering theory or information concerning a standard and what potential risks will be involved. "The central role of scientific and engineering knowledge is one of the most prominent features of environmental, health, and safety regulation and sets it apart from many other areas of social policy" (p. 273). More critically, this central role is greatly affected by "conflicting interpretations of deficient knowledge" (p. 2). Scientific knowledge is neither complete nor reliable. Nevertheless Greenwood concludes that federal agencies are generally competent in assembling and applying available scientific knowledge to final standards. "Final action by regulatory agencies is usually well grounded on scientific and engineering knowledge" (p. 273). The degree of success is affected by such factors as agency personnel,

structure, and procedures—but these factors are controllable.

The crucial problem lies in agency discretion (granted statutorily by Congress) to set standards based on administrative judgment. The book is therefore directed at "the nature of administrative discretion, how it is exercised, and how and to what extent it is constrained by knowledge" (p. 3). Values, policy orientations, and political considerations have a major role to play, because discretion must necessarily supplement deficient knowledge. "The most important causes of controversy in the regulatory arena are the exercise of discretion and procedural inadequacies" (p. 273). There are at least four dimensions to discretion: the ability to interpret statutory language, the right to balance conflicting values, the freedom to determine priorities, and the need to answer scientific issues.

The book contains valuable information on the two programs studied, and Greenwood's insights into the policy process are useful. However I found the presentation is to be unbalanced. The information and insights seemed to be subordinated to the author's clearly expressed goal of creating a general analytical framework capable of application to other policy arenas dependent on scientific information. The general framework overwhelmed the two policy areas studied. As a result, I found much of the presentation stilted. There is too much exposition of highly abstract arguments. I do not mean to be critical of the basic research involved: Much of Max Weber's most seminal work in sociology could be described in exactly the same terms. But the book is basic rather than applied research, despite its empirical foundation and policy orientation. In this sense, the book strikes me as unbalanced.

DUANE WINDSOR

Rice University

Presidents, Politics, and Policy. By Erwin C. Hargrove and Michael Nelson. (Baltimore: Johns Hopkins University Press, 1984. Pp. xii + 288. \$25.00, cloth; New York: Alfred A. Knopf, 1984. \$11.95, paper.)

Scholars of the presidency have viewed presidential influence as a reflection of a wide and quite varied range of factors: his constitutional powers, abilities as a strategic bargainer, personality, organizational skills, and popularity with the public, to list only a few. But few attempts have been undertaken to integrate these disparate insights into a coherent account of the modern presidency. Hargrove and Nelson attempt

this task in *Presidents, Politics, and Policy*; by and large they succeed.

Although Hargrove and Nelson do not present any new empirical research, they do offer a well-crafted critique and synthesis of relevant literature. Moreover, their perspective, analysis, and conclusions are thoughtful and illuminating. They begin by pointing out that most empirical accounts of the presidency are incorrect in describing its power as wholly weak or strong; from a normative perspective, the office cannot be assessed solely in a positive or negative light. Thus, the presidency should not be viewed either as "savior" (strong empirically, positive normatively), "satan" (strong empirically, negative normatively), "samson" (weak empirically, positive normatively), or "seraph" (weak empirically, negative normatively).

The basic thesis of the book is that presidential influence, properly understood, is multistranded and context bound. Rather than select one factor as determinative, Hargrove and Nelson argue that constitutional powers, political culture, and leadership skills must all be considered in analyzing presidential politics. Furthermore, they argue that a proper understanding of presidential influence should be historically grounded: Different historical periods call for different kinds of presidencies. A cycle of presidential influence exists, with particular types of influence appropriate to different periods in the cycle. Presidencies of "preparation" (Theodore Roosevelt, Kennedy, and Carter) anticipate new periods in American politics. They are followed by presidencies of "achievement" (Wilson, Franklin Roosevelt, Johnson, and Reagan), in which bursts of creative energy alter the bounds of public policy. Finally come presidencies of "consolidation" (Hoover, Eisenhower, and Nixon), in which reform is stabilized. Not all presidents meet the needs of the times. Presidencies of "stasis" (Ford), characterized by drift and confusion, can emerge. Incumbents also can fail to exercise influence in effective and proper ways: the "flawed" presidencies of Johnson and Nixon.

Successive chapters illustrate the impact of this cycle in situations where exercises of influence are crucial: the selection process, management of policy formulation, executive-congressional relationships, and management of policy implementation. With respect to relationships with Congress, for example, Hargrove and Nelson point out that presidencies of preparation need well-honed rhetorical skills, whereas presidencies of achievement require skills of bargaining and an ability to form political coalitions. Less satisfying is the authors' contention that tactical skills are largely irrelevant to presidencies of consolidation (p. 229). As the recent revisionist literature on

Eisenhower indicates, tactical skills are not irrelevant as much as they need to be more covert, selective, and indirect. Virtually no attention is given to how politics varies within a particular historical period, and thus how presidential influence must be tailored to the differing array of political forces the president faces in different policy areas.

What forces "drive" this historical cycle through its different stages especially requires further analysis. Hargrove and Nelson do contend that elections are the "engine," ideas the "fuel," and leadership the "carburetor." But this is rather sketchy mechanics; much more is needed by way of structural or causal analysis. Furthermore, the placement of particular presidencies in particular segments of the cycle could benefit from more critical discussion. Carter, for example, is classified as a president of preparation, whereas Ford is viewed as a failure. Other scholars such as James David Barber, Bert Rockman, and Stephen Skowronek have toyed with the notion of historical cycles, but with markedly different results. Skowronek, for example, proposes a cycle of regime construction, regime management, and regime enervation, with the Carter presidency now viewed as an enervated regime and Kennedy labeled as a regime manager. The authors and their editors also must be faulted for that most grievous of sins committable by scholars of the modern presidency: adding a period after the middle initial in Harry S Truman.

But that said, the work is indeed rich in original insights and observations about the presidency as well as in basic fact and detail. As such, it makes a welcome contribution to contemporary scholarship, as well as a thought-provoking discussion that is readily accessible to students.

JOHN P. BURKE

The University of Vermont

The Government/Press Connection: Press Officers and Their Offices. By Stephen Hess. (Washington, D.C.: Brookings Institution, 1984. Pp. xv + 160. \$22.95, cloth; \$8.95, paper.)

After spending a year inside the press offices of five federal agencies in Washington (the White House, State Department, Pentagon, Department of Transportation, and Food and Drug Administration), Stephen Hess presents his experiential findings in this compact but revealing volume.

A major portion of Hess's work is devoted to a discussion of the characteristics and functions of press secretaries and press officers. He finds little

real conflict in loyalties between the press secretaries, who are political appointees, and the subunit press officers, who are career personnel. Instead, he reports that most of the infighting he witnessed involved political appointees within an agency and notes that career officers had a real interest in bureaucratic politics but rarely in partisan battles.

Not surprising is the notion that reporters believe that access to an agency head is what makes a press secretary a good one. To be in the inner circle ("the loop," in Washington parlance) would seem to be essential for effective and honest press secretary performance. Hess notes, however, that it is common practice in government for agency heads to keep their press spokesmen uninformed in order to protect them from having to lie.

One of the more interesting features of this volume is the contrast in media activity it draws between the White House and other executive agencies. The typical government agency initiates very few "events," whereas the White House is the center of media activity. Significantly, the White House press office is oriented toward television, while other agencies (State, Defense, etc.) are essentially "print beats" (p. 103). During the Reagan administration, this fact has been underscored heavily with the administration often advancing a particular "line of the day," which the president's publicity team pushes and which often becomes the focus of evening network news shows.

Because Hess's experience was centered from 1981 to 1982, his comments about the Reagan administration carry particular relevance. Hess takes an unconventional view of the administration's decision to bar the press from the island of Grenada during the U.S. invasion in the fall of 1983. He writes that the impact of the administration's policy "redirected the press to get its news from Radio Havana, amateur radio operators, rumor mills, and political opponents. . . ." and concludes that "the price paid by the nation for the administration's press policy . . . was less accurate news. The price paid by the administration was fewer favorable stories" (p. 60).

Another painful lesson learned by the Reagan administration as well as its predecessors is that leaks—and Hess distinguishes among six different varieties—not only can be quite damaging and irritating to the president but also tend to emanate from the administration's own political appointees, especially perhaps from the most ideologically oriented among them.

During the first Reagan term, the White House became so upset over "leaks" that it announced that any unauthorized disclosures would result in officials being investigated, and that the investiga-

tions would "include the use of all legal methods" (p. 90).

Despite the occasional furor over leaks, Hess makes two telling points. First, the government is quite adept at keeping secret what it really wishes; second, it might not always be in the president's interest to stop leaks because many are in his favor.

One additional aspect of Hess's work is worthy of mention. The charge is often made that government press offices manage, manipulate, and control the news. Hess argues, however, that such offices are neither skillful enough nor large enough to accomplish such difficult goals and that instead they spend much of their time and energy at such basic tasks as collecting material requested by reporters and attempting to learn what their entire agency is doing.

It should be noted that Hess's press office observations were confined to the early Reagan administration rather than spanning two or more administrations or even the entire duration of one presidency. A comparative analysis might have been helpful here, although Hess's findings (which, he indicates, were tested through interviews with reporters and civil servants from previous administrations) seem likely to stand. Although greater elaboration of some of his points and more frequent reference to other administrations would have been valuable, Hess has produced a useful and occasionally provocative contribution to the growing but still inadequate body of literature that focuses on the symbiotic relationship between government and the press.

ROBERT E. GILBERT

Northeastern University

The Industrial Policy Debate. Edited by Chalmers Johnson. (San Francisco: Institute for Contemporary Studies, 1984. Pp. x + 275. \$21.95. cloth; \$8.95, paper.)

The 10 essays edited by Chalmers Johnson in this book do not represent, as the title might suggest, a cross-section of differing views on the design and implementation of industrial policy in the United States. Rather, they cohere around the single perspective described by Johnson as "recognition that the government-business relationship in the United States has become a serious, costly drag on American economic competitiveness in the world" (p. 3). The results overall are the expression of considerable suspicion about government's ability to produce and sustain policies that have positive consequences for American economic competitiveness and a very narrow rationale for why any industrial

policy should even be attempted at all. Consequently, I found it difficult to agree with Johnson's characterization of his colleagues' work as "a broadly based, non-partisan discussion" of a controversial and important topic.

A fairly broad range of particular issues in industrial policy is addressed, however, albeit with varying degrees of success. In addition to Johnson's introductory chapter, "The Idea of Industrial Policy," essays by Eugene Bardach on "Implementing Industrial Policy," Regis McKenna on "Sustaining the Innovation Process in America," and Paul Seabury on "Industrial Policy and National Defense" offer the reader the most stimulating and useful insights at both the conceptual and practical levels. I would especially cite Bardach's discussion of the political pressures likely to be faced by a hypothetical "Agency for Industrial Policy" and its subsidiary "Bank" for industrial loans as the book's most articulate challenge to advocates of an industrial policy managed by a pro-active, interventionist government.

On the other hand, Melvyn Krauss's "'Euro-peanizing' the U.S. Economy" and Bruce Bartlett's "Trade Policy and the Dangers of Protectionism" are the most problematic expressions of the book's partisan biases. Bartlett's argument, directed against those calling for government protection of disadvantaged or weak domestic industries, is that "the trade deficit is really a meaningless concept" (p. 167). The argument is neither well documented nor sensible in the context of what is practiced by America's foreign competitors—especially protectionist Japan.

Even more questionable is Krauss's loose interpretation of historical facts to try and prove that current "neoliberal" industrial policy—as proposed by Robert B. Reich, Lester Thurow, and Felix Rohatyn—is a new form of corporatism that Krauss labels "fashionable fascism" (p. 72). There is, he asserts, "an explicit institutional parallel linking the neoliberal version of industrial policy with the New Deal and the Mussolini era," primarily through Felix Rohatyn's proposal to establish a new Reconstruction Finance Corporation modeled on "the early Roosevelt version." According to Krauss, "the original RFC was inspired by Mussolini's Istituto per la Ricostruzione Industriale . . . established in January 1933 to combat the severe Italian recession" (p. 72). Krauss does not mention that the *original* RFC was created in 1932 by Herbert Hoover to combat the worsening American depression that began during his administration. What this might mean for the issue of who actually inspired what in the evolution of industrial policy is a question that Krauss does not pursue.

It is unfortunate that *The Industrial Policy*

Debate has no strong statement of the neoliberal position or any other position on industrial policy that could take issue with Bartlett, Krauss, and others in the book who argue that the best industrial policy may be no industrial policy at all. National defense considerations and tax reform opportunities are the sole positive arguments made for attempting industrial policy, and this amounts to a timid debate indeed. Johnson, who avows his support for the development of an American industrial policy, is left to conclude from his colleagues' contributions that industrial policy is only "a viewpoint, one similar to that of the coach of a football team who has to work both on fundamentals in training his men and on producing a game plan for each individual contest" (p. 241). It is a weak characterization of a complex idea that reflects the limitations of this book more than the idea itself.

DAVID MENNINGER

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Judicial Policies: Implementation and Impact. By Charles A. Johnson and Bradley C. Canon. (Washington: Congressional Quarterly Press, 1984. Pp. xii + 288. \$10.95, paper.)

This synthesis of research on judicial impact successfully succeeds Stephen L. Wasby's bellwether of 1970, *The Impact of the United States Supreme Court* (Dorsey Press). More, the compendium succeeds on three levels: as introduction for advanced undergraduates, as aid for graduate field examinations, and as assessment for practitioners.

Johnson and Canon recount the results of investigations of four populations whose responses to judicial decisions might be distinct. The findings indicate an interesting interplay of attitudes and behaviors based on general policy and partisan orientations. The interpreting population (primarily lower courts within the jurisdiction of the policymaker) is likely to heed the call of duty and professionalism when issues elicit no intense policy preferences from the interpreters. Outside this assumed "zone of indifference," however, the views of the interpreters are likely to skew interpretations. Similarly, responses among the implementing populations (officials who enforce the policy) are shaped by commitments to existing programs, the desire to conserve resources for agency-defined goals, the preferences of their communities, feedback from targets of enforcement, and continued pressure by courts. Those directly affected by a particular decision (the consumer population) naturally tend to respond according to their perceptions of the costs of com-

pliance and their attitudes. This finding suggests that the zone of indifference is attenuated for the consumer. For the secondary population (a residual category), attitudes toward policies seem to be the major factors in responses to the courts, although the authors admit that research on the secondary population is too scattered to warrant more than speculative conclusions (p. 140).

Interspersed in this distillation of what is known are notations of what is not. Students of the interpreting population have stressed the behaviors of judges, but can only infer attitudes because the direct study of judicial attitudes is so difficult (pp. 48, 66-70). Scholarly attention to the consumption of judicial policies seems to the authors too minimal to support very broad conclusions (p. 108). Among the least understood secondary publics are state legislatures and interest groups (pp. 155-159, 163-170). We learn that there is little evidence of the impact of mass media on the reception and understanding of courts' decisions (p. 175). Extensive survey research has led us to few conclusions regarding the importance of mass and attentive publics' responses to decisions and policies (p. 178).

Johnson and Canon conclude with a discussion of theories in the subfield. The upshot of the discussion is that many theories in the field have not been tested outside of very narrow cases, many theories have not been tested against one another, and indeed, many theories cannot be tested against one another very practically. The authors' efforts to systematize inquiry in the field recall the similarly laudable effort by Wasby in his synthesis to get scholars to speak to theory and to one another. Here Johnson and Canon may have been less willing to depart from the findings of research than some readers would prefer. Wasby admonished practitioners to reexamine their goals and their conceptual and theoretic tools for achieving the goals. Johnson and Canon have nicely synopsized the results of inquiry, but have chosen not to draw inferences for method. They have deftly (and, it seems to me, devastatingly) charted the theoretic progress to date (p. 222), but have not spun out all of the hypotheses and insights implicit in their own framework (which, to be fair, they regard as merely heuristic). For example, they use the "zone of indifference" to make sense of the mix of legal and political responses to judicial decisions in the different populations without pausing to consider how sensible this zone is for each. For the consumer population, the zone of indifference must be very narrow and probably insignificant. In contrast, it may be quite broad for the secondary population. For the interpreting and implementing populations the zone may be variable, and the more reasonable assumption may be a "zone of am-

bivalence" in which members of the populations are cross-pressured by legal and political attitudes or torn between dissonant values.

So, although some readers may long for what Johnson and Canon did not do and did not promise to provide, this guide to the subfield will rekindle the thirst for theory. On that ground alone this is a worthy contribution to scholarship.

WILLIAM HALTOM

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Mass Media and Elections. By Richard Joslyn. (Reading, Mass.: Addison Wesley Publishing, 1984. Pp. xii + 313. \$15.95, paper.)

This volume fills a gap in the literature on mass media and politics. As is always the case in a growth subfield, at certain points there will be a need for taking stock, synthesis, and appraisal of our knowledge. Joslyn reviews and highlights a broad range of scholarly and practical literature in the field of voting behavior, public opinion, and mass communication in an effort to identify the major ways in which mass media influence presidential elections. This is a tall order indeed given the different disciplines, forms of data, methodologies, and purposes that abound in the media and politics field. No author assaying the task can expect to find a tidy framework that will adequately order our knowledge.

Joslyn posits three themes around which his discussion is organized. The first of these is that the media have transformed the electoral process. The second theme is that it is the interactions among several types of participants (candidates, managers, journalists, citizens) that shape campaign communication. The third of the themes is that electoral communications have demonstrable effects upon voter attitudes and behavior. Obviously such themes have a commonplace quality, but the simplicity is deceptive, as Joslyn's intelligent and judicious treatment of sub-themes and scholarly disputes demonstrates.

Joslyn is eclectic in his approach to evidence, as can be seen best in his chapters on campaign communication, in which he employs excerpts from television spots, strategy memos from consultants, national survey data, an Arbitron media market map, and campaign spending figures. For the most part, Joslyn is able to maintain continuity in his treatment of the diverse material.

The middle portion of the book is a mildly critical overview of how journalists cover campaigns and what the content of that coverage is. Joslyn's notion of how the campaign press corps covers a presidential race seems to be based heav-

ily upon the experiences of 1972 and 1976 (hence several citations of Timothy Crouse's ideas on pack journalism). The 1980 and 1984 elections have already led to a revision of the 1970s' conventional wisdom on this phenomenon, however, and some observers of the process have begun to argue that the self-awareness of the press has led to the demise of the pack approach. This reservation aside, Joslyn does a creditable job of conveying a sense of how American journalists go about the business of deciding what is news.

In the final portion of the volume, Joslyn devotes three substantial chapters to a splendid assessment of the impacts that various forms of campaign communication such as hard news, debates, and television spots have on public opinion and electoral behavior. Although there is nothing new in the way of data in these chapters, they are more than ordinary literature reviews. Joslyn skillfully synthesizes two decades of empirical research, abundant concrete examples from recent elections, and thoughtful interpretations of it all. Anyone needing a good introduction or refresher on the effects literature could hardly do better than *Mass Media and Elections*.

The book concludes with the obligatory chapter on implications for the democratic electoral process. Here Joslyn suggests several possible conceptualizations of elections as media events including (among others) elections as occasions for prospective policy choice and elections as essentially empty rituals of symbolic reassurance.

Apart from the inevitable problems associated with writing a broadly focused synthetic volume, there is another deficiency stemming from the fact that Joslyn has chosen to ignore most of the scholarship on media in sub-presidential elections, except insofar as a specific piece of work might have an idea that is relevant to presidential elections. A chapter explicitly devoted to media in state-wide, congressional, and local elections could have proven to be a useful vehicle for the encouragement of comparative analysis.

This is on the whole a very solid piece of work. It has a good deal of adoption potential in courses on mass media and in courses on the electoral process that allow for a serious consideration of communications. Furthermore, for at least several years, it is likely to be an unusually handy quick reference for scholars needing a concise picture of the state of our knowledge on media and presidential election campaigns.

CHARLES M. TIDMARCH

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The Political Presidency: Practice of Leadership from Kennedy through Reagan. By Barbara Kellerman. (New York: Oxford University Press, 1984. Pp. xii + 300. \$22.50.)

In the Preface of her book, Kellerman writes that it is not intended to be the definitive analysis of presidential leadership, and she is clearly correct. The study of presidential leadership has taken off in the 1980s, with no fewer than six scholarly titles appearing and two presidential textbooks with a leadership theme coming into print. *The Political Presidency* makes a useful contribution to this burgeoning subject area.

The book is divided into two major parts: The first five chapters are devoted to a theory of presidential leadership; the second section covers the practice of presidential leadership with separate chapters covering the presidents from Kennedy through Reagan. A short epilogue allows Kellerman to tie the work together and to restate her judgment that Lyndon Johnson and Ronald Reagan have been the nation's best political presidents and the most effective directive leaders of the past 25 years.

Although conceding that presidential leadership in America is difficult because of our constitutional system and our anti-authority belief system, Kellerman believes that leadership, or the process of influence between leader and followers, is possible. It is her view that "the political leader who can coax followership through shared rewards stands the greatest chance of success" (p. 32). To be a politically skillful executive, a person must balance both outside societal forces and internal psychological factors in order to achieve stated policy goals.

No psychological factor is as important to Kellerman as the degree to which a president is extroverted. Introverted presidents, and Nixon and Carter are so identified, will find leadership exceedingly difficult in the American cultural context. Extroversion alone is not enough to become a successful leader, as witnessed by Ford, because a president also must display an adeptness at presidential politicking with political elites. For Kellerman, the political president combines an extroverted personality with an inner-directed determination to accomplish his goals.

To test her theory of presidential leadership, the author chooses to examine the major noncrisis domestic agenda items of the six chief executives under consideration. She openly rejects George Edwards's aggregate success level of influence measurement in his *Presidential Influence in Congress* (W.H. Freeman, 1980) in favor of an approach to study a president's attempt at influence on the single issue of greatest importance to him.

Six chapters provide a case study analysis of

how presidents exhibited their political skills (or lack of them) in pursuing a key policy objective. The case studies cover Kennedy and federal aid to education, Johnson and the war on poverty, Nixon and the family assistance plan, Ford and the tax cut, Carter and energy, and Reagan and budget cuts. Although Kellerman is precise in explaining her selection criteria of particular policies, one difference shows up between Kellerman's case studies and what Paul Light found in *The President's Agenda* (Johns Hopkins University Press, 1982), where Gerald Ford's primary domestic concern was energy.

In constructing her case studies, Kellerman relies heavily on secondary sources with some archival research. What saves several of these chapters from bordering on the too-often-told tale is Kellerman's inclusion of a concluding section, "The Practice of Leadership," for each president and policy goal. It is in these sections that specific insights into presidential policymaking are made. For example, Reagan's success as a political leader is attributed to his natural political instincts along with his uncanny tactical skill, whereas Carter's failure was rooted in his inability to understand the link between politicking and leadership in American national politics.

The overall value of *The Political Presidency* depends to a large degree on the nature of the reader. Kellerman has explored general psychological leadership theory and applied it effectively to a theory of presidential leadership. The book is of clear value as supplementary reading assignment for courses on American politics and the presidency, but for individuals seeking a theoretical high in the presidential leadership area the recent works of Bert Rockman, Paul Light, and John Kingdom will be more to their liking.

ROBERT LOCANDER

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Presidential Parties. By John H. Kessel. (Homewood, Ill.: Dorsey Press, 1984. Pp. ix + 645. \$22.95.)

Presidential Parties is an excellent analysis of American national politics. John Kessel provides the reader with a unique way of looking at national party activity. Three factors are especially important: the temporal context, internal structure, and external structure.

Kessel explains the ebb and flow of presidential, congressional, nomination, and campaign politics. Each of these has a time component during which certain activities occur sequentially. The temporal factor provides understanding of what presidential candidates, national legislators, and

chief executives do within allotted times for action. Most political studies ignore this factor.

The book also provides useful insights on the internal structures of presidential, legislative, nomination, and electoral politics. Kessel applies statistical methods to show how some groups are more complex and cohesive than others. Internal considerations are linked with external factors whereby presidents establish congressional and media liaison, legislators formulate new policy, presidential candidates seek primary votes, and presidential nominees develop campaign staffs and operations.

There are two excellent chapters on the two-candidate campaigns of 1972 and 1976 and the three-candidate contests of 1968 and 1980. Kessel synthesizes nearly all of the available research on the five presidential contests and shows how the various contenders developed strategies and tactics for putting together electoral coalitions.

Kessel is also concerned with how the voters respond to presidential candidates. Over the last three decades, levels of citizen information have remained modest, and strong partisan attachments have declined while the number of independents has increased. The most unusual finding is that issues have been important in all presidential elections since the 1950s. Voters apparently weigh how well the economy is doing (which usually favors Democrats) with international issues (which usually favor Republicans). Other complicating factors are involved in voter choice, but Kessel argues that voters want and need information from presidential candidates. They aren't getting it from television reporting. Even the print media has reduced the volume of issue-oriented stories in recent elections. This trend continued in 1984 when the mass media focused more on the horse race aspects of the campaign.

In a concluding chapter, Kessel effectively draws together the key findings and interrelationships developed earlier.

Presidential Parties is a welcome addition to the study of national parties and presidential politics. It is the most effective comparative analysis of recent presidential campaigns and voter response currently available.

ALAN SHANK

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Macroeconomics and Micropolitics. By D. Roderick Kiewiet. (Chicago: University of Chicago Press, 1983. Pp. 176. \$15.00, cloth; \$5.00, paper.)

For years, political scientists, politicians, and journalists have cavalierly said that "Americans

vote their pocketbooks." This generalization appeared to be a happy marriage of political common sense and empirical political science. Indeed, research by Gerald Kramer and others on economic cycles and voting patterns suggested that voters "give greater support to the incumbents during periods of prosperity, but turn against them . . . when times are poor" (p. 6). The 1984 presidential election is an illustration of the pocketbook thesis. Most political scientists and journalists projected a Reagan victory given the state of economic recovery. However, as Kiewiet's work clearly demonstrates, the economic theory of voting, so broadly stated, is too general and ambiguous to account for voter behavior. At the micro level, there are several paths to pocketbook voting.

The basic framework for Kiewiet's analysis is premised on two distinctions. The first is the recognition that voting one's pocketbook can result either from rewarding or punishing incumbents for current economic conditions or can be the result of policy-oriented voting. Policy-oriented voting is based on voters' perceptions of party differences on handling the problems of unemployment and inflation. In the first case, voters lash out at whomever occupies the White House. In the second, voters move toward one party or the other depending upon which economic problem is most pressing. The second distinction is that voters may react to their own economic circumstances or to general economic conditions. Perhaps the most important point that *Macroeconomics and Micropolitics* makes (and documents) is that "it is perceptions of national conditions and events which most heavily influence voting behavior" (p. 21). Kiewiet argues that many voters do not connect their personal economic situation with national trends. These two distinctions produce four types of pocketbook voting: incumbent-personal economic situation, incumbent-general economy, policy-personal, and policy-general.

Kiewiet's basic research strategy is to use panel survey data for the years 1956 to 1960 and 1972 to 1976 to confront the four types of pocketbook voting. The use of survey data is important because many individual behaviors are consistent with the aggregate level findings. The panel data enables Kiewiet to use the respondents' vote in the previous election as a "temporally prior baseline against which to measure changes induced by the particular short-term factors of interest" (p. 28). (Kiewiet also uses cross-sectional survey data for the years in which panel data are not available employing partisan identification as the baseline.)

The basic findings of the study are these: (1) pocketbook (of whatever variety) voting is stronger at the individual level in presidential as

opposed to congressional elections; (2) national assessments have greater electoral consequences than personal; and (3) incumbency-oriented voting has a greater effect than policy.

The student of voting behavior thus comes away from this compact volume with the sense that national presidential elections may be considered as referenda on presidential performance with respect to general economic conditions. To this reader, the study would seem to strongly support Morris Fiorina's theory of retrospective voting.

The predictive utility of *Macroeconomics and Micropolitics* was given a test in the 1984 presidential election. While general economic conditions had improved, especially with respect to inflation, unemployment was still relatively high. In the near term, Kiewiet argued that getting inflation down would be the dominant indicator of the national economic conditions. With inflation under control, an incumbent president should have been, and was, reelected. This is one instance where a highly quantitative work of scholarship was right on target.

Macroeconomics and Micropolitics would be most appropriate for graduate level courses in voting behavior. It is an exemplar for its sophisticated treatment of survey research.

DAVID H. EVERSON

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Promises and Performance: Presidential Campaigns as Policy Predictors. By Michael G. Krukones. (Lanham, Md.: University Press of America, 1984. Pp. 158. \$22.75, cloth; \$11.50, paper.)

Because of his concern about negative public attitudes toward political campaigns, Michael Krukones has undertaken this study to prove that presidential campaigns should not be regarded as futile exercises during which candidates say one thing and then act in another way once they assume office. In examining 15 campaigns from 1912 to 1976, the author concludes that successful presidential candidates kept approximately 75% of their campaign promises. Thus, campaigns are good indications of the future policy performance of presidents and provide the electorate with accurate information on the policy choices before them.

For each campaign examined, Krukones distinguishes the major campaign issues from other issues that arose in the campaign. He also gives presidents credit for "good faith" efforts to fulfill campaign promises, such as Wilson's efforts to

bring the United States into the League of Nations. Finally, he assigns each president an overall percentage rating based on the degree to which the president carried out his campaign promises.

In computing a president's rating, Krukones unfortunately gives all issues equal weight. The problem with such a mathematical analysis of all issues is that voters may recall only major issues. For example, voters may recall that Woodrow Wilson and Franklin Roosevelt promised to keep America out of war, that Lyndon Johnson promised not to enlarge the Vietnam War, that Herbert Hoover promised prosperity, and Richard Nixon promised an "open presidency." Of course, in a democracy, voters have the collective power to unseat an incumbent president if he fails to carry out his campaign promises.

In considering public opinions, Krukones does not take into account that even if a president extends a good faith effort, the voters may believe that "faith without works" is not sufficient to hold the nation's highest office. Although Krukones credits President Carter with a good faith effort to reduce unemployment and inflation, a majority of Americans voted no when in 1980 Ronald Reagan asked, "Are you better off than you were four years ago?"

Krukones admits that presidents do not always keep their promises, and that this may have an adverse effect upon voters. As an example of broken campaign promises, he admits that "the nation had been shaken by the events of Vietnam and Watergate," and, as a result, "trust in government was continuing to decline among the electorate" (p. 109). However, he still gives both Johnson and Nixon high marks for accomplishing their campaign promises (87.5 and 73.5%, respectively).

In the concluding chapter, Krukones factors major campaign issues and good faith efforts into his equation, but some of the results are most amazing. One result gives President Johnson a zero on major foreign policy issues while Richard Nixon receives a 100% rating on domestic policy. It is doubtful that political scientists and historians will agree with these findings.

It is also unfortunate that this text ends with a campaign already eight years out of date. This omission does, however, leave professors and students the opportunity to apply Krukones's theories to recent campaigns. Future studies may improve upon his methods by rating major campaign issues more heavily and not granting "good faith" efforts where they are not warranted.

Even with its limitations in methodology, this brief but well-focused text adds additional insights to one's understanding of the presidency and issues arising in modern campaigns. Although readers may disagree with some of the author's

conclusions, most political scientists share his contention that this subject deserves a rational analysis rather than being left to the whims of negative public opinions.

LARRY BOYLE

George Washington University

The Two-Party South. By Alexander P. Lamis.
(New York: Oxford University Press, 1984.
Pp. x + 317. \$25.00.)

The Two-Party South is tightly written around the premise that the politics of race was, and remains, the driving force of southern politics. According to Lamis, the national Democratic party initiatives to further racial equality led to the demise of the Solid South in presidential politics. The initiatives encouraged the somewhat uneven development of competitive Republican parties in the states of the former Confederacy during the late 1960s and early 1970s. In response to the political reality of Republican party growth, the centrality of race abated and led to biracial coalitions producing electoral success for moderate Democratic candidates in the 1970s and 1980s.

Lamis develops the racial politics/competitive two-party politics theme through a skillful and interesting combination of interviews and journalistic election analyses, both of which focus on the state-wide and, to a lesser extent, congressional two-party contests in the individual states. Although presented in each state chapter, incidental to each analysis are the graph depicting the David Composite B index of Democratic party strength and Democratic presidential vote 1932-1982 and a state county map plotting various political/demographic phenomena. For example, some state maps plot 1960 and 1970 census data (South Carolina, Florida, Mississippi, Alabama, and Georgia) rather than those for 1980; presidential (Mississippi, Arkansas, Florida, and Texas) or gubernatorial vote (Louisiana) rather than the first state-level, state-wide Republican electoral success; and simply political regions (North Carolina). Hence, the data presented are uneven, and issues of importance are neglected: Does the 1960 Florida urban horseshoe exist in 1980? Is the balloting for President Ford in Texas more important than the election of a Republican governor in 1978? Rather than note the low percentage of blacks in Texas and Florida, one might want to explore the political importance of Hispanics.

On the other hand, aggregate and limited survey data, although uneven, are brought together in the introductory chapters to supplement the explanation for the emergence and

growth of two-party politics from the 1948 Dixiecratic Revolt to the Voting Rights Act of 1965 and its aftermath. The 1960 and 1976 SRC/CPS National Election Studies are used only for partisan identification (p. 36), whereas previously published data for selected cities are used to illustrate the impact of race and class on the 1964-1972 presidential elections. A consistent use of the widely accepted SRC/CPS studies would have strengthened the presentation.

In direct contrast, the concluding chapter, "Southern Politics in the 1980s," rests on an analysis of the 1980 SRC/CPS National Election Study to determine the impact of selected demographic characteristics and political issues on partisan identification and presidential voting. Information is also brought together from individual state surveys, including a secondary analysis of those for Mississippi and Alabama in 1982.

The strength of *The Two-Party South* rests with the skillful use of interviews and journalistic election analyses to examine the impact of race on the development and maintenance of two-party politics in the South. Its weakness lies in Lamis's failure to address the transformation of southern politics through a systematic examination of the impact of the Voting Rights Act of 1965 from the standpoint of voter registration data (presented for Louisiana and Florida only) which are available by race, black electoral participation and success, and Republican party organizational growth beyond federal patronage parties and its success in state legislative and other less visible elections. The reader, then, learns little about the changing dual nature of strong state/weak national Democratic parties in the region.

The Two-Party South is interesting, readable, and useful; but it is limited by virtue of its omissions.

CHARLES D. HADLEY

University of New Orleans

Vice-Presidential Power: Advice and Influence in the White House. By Paul C. Light. (Baltimore: Johns Hopkins University Press, 1984. Pp. 278. \$19.95.)

This book's title appears to be a contradiction in terms: What power is there in the vice-presidency? Everyone has long known the weakness of "His Superfluity," as John Adams styled the office. What everyone "knows," however, is no longer true, as Paul Light demonstrates most effectively in this treatment of the contemporary vice-presidency.

Light's thesis is that the second position in American government has emerged from the relative obscurity of past times to become an office of no little substance. As he puts it in his introduction, "After two hundred years as errand-boys, political hitmen, professional mourners, and incidental White House commissioners, Vice-Presidents can now lay claim to regular access to the President and the opportunity to give advice on major decisions" (p. 1). Moreover, as Light makes clear from his analysis of Walter Mondale's tenure and the early days of the Bush vice-presidency, it is an office that also offers its occupant a chance to influence the course of presidential policymaking. Drawing on extensive interviews with current and former presidential and vice-presidential aides, Light develops his thesis through a careful analysis of the job of vice-president, the vice-presidential staff, relations with the president and White House staff, and resources and opportunities for vice-presidential influence.

As the subtitle makes clear, influence is Light's central concern. He focuses on the factors affecting vice-presidential influence over policy and presidential decisions and in doing so provides some useful insights into White House politics. Indeed, the book is as much about the internal politics of the White House as it is about the vice-presidency, with the vice-president holding center stage as a special kind of presidential advisor. Using a comparative case study method to contrast the influence of Vice-Presidents Rockefeller and Mondale, Light reveals how vice-presidents can take advantage of (or be swept away by) the ebb and flow of power in an administration. Consequently, much of what he writes about patterns of advice and influence apply to any senior presidential aide as well as to the vice-presidency.

The book is well-organized, clear, and convincing. Light has done exhaustive research, knows his subject well, and presents his case in an effective and compelling fashion. If there is a flaw to this work, it is that the author is almost guilty of overkill: The book seems almost to be over-argued. The word "almost," however, should be stressed. Although there is a great amount of information here about advice and influence (or the lack thereof) by every vice-president since Henry Wallace, there is nothing superfluous in Light's work. His task is to sway the reader from long-held beliefs about the vice-presidency, which is no easy task, so every piece of evidence and every bit of analysis is important to the argument.

Indeed, it would be helpful if the argument could be extended. Before the 1988 election comes around, Light should write a second edition to this book, which would update his analysis by incorporating the lessons and experiences of the

Bush vice-presidency. The comparison between two influential vice-presidents, Mondale and Bush, would be informative. Even without such an addition, however, this is a valuable book.

If, as George Reedy has claimed, the White House is a type of royal court, then Paul Light has written the book of the modern courtier. It should be read not only by students of the presidency and organizational politics, but by prospective vice-presidents, White House aides, and presidents as well.

RYAN J. BARILLEAUX

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The American Ethos: Public Attitudes toward Capitalism and Democracy. By Herbert McClosky and John Zaller. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xviii + 342. \$27.50.)

The American Ethos is a very intelligent and well-written book about fundamental democratic and capitalist values in the United States, and it offers an important and challenging theory about elite influence upon public opinion. The book will not only be of interest to students of public opinion and American politics, but also to political theorists, social psychologists, and historians.

McClosky and Zaller examine mass and elite opinion data from their *Opinion and Values* (1975 to 1977) and *Civil Liberties* studies (1978 to 1979) which are supplemented by McClosky's 1958 *Political Affiliations and Beliefs* study and tabulations (originally compiled by others) from Gallup, Harris, and other surveys over the last 50 years (although very little trend data are examined). Using a large number of survey questions—more than 70 items on democratic and capitalist values from the *Opinion and Values* study alone—the authors provide a concise analysis of attitudes toward freedom of religion, speech, the press, and assembly; the relatively new issues of moral freedom and unconventional lifestyles; equality of worth, dignity, rights, opportunities, and outcomes; the "Protestant ethic," individualism, ambition, achievement, self-interest, profit, and competition; and property, *laissez-faire*, and the role of government in the economy.

McClosky and Zaller's conclusion—that there is substantial support for democratic and capitalist values, but also some striking ambivalence—is not new. The ambivalence arises when abstract values are contrasted with political practice, especially in circumstances in which democratic and capitalist values come into conflict. The result

is the configuration of attitudes that provides the underpinning for the American welfare state.

What is novel is the authors' theory of public opinion change and the way in which they employ their evidence. They persuasively argue that citizens learn political norms and values from elites—formal leaders and citizens who are active in political organizations, campaigns, and the like—and this is most apparent for those most interested in politics and close to the mainstreams of opinion (p. 11). Their argument is supported by their comparisons of elite opinions with those of citizens with different levels of education and sophistication. Particularly illuminating is the fact that when there is elite consensus (i.e., substantial elite support for a position) on an issue, sophisticated citizens will be much more likely to take on the views of the elites. In the case of increases in political "tolerance," the authors' analysis suggests that it was not political sophistication per se that has fostered tolerance, but that sophisticated citizens have responded to clear and uncontested norms among elites.

The authors' theory is elegant and compelling, but it must compete with other, not necessarily mutually exclusive, explanations. For example, Elisabeth Noelle-Neumann's arguments about the "spiral of silence" are not refuted by their data (e.g., elite opinion may be mistaken by citizens for *public* opinion). Moreover, elite consensus may be less important than the influence of *specific credible elites*. McClosky and Zaller refer to President Nixon's influence upon public opinion toward wage and price controls in 1971 (p. 152), but here there is less evidence for the impact of many elites than for simply effective presidential leadership. As the authors would agree, further study of these competing theories requires comparisons of changes in public opinion with content analyses of the views of different elites over time (p. 317).

If their theory is not on target, the authors' conclusion about the future of the American welfare state may be overly optimistic. They maintain that elite support for the welfare state will be passed on to newer, more politically attentive cohorts. But if the cohorts that have reached young adulthood in the years following the 1978 to 1979 period of the authors' last survey are more responsive than older cohorts to the cues from the Reagan administration and other credible conservative elites, then support for the current welfare state may be more fragile than the authors anticipate.

ROBERT Y. SHAPIRO

Columbia University

Common Cause: Lobbying in the Public Interest.

By Andrew S. McFarland. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. x + 212. \$20.00, cloth; \$11.95, paper.)

Once a moribund area, the field of interest groups has undergone something of a renaissance recently as new and provocative works have appeared. Drawing on the classic work of David Truman, Raymond Bauer, Ithiel de Sola Pool, Lewis Dexter, and Mancur Olson, research by Terry Moe, John Chubb, Jeffrey Berry, Michael Hayes, and Theodore Lowi, among others, has sought to update earlier interpretations in light of the explosion of group activities that took place in the 1960s and 1970s.

Among these developments, few things have been more controversial than the rise of public interest groups. PIGs, as they often are affectionately called, have been the subject of considerable debate in the popular press. Supporters have defended them as a necessary counterbalance in the contemporary system of interest groups. Given the fact that several forces (i.e., the expansion of the federal role in the 1960s, legal changes in the campaign finance area, and the often-noted decline of political parties) have stimulated private group activities, many observers feel that public interest groups are necessary to limit the demands of greedy and selfish groups.

In contrast, critics have derided public interest groups on several grounds. Despite claims to represent the public interest, writers point to the elitist membership of these organizations (meaning their tendency to attract "upscale" adherents) as well as a more general skepticism about the notion of the public interest to question whether public interest groups really are very different from private interest groups. Others have examined public interest groups and—after finding that many of them lack formal memberships, local chapters, or much means of communicating with their members—conclude that these "groups without constituencies" essentially are front organizations for leaders pursuing their own private agendas.

It is within this context that Andrew McFarland undertook his case study of Common Cause. His research questions are as follows: Who joins Common Cause? Why do they join? What is the structure of the organization? How is it governed? And what has its impact been on the American political scene?

According to McFarland, Common Cause can best be analyzed as part of the tradition of political reform in the United States. Founded by John Gardner, the organization systematically and pragmatically tried to bring about procedural reforms (such as campaign finance and opening

up the system) in the policymaking process.

However, the book often is disorganized and unclear in developing its argument. For example, McFarland's discussion of group origins and maintenance wanders from Truman's political disturbance models (i.e., the nature of the times and the need for a public interest organization) to Robert Salisbury's entrepreneurial approaches (the leadership of Gardner) to Jack Walker's patron perspective (the role of outside foundation support) to even Olson's economic approach (the logic of economic sidepayments). The absence of conceptual clarity means that McFarland's conclusions are confusing or give the appearance of a "laundry list" approach to explanation.

In addition, it is not clear exactly how McFarland comes down in his evaluation of Common Cause (either as a public interest group or as part of a reform movement). He concedes that Common Cause has a membership that is predominantly white, professional, and upper-income; that its issues often go beyond good government issues (such as open government and fair processes) to more partisan legislation (such as the B-1 bomber); and that some of its procedures for giving members a role in group decision making are more symbolic than substantive. Yet he also seems to feel that Common Cause has made a major and positive contribution to the American political system (notably in the campaign finance area). It would have improved the book for the author to have better integrated his disparate findings and evaluations.

Finally, I would have liked to have seen more explicit connections with the growing literature on public and private interest groups. McFarland claims that Common Cause is different from other public interest lobbies in its degree of pragmatism, its sensitivity to considerations of internal democracy, and its overt efforts to avoid the mistakes of past reformers. But he does not make these contrasts in such a way as to show how his conclusions fit with the work of others. Since we are experiencing an expansion in the literature on interest groups, McFarland misses an opportunity to make a major theoretical contribution in this area.

DARRELL M. WEST

Brown University

The Politics of Protection: The U.S. Secret Service in the Terrorist Age. By Philip H. Melanson. (New York: Praeger Publishers, 1984. Pp. ix + 215. \$22.95.)

The Politics of Protection is a refreshing, long overdue examination of one of the smaller and lit-

tle understood federal agencies—the Secret Service. By the author's own admission, the book "is the first major analysis of the U.S. Secret Service in 16 years" (p. viii). The central thesis of the book is that the Secret Service is a paradoxical agency—visible, yet secretive; "highly professional and merit based, yet frequently dominated by the political arena in which it operates; a complex, dual-purpose organization with a series of missions and a variety of occupational specializations" (p. viii).

Melanson examines this complexity by giving the book a unique political focus, one that enables the reader to view the interactions of the Secret Service not only in terms of a bureaucratic entity, but also from the point of view of political process and political culture. This approach is well illustrated as Melanson traces the evolving nature of the Secret Service (chap. 1) and adaptable nature of the agency as it interacts with the changing presidential personalities that it is assigned to protect (chap. 7). In essence, the author has opened a new window on the American political process.

Melanson uses methodology that is traditional, descriptive, and analytical. The book is very readable; the study easily bridges the gap between academic and popular interest. Arranged in 11 chapters, it falls into three general conceptual divisions. The first division is descriptive and organizational (chaps. 1 through 4) and includes the historical rise of the Secret Service, its organizational peculiarities, and personnel. The second division describes the complexities of problems the service has to deal with and the increasingly threatening world of contemporary political actors (chaps. 5 and 6). In addition, Melanson depicts the impact of presidential psychological predispositions on agency conduct (chap. 7) and the paramount role of politics in the protection process (chap. 8). The final division involves case study analyses of the John F. Kennedy assassination and the attempted assassination of President Reagan (chaps. 9 and 10). These last chapters are evaluations on how the Secret Service has measured up. The evaluations are objective, with no attempt to gloss over the errors of the Secret Service or to indict the agency unduly with a destructive attack on the agency's character.

If the book has a weakness, it is in the implication of the title. The impression is given that the book will add insight into the growing problems of protecting presidents and combating terrorism in contemporary politics. These problems are discussed, but only in a peripheral manner. Most of Melanson's effort concentrates on history and a discussion of the complexities of protection. The hard core discussion of how to deal with terrorists in any prescriptive sense is absent. This absence may be due as much to the ambivalence

of the phenomenon and political culture as it is to the analysis. In the words of former Assistant Secret Service Director James P. Kelly, "It is true that nearly all attempts on the lives of U.S. presidents have been by single gunmen, but the Secret Service's orientation nonetheless is reminiscent of the French General Staff in 1939: prepared to fight the previous war rather than the next" (p. 198).

In some ways Melanson is like the service he studies, dealing more insightfully with the past than giving direction for the future in a terrorist age. It is possible that this task was unrealistic from the outset. Nevertheless, *The Politics of Protection* is a noteworthy effort and a welcome contribution to the literature on American politics and the Secret Service.

JAMES L. WAITE

Central Missouri State University

Clients and Lawyers: Securing the Rights of Disabled Persons. By Susan M. Olson. (Westport, Conn.: Greenwood Press, 1984. Pp. xv + 236. \$29.95.)

Firmly grounded in the research tradition of the late Clement E. Vose, Susan M. Olson's *Clients and Lawyers* is a valuable addition to the scholarly literature on interest group litigation. While perhaps delivering a bit less than the title seems to promise, Olson's book offers a detailed examination of six mid-seventies lawsuits in which interest groups sought the aid of the courts in the battle for public transportation accessible to those with physical disabilities.

The primary contribution of this book is that it substantially extends our knowledge of interest group litigation in the lower state and federal courts, in contrast to Supreme Court-oriented constitutional litigation depicted in the works of such scholars as Vose, Frank Sorauf, and Karen O'Connor. A central theme of the book is the distinction between the "NAACP prototype" of centrally coordinated, lawyer-dominated, precedent-oriented interest group litigation and the "new style" of decentralized, client-controlled, remedy-oriented campaigns illustrated by the accessible transit cases. After introducing this new style of litigation in general terms, Olson presents sharply focused accounts of the six cases (in five metropolitan areas) studied in depth and concludes with an exploration of the implications of the new style.

A significant theme emerging from the case

studies is that the accessible transit plaintiffs generally avoided lawyer domination of their lawsuits. Unlike clients in the traditional model of interest group litigation, both interest group and individual plaintiffs were actively involved in making decisions about the conduct of their cases from initiation through settlement to monitoring of consent decrees. Such active client participation, Olson argues, helps avert the danger of relying entirely on the courts in pursuing group goals and improves the prospects of long-term success. Intriguing as these findings are, it should also be noted that differences in client participation may be largely a function of the organizational link between attorney and interest group. Lawyers affiliated with the NAACP's Legal Defense Fund, as well as other civil rights attorneys, were generally activists committed to pursuing group goals through the courts, whereas the lawyers in the accessible transit cases resembled more closely the traditional model of the private attorney retained to represent the interests of clients.

Thus, the most serious problem in the interaction of clients with their activist lawyers simply did not arise in these cases. Civil rights attorneys and lawyers for the poor have regularly faced the problem of what to do when pursuit of the immediate interests of an individual client may conflict with the pursuit of long-range group goals. Because the accessible transit attorneys generally entered the litigation without any prior commitment to specific policy goals, they faced no conflicts when remedy-oriented clients were ready to accept limited gains in preference to pressing for a broader declaration of rights (p. 127).

Perhaps the most challenging aspect of Olson's study is her effort to relate the remedy-oriented strategy adopted in these cases to outcomes. With appropriate caveats, Olson conveys the message that these interest group efforts to achieve social reform through litigation were, given the circumstances, reasonably successful. Yet a careful assessment of gains actually achieved cautions against an unduly optimistic conclusion. In general these suits were resolved by a negotiated settlement or consent decree in which defendant transit authorities agreed not to ignore the needs of disabled riders entirely. In no case did the plaintiffs obtain a decree declaring a statutory duty to provide accessible transit coupled with an order directing compliance. In short, there was no accessible transit decision analogous to either *Brown I* or *Brown II*. Plaintiff assessments of their success two years after the conclusion of their lawsuits were mixed. A rather discouraging picture emerges when such limited gains are examined in the context of a second Reagan term in which budget-cutting proposals seem to place urban mass transit in general—not merely transpor-

tation accessible to the physically disabled—at risk.

EDWARD V. HECK

San Diego State University

Regulatory Decision Making: The Virginia State Corporation Commission. By Laurence J. O'Toole and Robert S. Montjoy. (Charlottesville: University of Virginia Press, 1984. Pp. xiv + 389. \$32.50.)

This is a long, discursive, interesting case study. Its subject is the Virginia State Corporation Commission, once perhaps the most esteemed member of what is now an endangered species, the independent regulatory commission. Its focus is organizational change. Its theme is adaptation to political, intellectual, and technological changes in the organization's environment. Its protagonists are the organizational leaders who defined the SCC's strategy, shaped its structure and its organizational processes, and defended its distinctive competencies for over half a century.

As is the case with any good case study, this one enriches our understanding of the complexities of organizational and political life. It reinforces some of our interpretations and challenges others. For example, I have always been inclined to believe that independence matters—that to do good a regulatory commission must have the discretion to do bad, that the social utility of the regulatory commission approach to managing business behavior has been largely sacrificed to the American obsession with due process, and that regulators can be expected to serve the public interest well only where there is agreement as to its nature. I have also been inclined to doubt the utility of life-cycle models of the organization, although not of products or missions. This case study increases my confidence in all of these beliefs. That this has not challenged more of my beliefs is not a reflection on the quality of this work. Rather, it reflects my agreement with the basic premises of the study: that "the decisions of regulatory bodies are affected substantially by their decision-making processes" (p. 6); that choice is affected by the order in which alternatives are considered; the cost of information, and the time available, and that, insofar as they reduce the range of alternatives and consequences considered, so too do the standard operating procedures of the organization in question and the values of its members.

What this work sets out to do, it does very well. Its weaknesses are those of the case study method. But even for a case study, this work is unusually

atheoretical. It is, in part, about organizational change, yet it wholly ignores the literature on organizational strategy and structure. It is, in part, about the role played by regulators as agents of the polity, yet it ignores contemporary agency theory (e.g., see Glenn M. MacDonald's excellent review essay in the *Canadian Journal of Economics*, 1984, 17, 415). Indeed, I am somewhat inclined to fault its authors for methodological parochialism. This is not because they believe that the case study is the best way to study organizational dynamics—it may well be—but because they appear to believe that it is the only way to do so. If so it would explain their failure to acknowledge relevant empirical work such as M. W. Meyer's *Change in Public Bureaucracies* (Cambridge University Press, 1979) or the growing body of experimental studies of organizational and regulatory decision making in the tradition of Charles Plott and V. L. Smith. The authors of this study do acknowledge the influence of Richard Cyert and J. G. March on their thinking, but they ignore their methodology—the formulation of computer simulation models of decision-making routines and the testing of the models' outputs against the past behavior of organizational decision processes. Consequently, they have overlooked a substantial body of work that is directly relevant to their topic, starting with Paul Joskow's articles in the *Bell Journal of Economics* (1972, 3, 632) and the *Journal of Law and Economics* (1974, 17, 291).

As a result, this work will be of greatest value to those who are primarily interested in the facts of the case, to those who are interested in state politics in general and Virginia in particular, not to those who are primarily interested in regulatory decision making or organizational change.

FRED THOMPSON

Columbia University

The Politics of Bureaucratic Reform: The Case of the California State Employment Service. By Michael B. Preston. (Urbana: University of Illinois Press, 1984. Pp. xvii + 193. \$16.95.)

Michael Preston's *The Politics of Bureaucratic Reform: The Case of the California State Employment Service* is an extensive and well-documented study of the reform of the California State Employment Service during the 1970s and 1980s. Preston's central theme is that bureaucratic reform may be motivated more by political considerations than by concerns for efficiency. Indeed, he argues that reform is often pursued in order to disorient bureaucracies by "altering the distribution of power and influence in the

system" (pp. xiv-xv). The book proceeds to examine the nature of various reform models and strategies and to assess their effect on employment policy in California.

Quite frequently, case studies dealing with social service or labor policy are unable to overcome the difficulty of focusing too narrowly on the case at issue. As a result, such studies may explain a policy event, but they contribute little to an understanding of the substantive policy area in general. On rare occasions, certain case studies have overcome this difficulty and broadened our understanding of the administrative and policy processes. Jeffrey Pressman and Aaron Wildavsky's *Implementation* (University of California Press, 1973) was one such study. *The Politics of Bureaucratic Reform* should enjoy a similar degree of success.

In the Preface, Preston acknowledges that the problem of reform is personally and intellectually significant to him. The book continually reflects his interest in and enthusiasm for the topic. For a number of reasons, this study of the California State Employment Service allows the reform process to be examined extensively. Because the agency was consistently involved in some type of reform effort between 1968 and 1981, Preston is able to benefit from the use of longitudinal analyses in his research. Moreover, the size of the agency (a \$700 million budget in 1980 and 1981) certainly qualifies it for consideration as a major bureaucracy under virtually any criterion. Finally, the events described in the book constitute a rich and varied array of reform approaches and experiences. One is even prone to discount the fact that it is just another California case study.

One of the most appealing characteristics of the book is its extensive use of the literature on administrative reform, organizational change, and political power. Throughout the study, Preston skillfully weaves this broader body of literature into case-specific situations, generally giving the event a much broader definition and leaving the reader with a better understanding of its significance. Administrative and policy changes are assessed for their impact on agency personnel and clientele as well as for their impact on the agency itself. The treatment is balanced and fairly exhaustive. At various points, the effects of administrative structure, political leadership, personnel policies, and centralization/decentralization are assessed. As a consequence, the book is a powerful instructional tool that considers both the potential and the limitations of reform. Perhaps just as importantly, Preston avoids overburdening this excellent study with irrelevant minutiae. The book is more than readable; it is interesting.

If there is a weakness in the book, it is the

absence of any higher order statistical analysis. The empirical studies used in the book are satisfactory; they are used effectively to substantiate a point or call an issue into question. Still, Preston could have been more innovative in the management of his data bases and more creative in the presentation of his statistical evidence.

In sum, however, *The Politics of Bureaucratic Reform* is a superb case study. The book should very quickly find its way into the mainstream of the literature on administrative reform.

LUTHER F. CARTER

College of Charleston

Forecasting Presidential Elections. By Steven J. Rosenstone. (New Haven, Conn.: Yale University Press, 1983. Pp. xii + 211. \$20.00.)

Whether or not the problem of ecological inference is ever resolved, the analysis of voter decisions and election outcomes will each remain important subjects in the study of American politics. At this point, however, most research is exclusively about voters.

Through a careful analysis of state-level aggregate presidential election returns from 1948 to 1980, *Forecasting Presidential Elections* begins to correct this imbalance. Most of this book is a justification for and test of an econometric model of presidential election outcomes. Included as explanatory variables are changes in real disposable income, incumbency, secular political trends, friends and neighbors effects, "New Deal Social Welfare" and "Racial Liberalism" scales, percent catholic (1960 only), public opinion about the president's mismanagement of war (1952 and 1968 only) and about the party best for prosperity and for peace (1980 only), party factionalism, and candidate issue positions (measured by surveying "scholars of American political parties and elections," p. 174). One can disagree with choices of specific variables (particularly the latter) and with different functional forms, but it is unlikely that these would markedly change Rosenstone's substantive conclusions. Moreover, not only does the book provide an exemplary statistical analysis, but, as is rarely the case in political science, sufficient information is also reported so that the reader can understand precisely what analyses were conducted.

To evaluate the model, two types of forecasts are generated. First are "pre-election forecasts"—based only on information available before election day. Although these forecasts do universally better than those of other political scientists, they never surpass, and are therefore less useful forecasts than, last-day public opinion polls. The

second type of forecast used in the book is based on "perfect information." This means, for example, that second-quarter real disposable income was used to forecast the 1980 election even though this indicator was not announced until five days after Carter was defeated (p. 119). These forecasts almost always did better than even the last-day public opinion polls. Thus, what Rosenstone calls "perfect information forecasts" are not really forecasts at all, except in the sense that it was theoretically (although not practically) possible to have perfect information. These "ex post forecasts" are therefore useful in evaluating the model but are useless for forecasting elections. Because the models that perform better than the public opinion polls are derived from information available only after the election, the substantial contribution of this book is to "capture the essential features of presidential contests" (p. 112)—to explain rather than forecast presidential elections. This is a minor difference in practice, but a major difference in conceptualization.

Another point not mentioned in the book is that the model does well even by ignoring most of the factors journalists and politicians attribute to causing presidential election results; these include Ferraro's finances, Ford's "Soviet domination of Eastern Europe," Carter's "ethnic purity," and Reagan's "Are you better off today." It is possible that a "campaign detail" variable that is omitted from the model is correlated with some of the included variables, therefore biasing the results. But although this is statistically possible, it is theoretically unlikely. That Rosenstone does so well in forecasting and explaining presidential elections without these details is confirmation of the suspicions of some (Kathleen A. Frankovic, "The 1984 Election: The Irrelevance of the Campaign," *PS*, 1985, 18(1), 39-47) that the campaign (or at least most of the rhetoric of which it is composed) is indeed irrelevant.

Considering that in 1980 20% of voters changed their minds in the last few days of the campaign and 10% decided whom to vote for on election day (pp. 28-29), it would have been amazing if Steven Rosenstone were half as successful in forecasting or explaining presidential elections. Well-designed, well-written, and well-executed, *Forecasting Presidential Elections* is an important contribution that should be read by all who are interested in understanding electoral results or voting behavior.

GARY KING

New York University

The Presidency and Public Policy: The Four Arenas of Presidential Power. By Robert J. Spitzer (University: University of Alabama Press, 1983. Pp. xii + 189. \$18.75.)

Challenging the view that presidential power derives from the president's personal political skill, Robert Spitzer calls upon scholars to abandon this Neustadtian (or "great-man") model in favor of a "situational," or policy approach to leadership and power.

There are, Spitzer maintains, "specific characteristics of policies proposed by presidents that shape what the president can do and how well he can do it . . . policies structure the interests involved and help to determine the political arenas in which decisions are contested or made" (p. xiv). In short, different policy arenas produce different political interactions and different success rates.

The policy model that Spitzer proposes is based upon Theodore J. Lowi's "arenas of power" scheme, which suggests that there are four policy areas: distributive, regulatory, redistributive, and constituent. Spitzer attempts to apply this policy model to the presidency.

Using only domestic issues from 1954 to 1974, Spitzer hypothesizes that each policy area produces a different involvement and success rate by presidents, that this holds true regardless of who is president, and that issue area is more important than personal skill in determining success or failure.

Spitzer projects that the level of presidential influence and involvement in policy areas "will be, in order from greatest to least, redistributive, constituent, distributive, and regulatory" (p. 36). Distributive and constituent policies tend to be more consensual, whereas regulatory and redistributive policies are more conflictual. Given these policy domains, Spitzer arrives at "a primary theme of this study: that presidential activities in the four policy areas differ sufficiently to suggest the existence of 'four presidencies'" (p. 38). There is the "special interest presidency" (distributive), the "presidential broker" (regulatory), the "public-interest presidency" (redistributive), and the "administrative presidency" (constituent).

Spitzer attempts to test this policy approach to the presidency by applying both illustrative case studies and congressional voting data to the model. In general, the approach stands the test of congressional voting. The president is able to achieve a higher success rate in the redistributive arena, and the lowest success rate in the regulatory arena.

Does this mean that such factors as the president's political skills, partisanship in Congress,

and public pressure do not affect legislation? Although Spitzer does not go this far, he does suggest that these forces are less influential than "conventional wisdom" would suggest. "Policy characteristics determine the shape of the president's political universe, at least as it relates to his dealings with Congress" (p. 154).

What lesson can presidents learn from this study? As Spitzer notes, if the main concern is legislative success, they should "concentrate their legislative attentions on constituent and redistributive policy efforts, with perhaps a sprinkling of distributive bills aimed at pacifying particular congressmen and constituents. The high political costs and absence of immediate rewards connected with regulatory policies would discourage any major efforts in this area" (p. 156). The implications of these "lessons" could have a significant impact upon legislation and problem-solving from a national perspective.

This fine study does indeed challenge (although I do not think it will replace) the Neustadtian model of presidential power. An effort to integrate this policy model with such factors as presidential skills, public mood, and partisanship to form a more comprehensive portrait of president-congress relations might give this policy model more credibility. On its own, however, I am not sure that the model—although innovative and important—is compelling enough to persuade scholars that it could stand on its own.

Spitzer's study would have benefited from a more explicit application of the policy model to such bursts of presidential legislative success as the early Lyndon Johnson years or the first two years of the Reagan presidency. In spite of these weaknesses, *The Presidency and Public Policy* merits attention and consideration.

MICHAEL A. GENOVESE

Loyola Marymount University

Procedural Structure: Success and Influence in Congress. By Terry Sullivan. (New York: Praeger Publishers, 1984. Pp. xiv + 284. \$29.95.)

In *Procedural Structure*, Terry Sullivan seeks to examine and specify the conditions behind the often-repeated maxim that "procedure makes policy" within the U.S. Congress. This is an important, if arcane, subject, and Sullivan is an expert guide through the familiar, yet extremely complex, terrain of legislative procedure. In the end he gives the reader a valuable atlas for tracking the various paths that a bill may follow, although we remain at some distance from determining how process affects policy.

This is a specialist's book, with two very specific audiences. First, it aims at congressional scholars, for whom it provides both a reference-like discussion of House rules and a set of analyses of how these rules may affect policy outcomes. Sullivan also directs his attention to those who are interested in examining formal relations among political actors, regardless of the institutional setting.

Sullivan succeeds in providing detailed descriptions, often expressed in formal terms, of House rules. His discussions of sequence, resolution, and jurisdiction are very useful; a related discourse—on “resource distribution” rules—is less so. Indeed, resource distribution in the Congress operates differently than do rules that affect resolution or jurisdiction, and Sullivan might well have omitted any discussion of resource distribution within the “rules” rubric.

The book succeeds in presenting the rules as part of a formal set of arrangements that have real implications for the legislative process. For example, Sullivan carefully develops the significance of sequence (pp. 30-34) and later provides some relevant analyses that link sequence to complexity. The manipulation of sequence may be especially significant where “the procedures require that those alternatives which are offered must be of a specific structural type . . . [such as] a substitute rather than a simple amendment” (p. 127). Although all members have some power here, many advantages accrue to a small number of legislators (leaders, Rules Committee members) who have special authority in affecting sequencing. This control has consequences; Sullivan asserts that “the more complex the legislative situation, the more difficult it is for amendments to succeed” (p. 141).

The reasonable inference is drawn that “this research helps one to understand why all the effort put into Rules Committee activity is so important” (p. 142). The question remains, however, of whether the Rules Committee members understand fully the ways in which their actions structure the decision-making process. Sullivan provides anecdotal evidence to demonstrate that some, like legislative strategist Richard Bolling, had clear visions of the scenarios that they were scripting. Further anecdotal evidence (not presented here) suggests that other key Rules actors have less acumen in devising procedural ploys. Indeed, the democratization of the House may have opened up the legislative strategist role to a host of members, from both sides of the aisle.

The major limitations of *Procedural Structure* come in Sullivan's attempts to generalize toward conclusions that are broader than the study and its data warrant. A particular problem arises in his analyses of responsiveness within the House.

Without any reference he observes that the “objective of a representative legislature is to reflect through its decisions the preferences of those whom it represents. In the long run, failure to realize those preferences is a liability for the institution and for the actors associated with it. Legitimacy in this form, related to the reflection of preferences, will be called *responsiveness*” (p. 84).

Such a working definition oversimplifies a concept that has received a great deal of attention in the recent past. How is the reader to assess a subsequent conclusion that the “failure of to provide responsiveness through its most basic decision rules means the legislature's procedural structure does not merely act as an engine of choice but exercises an independent effect on decision making” (p. 142)? Close observers of the Congress have rarely expected the body's rules to facilitate majority-based decision making. The very number of ways in which the legislature can be responsive to its various constituencies lies at the heart of what makes a representative assembly such a continually intriguing institution to examine.

In sum, *Procedural Structure* is at its best in its specific explication and examination of the rules of the House of Representatives, a most complex organization. As a reference-like volume on the nature of these rules, congressional scholars will find it of great value. The more general analyses of how “procedure makes policy,” while suggestive, are only very modest first steps in untying the knot of the legislative process. Where Sullivan sees regularity and pattern across the entire array of legislative activities, I tend to see an unruly tangle, which on occasion can be partially unravelled, but which remains an unlikely candidate for formal and general theorizing.

BURDETT LOOMIS

University of Kansas

Making Bureaucracies Think: The Environmental Impact Statement Strategy of Administrative Reform. By Serge Taylor. (Stanford, Calif.: Stanford University Press, 1984. Pp. x + 410. \$29.50.)

When the National Environmental Protection Act went into effect in 1970, its impact upon agency decision making was never thought to be as tremendous as this volume and other studies have vividly demonstrated. The Environmental Impact Statement (EIS) process, which NEPA requires, has forced development-oriented federal agencies such as the U.S. Forest Service and the Army Corps of Engineers to avoid projects that

would damage the environment lest the projects be delayed or even discontinued as a result of court litigation or public criticism. As it turns out, the EIS process has played the role of a 23rd Psalm, according to Richard N. L. Andrews, to which federal development agencies cannot afford to pray if they are to proceed with the blessings of the high priests of the environmental movement.

Writing an environmental impact statement is no easy task; NEPA requires that they must be interdisciplinary in scope and nature with an emphasis on balancing socioeconomic objectives with environmental values. It became imperative for development agencies to add to their staffs a special breed of civil servant such as landscape architects in the Forest Service and archeologists in the Corps of Engineers. These environmental analysts are the personification of the change agents, envisaged by Warren Bennis, Louis C. Gawthrop, and the New Public Administration, who are supposed to play the roles of value allocators and policy advocates in public organizations. Implementation of this innovation, however, was slow and difficult because there was a strong resistance from within the organizations to this kind of social change. The development agencies may have accepted the fact that the environmental analysts are going to be part of their work force permanently, but the analysts will have to deal with the problem of social acceptance by their engineering colleagues in the Corps and the Rangers in the Service. They will also have to rely on the help of the environmental groups to bring pressure upon the agency leadership so that environmental values will be considered in the planning process. As environmental analysts seek the help of such groups as the Sierra Club or the Natural Resources Defense Council, they run the risk of organizational disloyalty. Consequently, they stand to lose their credibility and run the risk of diminishing their effectiveness.

Commenting agencies like the Council on Environmental Quality and the Environmental Protection Agency will undoubtedly reinforce their standing as watchdogs of the environment. But as long as they work in an agency organized along hierarchical lines led by elites who play the game of bureaucratic politics, their EIS analysis will be worthless and meaningless, and the entire process becomes futile.

Environmental Impact Statements can be enriched by adding to NEPA agency offices humanists and ethicists who are better equipped and prepared to address the problem of value allocation. And if these environmental specialists feel strongly that their warnings of environmental damage falls on the deaf ears of agency leadership, there is a way to force agencies to act more responsibly. Under the Civil Service Reform Act

of 1978, the way to straighten an agency up is to blow the whistle.

The heart of NEPA is the EIS process. Its objective transcends the environment. Its target is bureaucracy itself and to make it accountable for its acts. In the light of this volume, bureaucracy can listen to advice. It can modify its plans. It can make some accommodations to improve its sagging public image. It may not be the panacea hoped for by students of public administration, but as Frederick Anderson said, it has "led the agency horses to water even if they do not always drink."

A. B. VILLANUEVA

Western Illinois University

Children, Families, and Government: Perspectives on American Social Policy. Edited by Edward F. Zigler, Sharon Lynn Kagan, and Edgar Klugman. (New York: Cambridge University Press, 1983. Pp. xvii + 460. \$37.50.)

Nowhere does the complexity of relations among political and social institutions, social science research, and societal values appear more vividly than in the arena of public policy affecting families and children. This book, conceived in the late 1970s but reflecting the dramatic changes of the early 1980s, encompasses a broad range of these relations. The intent is to provide the reader with a "palette of different perspectives" (p. 4) on the development and implementation of such policies. Twenty-three chapters by social scientists and public and private practitioners give theoretical, political, and historical background, strategic advice, and case study analysis.

The theoretical essays that seem most valuable are related directly to the policy area, such as G. D. Brewer's examination of weaknesses in problem estimation that led to failure to make connection between known data on the rubella epidemic of 1963-1965 and later demands on the service delivery system for the handicapped. Chapters on perspectives in political science and the federal legislative process are not targeted to family policy and are less useful.

A set of essays on federal and state responsibilities in social policy provides an intriguing variety of viewpoints on issues of centralization. J. L. Aber, in a case study of Massachusetts, shows state development of an impressive array of services, with growing provision by the private sector and institutionalized public participation. The effects of rapid contraction on these activities in a state fiscal crisis give a preview of later federal cutbacks. Reagan administration policies are viewed with some optimism by J. E.

Hansen, who looks to a sorting out of government functions and opportunity for more local initiatives. S. B. Heintz, in a study of Connecticut, argues that what Hansen called the "end" of federal support (p. 127) has decentralized costs rather than decision flexibility to state or local officials.

As current trends in the nation and states show, even established programs in this area are vulnerable to reversal. Efforts to influence policy must be equipped with data, learn sophisticated media techniques, and adjust to the shift to the federal budget process as the center of legislative policymaking. One comprehensive essay details difficulties in development and application of childhood social indicators and guides the reader to important sources of data. Research must be protected, even if services decline, or long-range impacts of the decline will be unknown. Other authors consider strategies for effective advocacy.

Eight policy case studies demonstrate complex interactions of research, values, politics, and practice. The search for comprehensive policies with clear goals and measurable results is rejected by most as unrealistic in spite of existing duplication, overlapping, and confusion. The Headstart study, for example, largely attributes the program's success and survivability to its multiple, shifting goals. It is made clear that urgent needs overtake research. Programs cannot wait for comprehensiveness or clarity. The political system itself is recalcitrant to planning, yet as present reverses indicate, in the long run the inefficiencies and lack of measurable gains undercut support. A chapter by Seymour B. Sorenson warns that the narrowness of social science research reinforces a dangerous myopia in application of single policies. His study of implementation of the

"mainstreaming" education for the handicapped law shows that without understanding of the system, the intent of the law may be circumvented or other dislocations occur. It is just the breadth Sorenson advocates that others view as impossible.

The case studies also show ambivalent attitudes to the role of values in family policy. Those who support government action speak from convictions of important values. Their very claim for government actions, however, often phrased as "intervention," grates against other values of privacy and market primacy. Moral convictions of those supporting or opposing government policies may add intensity to their efforts. Yet as in child abuse and drug abuse remediation, the moral overtones may interfere with effective treatment. Value conflicts and distaste for righteous policies of any stripe form the context in which child and family policies must be forged. The difficulty is to devise programs that will meet needs and endure through the inevitable shifts of dominant values in a society with mixed loyalties.

This book is indeed a palette with resources for scholars, professionals, advocates, and political leaders. The approach is nonpolemical, scholarly, and displays keen sensitivity to the political process. A more incisive discussion of the issues raised by the chapters would enhance its usefulness. The introductory preview is superficial and not fully accurate as it deals with coming chapters. The stated purpose, however, is achieved on a level that is both encouraging and helpful to those who continue to study and work in the fields of human services.

MARY ANNE CAVICCHI

John Carroll University

Comparative and Other Area Studies

Political Anthropology Yearbook: Vol. 2, Culture and Political Change. Edited by Myron J. Aronoff. (New Brunswick, N.J.: Transaction Books, 1983. Pp. 221. \$29.95, cloth; \$12.95, paper.)

Political Anthropology Yearbook: Vol. 3, Religion and Politics. Edited by Myron J. Aronoff. (New Brunswick, N.J.: Transaction Books, 1984. Pp. 139. \$29.95, cloth; \$12.95, paper.)

The term *political anthropology* usually refers to the study of political processes in the types of

societies that anthropologists study—that is, in societies that may not have a clearly demarcated political domain. Political anthropologists have thus faced in its most acute form the problem that political theorists face in trying to define "politics." With recent interest in interpretive anthropology (in the manner of Clifford Geertz) and the study of symbolism, the cross-cultural applicability of a Western term like *politics* has come to seem even more problematic. These volumes on the relationship between politics, culture, and religion speak directly to such difficulties.

As is usual with collections, some of the essays

of these volumes are quite good, whereas others are pedestrian. Volume 2 focuses on the contrast between culture and ideology, that is (as Aronoff defines the distinction), implicit patterns of meaning relatively unaffected by rational reformulations versus explicit, rationally formulated beliefs that can be self-consciously affirmed or attacked. Aronoff and several of the contributors think of myth and ritual as examples of culture and rational discussions of political means and ends as exemplifying ideology. They tend to agree that implicit cultural patterns constrain political action, keeping it within the confines of a status quo, to a greater degree than ideology. This is because change comes about when cultural values are explicitly examined (as in ideology), then renewed, reinterpreted, or rejected. All the essays of Volume 2 speak more or less directly to these issues. Particularly successful are Marc Swartz's discussion of domestic politics in the Swahili nuclear family, W. Lance Bennett's analysis of American elections as nonrational rituals, and (in a more ethnographic mode) James Scott's depiction of common sense and political criticism in a Malay village.

There are several subtle but important criticisms to be made of the culture/ideology distinction as Aronoff and some of his contributors (particularly Bennett, David Kertzer, and Mary Hegland) have formulated it. First, they overlook the possibility of a nonrational basis of rationality, if I may so phrase the matter. In other words, they assume that rationality can be easily defined, or at least that it represents an unproblematic dimension of human thought. But many linguists and anthropologists would challenge that notion, arguing that *all* thought depends upon implicit linguistic and/or cultural patterns. In this view, Aronoff's concept of rationality suggests the hubris of the Western tradition, which has typically seen its own—culturally specific?—rationality as universal, as rationality writ large. If such a critique has merit, we would have to revise Aronoff's theoretical formulation to show that a political status quo might equally well endure on either rational or mythic foundations. And that suggests that his notion of myth might need to be revised as well, for it may not be as easily distinguished from rationality as he and some of his contributors suggest. (To what degree is the notion of rationality our myth?)

Volume 3, on religion and politics, is less coherent than Volume 2 because the collected essays do not engage the fundamental issue raised by I. L. Horowitz in his preliminary comment to the volume. Horowitz asks us to reevaluate our common-sense understanding of the categorical distinction between religion and politics and suggests that to the degree that we fail to do so, we

may be condemned to misunderstand social action in non-Western contexts where religion and politics may not be separate or (I would add) relevant categories. Unfortunately, the essays in Volume 3 do not explicitly pursue such questions. Two essays on religion and politics in Israel (one by Aronoff, the other by Charles Leibman and Eliezer Don-Yehiya) would seem to belong in Volume 2, for they focus on the reformulation of religious ideology for political as well as religious goals. Essays on Iran (by Jerrold Green) and Lebanon (by Augustus Norton) confine themselves to the role of religious groups in political maneuvering without using the data to reflect on the larger issue of whether religion and politics are helpful scientific categories for those cases. Indeed, one might go beyond that question to ask about political anthropology itself—for there is an uneasy relationship in these essays between political science and anthropology. Many of the analyses of ideology and culture would be read by anthropologists as "anthropology" pure and simple rather than "political anthropology." (I should mention in this regard the fine piece of intellectual history found in Wilson Carey McWilliams's contribution to Volume 3, "The Bible in the American Political Tradition.") Other essays seem to me (an anthropologist) to be straight political science. Is that difference (between the two groups of essays) related to a more fundamental distinction between interpretive approaches versus positivistic ones, symbolic versus social-structural analysis? Would a radically interpretive approach throw out *politics*, *religion*, and even *social structure* as Western terms that cannot be cross-culturally applied? Such are the questions raised by these volumes and by the type of interdisciplinary inquiry that Aronoff is to be commended for organizing.

RICHARD HANDLER

Lake Forest College

Scheming for the Poor: The Politics of Redistribution in Latin America. By William Ascher. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 348. \$25.00.)

A generation ago in *Journeys toward Progress* (Twentieth Century Fund, 1963), Albert O. Hirschman argued the case for "reformmongering," or the potential for the contriving of reform by astute policymakers within the confines of the seemingly resistant economic and political structures of Latin American countries. However, the ultimate failure of the reforms Hirschman touted, the increasing militarization of Latin American

politics beginning in the mid-1960s, and a growing preoccupation with "revolution" called Hirschman's reformist optimism into question and made his analysis seem less relevant to Latin American political realities.

Scheming for the Poor by William Ascher revives such concerns. In a vein rather similar to Hirschman's, Ascher's central point is that:

In case after case the masters of redistribution prove to be the tacticians rather than the warriors. The best records of redistribution are held by the pragmatic politicians whose familiarity with the policy process enables them to manipulate the political atmosphere to lull, disarm, or intimidate the . . . potential allies among other groups fearing drastic redistribution. (pp. 18-19)

The heart of the book comprises a series of case studies of postwar regimes in Argentina, Chile, and Peru and is divided into sections that examine authoritarian populists (Perón, Ibáñez, Odría), democratic reformists (Frondizi, Illia, Frei, and Belaunde), and radicals (Allende and Velasco). Conclusions are drawn concerning the reasons for the successes and failures of the "redistributional" policies of each regime, and each variety of regime as well as redistributional policymaking in general. Although Ascher is certainly aware of contextual and structural constraints, his interpretation of the evidence is broadly nondeterministic, affording substantial scope for leadership and redistributional change within Latin American policy processes under a variety of regime types.

In a sense the book can be read as a primer for policymakers. It is replete with such generalizations/admonitions as: progressive policy measures tend to generate additional demands, rather than increased support, from beneficiaries; the beneficiaries of redistribution tend to resist the extension of benefits to others; allies from the non-poor are essential to redistributional success; and improvisation and attention to detail are important for policy success even on the part of radicals with initially sweeping programs.

Critics may wonder how significant or permanent the redistributional policies of Ascher's regimes may be, given that many of them entail such things as changes in tax rates (which Ascher argues often have a more effective redistributional impact than more dramatic changes) rather than deep structural transformation. Given the ultimate fate of, say, Hirschman's Colombian agrarian reform law of 1961, so apparently impressive in its legislative phase but extremely modest in its implementation, one may indeed wonder whether truly significant redistribution can take place in Latin American political systems short of something like the Cuban Revolution.

Some of Ascher's conclusions concerning the treatment of beneficiaries may also miss the political realities of the need to cultivate one's initial supporters merely in order to remain in power.

Still, *Scheming for the Poor* is clearly a major contribution to the slowly growing literature on policymaking in Latin America and is perhaps the most significant contribution to date to the literature on comparative public policy from an author writing on Latin America. The book has important normative implications as well. It will give sustenance to those who hold that meaningful change on behalf of the poor can be attained without full-scale revolution, and that the fate of regimes and their policies owes as much or more to political leadership as it does to determinants of economics or social structure.

ROBERT H. DIX

Rice University

The Industrial Future of the Pacific Basin. Edited by Roger Benjamin and Robert T. Kudrle. (Boulder, Colo.: Westview Press, 1984. Pp. xvi + 285. \$27.50.)

The subject of this edited volume represents one of the more fascinating and important topics in the contemporary world political economy. The volume examines the substantial shifts in industrial production and foreign trade patterns among countries on both sides of the Pacific. Contributors include government officials and academics from Japan, the Philippines, South Korea, Thailand, and the United States as well as representatives from the Asian Development Bank and the United Nations. Primary attention is given to the newly industrializing countries (NICs) of Asia (Hong Kong, Singapore, South Korea, Taiwan), although consideration is also given to other developing countries in the region (notably, the Philippines) and the Pacific's more advanced industrialized countries (Japan and the United States). This region is characterized by perhaps the most dynamic changes in economic growth, industrial development, comparative advantage, and export trade in the entire world economy. Such dynamism demands explanation. In particular, the identification of the sources of such industrial growth and changing comparative advantage stands out as the most compelling issue in this area of study.

Part I offers qualitative and quantitative descriptions of the rapid industrial and foreign trade changes in the region. The causal link from industrial restructuring to aggregate economic

growth is documented. Ventures beyond this important and basic point, however, flounder. Policy recommendations are made without sufficient analysis. The NICs, we are told, must foster further industrial adjustment to avoid under-specified economic problems (e.g., unbalanced growth). Why we should be concerned about the growing pains of the NICs rather than the problems posed by the NICs for the mature industrial economies is not established. Recommendations are made to avoid collision among rival industrial economies. For instance, the Southeast Asian economies should concentrate on labor-intensive industries while the NICs move on to more technology-intensive sectors. However valid such advice may be, it remains vacuous without an understanding of the motive forces underlying industrial change in Asia. Only after understanding why the Pacific economies are riding each others' bumpers in a race for industrial development can we consider sound measures to coordinate economic relations. More importantly, initial efforts to identify these motive forces become muddled. After recognizing the importance of structural economic change for aggregate growth, the causal arrows are reversed when it is argued that economic development can explain changes in comparative advantage and export structure. While feedback loops are to be acknowledged, tautology must be avoided.

Greater success is made in Parts 2 and 3, where three candidates are considered as accounting for Asian industrial development: government industrial policies, labor force characteristics, and foreign direct investment. Other than covering well-known ground, a chapter on Japanese industrial policy suffers from two flaws. First, after reviewing the record of government support of industrial development and changes in Japan's industrial structure, the critical link between the two is established with neither qualitative nor quantitative analysis. Second, the relevance of the Japanese case for structural change in the industrializing "new Japans" is not made explicit. The causal link between government policy and industrial restructuring is much more carefully explored in a chapter on the Philippines. A shift away from two decades of import substitution provided opportunities for Philippine industrialization and export expansion in manufactured goods in the 1970s.

Another possible source for industrial change in Asia is considered in a chapter on labor force characteristics. In particular, a strong correlation is established between the educational attainment of a country's labor force and the composition of the country's industry. This relationship deserves further investigation because the authors, Andrew Mason, Sung-Yeal Koo, and Wi-Sup Song, admit

the correlation may be spurious and do not attempt to unravel the direction of causation between high levels of education and high levels of development.

Although three separate chapters are devoted to the role of foreign direct investment in Asia, only one explicitly considers the multinational corporation (MNC) as an agent of structural adjustment. One chapter explores the proclivities of MNCs in South Korea with regard to industrial concentration (higher for MNCs than domestic firms), capital and technology intensity (higher), labor productivity (higher), and profitability (no difference). Although such findings would certainly provide the basis for further empirical investigation, the characteristics of foreign capital are not, in turn, linked to broader changes in the Korean industrial economy. A second chapter treats foreign direct investment as a dependent variable to be explained rather than an independent variable with an impact on structural change. Indeed, both of these chapters observe that the pattern of foreign direct investment in Asia may *reflect* rather than shape country comparative advantage. Whether foreign investment follows or leads comparative advantage is a critical issue deserving more empirical research. Only a third chapter (chap. 9) hypothesizes that industrial restructuring can be brought about by foreign direct investment. The diffusion of technology by the multinational is identified as a major agent of change.

In an otherwise comprehensive survey of the Pacific basin economies, the absence of any serious treatment of China is notable. In addition, reference is made throughout the volume to the fostering of a "Pacific basin community" without any explication of the rationale for the coordination, let alone integration, of the respective economies.

Important issues and provocative hypotheses emerge from this volume. The contributions highlight the need to sharpen those issues and further investigate the hypotheses.

GLENN R. FONG

University of Illinois at Chicago

Change in British Politics. Edited by Hugh Berrington. (London: Frank Cass, 1984. Pp. 239. \$29.50.)

Hugh Berrington and his 10 fellow contributors have produced a very useful collection of articles on British politics in the Thatcher years. The major themes concern electoral realignment and changes in the party system, but regionalism and

local government are also included along with informative papers on both business and trade unions.

The 1983 election results are sketched by Michael Goldsmith and Ken Newton, placed in the context of postwar partisan dealignment (the loss of partisan support for the two major parties) by Ivor Crewe, and interpreted in terms of the growth of regionalism by W. L. Miller. The rise of center politics with the formation of the SDP-Liberal Alliance is documented by David Denver, and James Douglas and Philip Williams consider changes in the Conservative and Labour parties respectively. This arrangement of discussion leads to some repetition, particularly in the lists of electoral swings and party supports, but Berrington's editorial control is much more evident than is the case in some collections of articles. This is not to say that the conclusions reached show much more consensus than has marked the period in British political life under scrutiny. For example, the section on regionalism portrays the Thatcher government as destroying local democracy with an overbearing centralism, whereas the chapter on the Conservative party, although critical of the ideological fervor of the first years of the Thatcher administration, argues that a more moderate, incrementalist approach is now visible.

Most contributors stress the economic impact of Britain's decline as a manufacturing nation. Higher unemployment, the differing regional economic performances, the decline in the traditional class solidaristic basis of the Labour party, a new professionalism in both party workers and activists, the failure of attempts at corporatist economic control, and the failures of both Keynesian and monetarist economic policies all have resulted in a political environment markedly different from the cozy complacency of the Macmillan years. The electorate has come to terms with greatly reduced expectations but has lost faith in party politics. The new generation of British voters lacks class loyalty and is regionally diverse and volatile. The old landscape of British politics has been partly destroyed, and students of British politics await the erection of a new architecture on the site with very open minds about the likely design. Party activists, particularly in the Labour party however, are increasingly certain about their differing political truths. Williams's analysis is interesting here. He examines the rise of the Left amid the fall of the Labour party and compares the new Labour militants with the revolutionary determinists predominant in some Continental socialist parties in the 1920s.

The problems of the Conservative party, on the other hand, have been disguised by their landslide victory in 1983. Despite a lower share of the poll

than in some previous defeats, they now dominate the Parliament due to the split in the Opposition and the vagaries of the electoral system. And the electoral system could crush them if their support was slightly reduced. This is important because Conservative support depended heavily on the temporary surge of imperialist sentiment that followed the Falklands affair in 1982. The Conservative party is now a regional party largely confined to the South and Midlands. Its parliamentarians are thus largely removed from the experience of economic decline in the North and Scotland. Desperate industrial strife and renewed demands for separation in Scotland are likely developments. The party of national unity and industrialism has shed its patrician style of leadership and now attempts to impose centralist policies on a disunited kingdom. Berrington and his fellow authors have raised these important paradoxes in our assumptions about modern British politics. There remains much theorizing to be done if these paradoxes are to be resolved.

JEFF ARCHER

University of New England (Australia)

Coalition Government in Western Europe. Edited by Vernon Bogdanor. (Portsmouth, N.H.: Heinemann Educational Books, 1984. Pp. vi + 282. \$42.00.)

In the early 1980s the British went through a crisis of confidence in two-party government. The polls were suggesting that the alliance between the new Social Democratic party and the Liberal party could prevent either the Conservatives or Labour from winning control of Parliament. Holding the balance of power, the alliance would then demand the installation of proportional representation, thus making coalition government a permanent fixture of British politics. This book is a result of that period. It is the product of an international seminar held in October, 1982 at the Policy Studies Institute in which experts from a number of Continental countries explained how coalition politics worked in their nations so that the British could be prepared to cope if the need arose.

Bogdanor provides erudite introductory and concluding chapters that eschew formal mathematical coalition theory in favor of a cautious "lessons for Britain" analytical viewpoint. He asserts that coalition government is more than a modification of the Westminster model of parliamentary practice. It is a specific type of government whose rules and conventions

all flow from the fundamental principle of power sharing. Bogdanor wants to investigate such questions as how the role of the monarch differs in countries where coalition building is the norm; how coalitions alter traditional British notions about cabinet responsibility; whether they strengthen or weaken parliament with regard to the executive; and how coalitions alter the functions of elections in the political system. Do they make any difference in the system's ability to make policy or in the content of policy outputs? Bogdanor effectively illustrates the complexity of trying to answer these questions definitively. For the U.K., he writes, those who would favor coalition government do so because they believe "that a basic consensus in British society is frustrated by the adversary process in Westminster. A coalition, on this view, will be better placed to mobilize consensus in the interests of social and economic modernization" (p. 277). But, ever cautious, he is not ready to claim that this book demonstrates the truth of that position. Instead, he calls for more research to identify the conditions under which coalition government is likely to lead to consensus and progress and those under which it will result in immobilism.

The 10 country chapters offer the usual mixed bag. West Germany, Scandinavia, and the Netherlands get two chapters apiece. The first set of chapters (by Klaus von Beyme, Pertti Pesonen and Alastair Thomas, and Jan Vis) describes the politics of coalition formation. The second set (by Manfred Schmidt, Bo Sarlvik, and Ken Gladdish) assesses the impact of coalitions on policy outputs. The chapter written by Geoffrey Pridham on Italy adequately covers the political side but totally ignores policy outputs. The chapters on Ireland and Belgium are too brief to be of much interest to readers with some background knowledge of these countries. Finally, there is an exploratory chapter by Collin Mellors on coalition strategies in British local governments based on the experience of 83 "hung councils" during 1981 and 1982.

What does it all add up to? We know, of course, that in the parliamentary elections of 1983 the Tories won 61% of the seats with only 42% of the votes. So the book's original thesis is not as timely as it once was. There is no overarching theoretical framework to unite the diverse chapters. Bogdanor's chapters comparing the way parliamentary government works in Britain and on the continent are interesting but hardly earth-shaking. Some countries (France, Switzerland, Austria) that have interesting variants on coalition politics have been left out entirely. In America, this book will primarily be of use to specialists on parties in West Germany, Scandinavia, or the Netherlands who will find that the relevant sec-

tions offer a helpful synoptic overview coalition government in these three nations.

JAMES A. DUNN, JR.

Rutgers University, Camden

Constitutional Reform and Apartheid: Legitimacy, Consociationalism and Control in South Africa. By L. J. Boule. (New York: St. Martin's Press, 1984. Pp. xii + 270. \$27.50.)

In 1984 the Republic of South Africa adopted a new constitution that, for the first time, established parliamentary representation for its Coloured and Indian population. Boule, a South African law professor, examines the historical process behind these constitutional changes and analyzes how the apartheid government has invoked theories of pluralism and consociationalism to justify these new structural alterations.

Boule begins his study with a review of modern Western constitutionalism, focusing on the Westminster parliamentary system and the American presidential system. He notes that both systems are premised on the principle of majority rule and an adversarial political context, and that each system includes constitutional checks and balances to prevent majority rule from becoming unrestrained.

Boule's second chapter explores several theories of pluralism that have emerged in Britain and America and concludes that the majoritarian assumptions inherent in these traditions are ill-suited to the present South African government because it presumably will result in permanent political majorities and permanent minorities. Chapter 3 explains that consociationalism has been introduced in South Africa as an alternative to the British and American systems because it provides an opportunity for all groups to be represented in decision-making processes. South Africa's new tricameral parliament represents the reification of this consociational alternative, allowing whites, Coloureds, and Indians their own institutional framework for advancing their respective interests.

Chapters 4 and 5 provide an historical overview of the main constitutional developments in South Africa since it became a union 75 years ago. The constitutional policies of the ruling National party are contrasted with other political parties in South Africa including Inkatha, a Zulu national movement. Boule also provides a comparative analysis of South African constitutional developments with constitutional experiments in Namibia; Rhodesia-Zimbabwe; and the quasi-independent African "homelands" of Transkei, Ciskei, and Bophuthatswana. In addition, he describes the

constitutional debates of several recent commissions that have influenced the rejection of unqualified majority rule in South Africa.

Boulle uses his book to join several other scholars who have questioned the feasibility of the consociational option in South Africa. Boulle notes that there is no voluntary group membership in South Africa, there is no multiple balance of power among existing groups, there are few overlapping memberships among groups, there is no strong tradition of political moderation and cooperation, the country is fairly large in size and population, it has a high international profile, the external threats against it are not uniformly perceived by all inhabitants, and there is a high degree of socioeconomic inequality (p. 65). Not surprisingly, the new constitution falls far short of the consociational ideal, resulting instead in an enormous concentration of power in the state president (and his cabinet), who possesses "sovereign legislative authority" and is immune from any judicial review process. A vote of no confidence by the three houses of Parliament could encourage the state president's resignation, but the new constitution provides the president with the legal option in this circumstance to dissolve Parliament. Boulle reports that even Arend Lijphart, the political theorist most closely identified with consociational theory, has described South Africa's form of consociationalism as a travesty of his conception of the system (p. 185).

In short, Boulle has written a comprehensive, scholarly, and convincing indictment of the constitutional changes that are taking place in South Africa. He criticizes the omission of blacks (who constitute the great majority of South Africans) from the constitutional process, arguing that this precludes the political inclusiveness that consociationalism presupposes to stabilize the system (p. 217). Given the crisis of legitimacy facing South Africa today, one must also admire Boulle's courage in admonishing his government's entrenchment of the apartheid system.

ALLAN D. COOPER

St. Augustine's College

Grüne Politik: Ideologische Zyklen, Wähler und Partiensystem. By Wilhelm P. Bürklin. (Opladen: Westdeutscher Verlag, 1984. Pp. xiv + 270. DM 37.)

In recent years the pattern of party stability and frozen cleavages in most Western democracies has been supplanted by party fractionalization and increasing electoral volatility. One of the most interesting and significant examples of these trends is West Germany, where the established parties

are now challenged by the emergence of the strongest ecologist party in Western Europe, *Die Grünen*. The Greens have attracted the attention of many scholars, but their true meaning is much debated. Some argue that the Greens signify a revival of German romanticism, the youth movements of the 1920s (*Wandervogel*), a conservative protest against too rapid social change, or the new political cleavages of postindustrial societies. Bürklin's study is a systematic attempt to identify the social and political meaning of the Green movement in West Germany.

Grüne Politik is the first major empirical study of popular support for the Green party. Most of the book is based on a series of public opinion studies conducted from 1976 to 1980 as the ecological movement coalesced into the Green party. Additional data are drawn from a survey of Green candidates in the 1980 state elections in Baden-Württemberg and several other opinion surveys conducted by the researchers in Mannheim. Separate from the theoretical interests of the author, this book contains a wealth of information for students of West German public opinion.

Bürklin finds that the popularity of the Greens exceeds their early, or even present, support at the polls. As others have shown, this popular support is heavily concentrated among the young and better educated. However, Bürklin makes an important extension to these findings by demonstrating that the political characteristics that define Green support have a much stronger effect for younger generations. Bürklin cites this consistent pattern as evidence of a generational change in the bases of political conflict in West Germany, a point that has relevance for many other Western democracies.

Another component of the study examines the civic attitudes of West Germans—such as political trust, system support, political efficacy, and acceptance of conflict—and their relationship with Green support. Several political analysts have labeled the Greens as a radical, antidemocratic party. The empirical evidence is more mixed. Green supporters are less positive toward the political system and established political institutions, but at the same time they accept democratic political norms and have high levels of political efficacy. Bürklin reconciles this apparent contradiction by suggesting that Green voters have idealistic views that contrast with political reality. In other words, the Greens may reflect the same type of "creedal passion" that Huntington has described in the American context, and the good and bad consequences that this implies.

This study provides a rich blend of political theory and empirical data. In one area, however, the theoretical framework may be overextended. Bürklin describes the Green phenomenon in terms

of a cyclical model of political change, largely derived from American realignment theory. Young West German voters represent the "third generation" following a realignment and are therefore available for a new basis of political mobilization. This model has great heuristic value, but it can be applied too rigorously; political change does not follow a predictable sine curve, and even a general pattern of idealistic/reactionary conflict overlooks important differences in the specific issues of conflict.

The Green party represents an important example of the political changes now affecting Western democracies, and Bürklin has written a study that deserves a wider audience than just German specialists.

RUSSELL J. DALTON

Florida State University

The British General Election of 1983. By David Butler and Dennis Kavanagh. (New York: St. Martin's Press, 1984. Pp. xii + 388. \$29.95.)

This book is the twelfth in the series, beginning in 1945, of British general election studies sponsored by Nuffield College, Oxford. The authors acknowledge an "obligation to provide material in a form comparable to previous studies" (p. xi). Their study follows a now-familiar format. Individual chapters survey political events since the preceding election; report developments within each of the major parties; follow the activities of the national party leadership during the campaign; analyze opinion polls, the broadcast media, and the print media; and anecdotally discuss candidate selection and electioneering in the constituencies. One appendix reports constituency electoral results; another provides aggregate psephological analysis of the results.

The 1983 election study, like its predecessors, is a successful synthesis of political science and contemporary political history. Its intention is much less explanatory or interpretive—although such material is artfully woven into the discussion—than it is descriptive. Butler and Kavanagh present a variety of interesting insights and statistical descriptions of specific electoral topics rather than a sustained argument or theoretical model. Their observations are invariably careful not to stray beyond the limits of the evidence or of good sense. In keeping with its concept, the book places more emphasis on short-term rather than long-term electoral influences, and more on the impact of personalities and events than on the consequences of social and economic forces. This is not a criticism, for the authors do very well what they intend to do. The study admirably recreates the

experience of the election, which can be appreciated at two levels—among both a general audience and a specialist readership.

If the book has a thesis, it is that the 1983 election was not so much a Conservative party victory as it was a Labour party defeat. Butler and Kavanagh identify three possible reasons for Labour's electoral rejection: (1) the party's poor performance during the campaign, (2) its betrayal of its socialist principles, and (3) its loss of credibility over time from immoderate policy positions and perceived incompetent leadership. They explicitly reject the first explanation—raising questions about the significance of the very phenomenon that their monograph analyzes in detail—and clearly support the third in favor of the second. This explanation is consistent with the findings of electoral research of Crewe and others concerning the long-term decline of Labour issue-partisanship and partisan identification in the British electorate. However, such an explanation makes it difficult for us to understand Labour's lead in public opinion polls from the third quarter of 1979 to the third quarter of 1981. Perhaps polls of voting intention do not elicit valid responses unless an election appears imminent. In any case, an implication of their view is that the "Falklands Factor" or the "Thatcher Factor" were relatively less important to the election than conventionally thought.

Much of the analysis in the *British General Election of 1983* rests upon aggregate survey data and electoral results. A definitive understanding of the election, of course, must await individual-level investigation. But if not definitive, this work is certainly authoritative. Its conclusions shape the received wisdom on the subject. In fact, the Nuffield studies constitute in themselves an important British electoral institution. This institution may actually outlive such other "classical" features of the British polity as its two-party system or its plurality electoral system. (One wonders what Bagehot might have revealed about these studies had *The English Constitution* been written in our day.) Regardless, the Nuffield studies are a singular political institution requiring virtually no reform.

JEFFREY B. FREYMAN

Transylvania University

Absent Mandate: The Politics of Discontent in Canada. By Harold D. Clarke et al. (Toronto: Gage Publishing, 1984. Pp. xi + 193. \$11.95, paper.)

In 1974, 1979, and 1980 general elections were held in Canada, the last two within a period of

eight months. This book is an analysis of Canadian electoral behavior on those occasions undertaken within the larger context supplied by public opinion data from panel studies conducted during the preceding decade and from other sources.

The thesis is stated summarily on page 174: "The political issues associated with the economic crisis of the 1970s and 1980s in Canada arose in a political context unequipped to deal with them." Because governing parties in Canada seldom gain a majority of total votes and gain what votes they do in regional concentration, there is no national mandate; because victory is independent of sustained policy preference, there is no "directional" mandate; because voters' choices indicate conflicting and short-term preferences, there is no policy mandate. Accordingly, the only mandate that can be said to exist is what the authors call a performance mandate; that is, elections empower the government to do something, without specifying what is to be done. When governments fail to perform, as according to the authors they have failed, public discontent increases. Hence the book's title and subtitle.

There are useful figures provided regarding shifts in public opinion during the 1970s, but the argument as a whole resembles nothing so much as a suit constructed by a committee of Laputian tailors.

Stated in commonsensical terms, sophisticated analysis of public opinion data shows that support for long-term change, which the authors seem to think desirable, cannot follow from elections fought by "brokerage parties" over short-term grievances. But anyone who thinks, for example, that the grievances of Western Canada are short-term had better bone up on elementary facts.

Insufficient attention is given to the problem of a mature federal political body gaining political articulation by way of parliamentary institutions that reinforce rather than mitigate regional majoritarianism. There is no analysis of the importance of the reorganization of the Liberal party under Walter Gordon and Keith Davey into an organization incapable of anything but brokerage politics and short-term perspectives. No attention is given to Prime Minister Trudeau's belief in "rational management" of the economy nor to the inevitable and enormous growth that his ideological interventionism implied. There is no discussion of Trudeau's extraordinary obsession with his own version of the future of Quebec—an obsession that enabled him to choose his own successor, in another party, as prime minister. There is no focused analysis of the most important feature of Canadian politics, regional identities and conflicting regional interests that give bite to those identities. There is no discussion of the significance of political duplicity, as practiced by

the Liberal Government in 1974 and 1975 over price and wage controls and in 1980 and 1981 over oil and gas taxes, for the continued health of liberal democracy in Canada.

By assuming that electorates can give various "mandates" to governments, the authors ignore the actual leadership given to the country during the 1970s. By relying so heavily on public opinion and voting data, the authors seem implicitly to have assumed the validity of a plebicitarian notion of Canadian democracy. In a federal, parliamentary regime led for the most part by a man such as Pierre Trudeau, it is not astonishing that the authors found an absent mandate. But it is not self-evident why they were looking for one.

BARRY COOPER

University of Calgary

The State and Underdevelopment in Spanish America: The Political Roots of Dependency in Peru and Argentina. By Douglas Friedman. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 236. \$22.50.)

This is a neo-Marxist exercise in comparative political history that attempts to refute the notion that external factors (i.e., the evolution of the world capitalist economic system) determined the rise of primary export national economies in the postcolonial world. Using Peru and Argentina as case studies, Friedman argues that internal political factors, mainly the competition between emergent national elites for control of the nascent state, determined their eventual insertion into the capitalist world economy as dependent primary good exporters. Political victory allowed specific elites to assume control over the national state apparatus, which they then used to consolidate their positions of dominance in (and the very structure of) the national mode of production. In both cases, the social groups that eventually allied to gain control of the state by the late nineteenth century (when rapid economic growth confirmed the conditions of their position and role in the international economy) were the large land-holding interests and the export-oriented bourgeoisie.

In posing this counterfactual argument against the initial Leninist imperialist premise that sustained early dependency theory, Friedman restates and synthesizes the work of theorists such as Dos Santos, Gunder Frank, Cardoso and Faletto, Amin, and Wallerstein, as well as recent European Marxist polemics over the nature of the state, class conflict, and base-superstructure relationships. His reliance on Marx and more recent authors available in English such as Poulantzas, Miliband, Anderson, and Sweezy to develop this

refutation is undermined by the fact that Friedman ignores the work of an intervening theorist (and *dependista* of sorts) who indelibly influenced the substance of modern Marxist exchanges, and who with several others shared the early quest for non-Leninist alternatives in Marxist theory and practice: Antonio Gramsci.

Even more problematic, although not surprising in light of what has been mentioned previously, is the fact that although it is excellently annotated and broad in scope, the book produces a sense of *deja vu* because it argues a point in a debate that has already been resolved. A number of Latin American scholars have long since incorporated the dynamics of internal political competition and national state formation into theories of dependent capitalist economic development. Several authors (Cotler, Kaplan, Laclau, Lechner, O'Donnell, Osizak, Torres Rivas, and Zermeno, among others) have contributed significant studies that confirm the important role played by internal factors in the complementary processes of national state formation and economic and political development in Latin America.

A clue as to why Friedman would choose to argue an essentially moot (or at least conceded) point can be found in his bibliography and notes. All of his sources are in English. The implication is that he may be unfamiliar with non-English elements in Marxist theory (such as a 1982 edition of *Nova Americana* titled "Forme Storiche dello Stato" that is entirely dedicated to the subject of national state formation in Latin America). More worrisome is the fact that the possible lack of language training not only caused him to miss the original debate on this point argued in Spanish, but it also means that he was unable to document his case studies through use of primary sources such as archival material, government documents, and newspapers, or even non-English secondary sources. In other words, Friedman's entire argument is based on his interpretation of original and translated works by authors with the language ability to conduct primary data research in Spanish America.

That Friedman is able to cull this argument from a large and varied pool of English-language secondary sources is a tribute to his transposing skills and abilities as a rapporteur. Moreover, the book is one of the first English-language studies to discuss this subject at length. Unfortunately, that is more a reflection of the quality of the English-language literature than it is a comment on the book's merits. In addition, numerous grammatical and typographical errors detract from the overall presentation.

Thus, although it may be a helpful introduction to the subject for those lacking reading skills in Spanish or Portuguese, for the field in general this

book constitutes another track on a well-worn road.

PAUL G. BUCHANAN

Naval Postgraduate School

The Brezhnev Politburo and the Decline of Detente. By Harry Gelman. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 268. \$29.95, cloth; \$9.95, paper.)

During the 1970s, as Soviet-American relations entered and left the era of detente, Western views of the Brezhnev leadership and its attitudes toward the Western democracies tended to become increasingly polarized. According to Gelman, by the end of the decade those at one extreme of the spectrum of opinion saw the Soviet leaders as perpetually threatening, crude military aggressors in the manner of Hitler. At the other extreme, particularly in Europe, there were many who continued to believe that these same Soviet leaders showed an unrequited desire for permanent accommodation. Between these extremes, there were opposing tendencies. One current of thought assumed that events in the Third World during the latter part of the 1970s had alarming implications for the West, and that these events signaled a steady growth in the Soviet propensity to expand. An opposing trend of opinion argued that such events were part of a natural process, unpreventable and fundamentally unthreatening, through which the USSR was emerging as a great power on the world scene. One view stressed Soviet gains; the other, Soviet setbacks (p. 13).

Gelman's book, which has a place among the best analyses of Soviet politics and foreign policy during the 1970s—what many experts consider to be a unique period of detente and discord—is an important attempt to clarify the motivations, assumptions, and strategies underlying the foreign policy of the Brezhnev Politburo. Focusing upon the rise and fall of Soviet-American detente, according to Gelman:

In the discussion that follows I argue that throughout the Brezhnev era a profound sense of vulnerability—over both the legitimacy of party rule and the stability of Soviet gains—powerfully reinforced the leadership's felt need to keep pressing for incremental gains within the scope of available opportunities and the limits of prudent risk. In consequence, I contend that from start to finish the consensus of the Brezhnev Politburo was highly pessimistic about the compatibility of Soviet interests with those of the United States. (p. 16)

The Brezhnev Politburo and the Decline of Detente is divided into five full chapters. The first seeks to reconstruct thematically the Soviet leadership's goals and fears in its worldwide interaction with the United States and to isolate those factors that drove the evolution of Politburo conduct during the Brezhnev era. The second examines, among other things, various structural characteristics of the internal Soviet political environment that helped determine the course of the political struggle within the Brezhnev Politburo. Chapter 3 traces certain events in that internal struggle and their subsequent effects upon the attitudes the Brezhnev leadership brought to detente. The fourth discusses the evolution of the Soviet detente relationship with the United States within the historical competitive context of external Politburo behavior from which it first emerged in the 1970s, and into which it eventually returned. Finally, Chapter 5 considers the prospects for Soviet-American relations in the post-Brezhnev era against the background of the "asymmetrical vulnerabilities" of Soviet and American societies (p. 17).

In reference to the international sphere, Gelman thoroughly discusses the rise and fall of the detente relationship and covers many of the factors that influenced its development and decline: the China factor, technology transfer, the SALT process, Soviet calculations in Africa and Southeast Asia, Soviet interaction with Egypt, third-world politics and Soviet opportunities, Soviet interaction with American pluralism, and the invasion of Afghanistan.

The Brezhnev Politburo and the Decline of Detente is a solid work that will be of interest and value to students of Soviet studies, international affairs, and political science, as well as anyone interested in Soviet-American relations during the period of detente and the prospects for East-West relations in the 1980s.

JOHN M. CARFORA

Dartmouth College

The Dynamics of the One-Party State in Zambia.

By Cherry Gertzel, Carolyn Baylies, and Morris Szeftel. Edited by Cherry Gertzel. (Manchester: Manchester University Press, 1984. Pp. xi + 262. \$35.00.)

A truly collaborative effort among three scholars who have spent a number of years in Zambia, this book continues the intellectual tradition established in the two books edited by William Tordoff, *Politics in Zambia and Administration in Zambia*, which were published in Britain by Manchester University Press in 1974

and 1980. This tradition, which can be designated the Manchester school of Zambian political studies parallel to the earlier Manchester school of Zambian anthropological-sociological studies, is both the dominant school in terms of number of publications and the most sophisticated one yet to appear. Furthermore, it is constantly evolving, and the book under review makes significant contributions to that evolution.

The central concern of this book, like that of earlier books in the same tradition, is on the influence of various forms of social relations on changes in political institutions and political conflict. The major innovations in the content of that central concern that are made in this book are a more explicit treatment of class and greater emphasis on central-provincial relations. More concretely, the emphasis of the book empirically is on the interaction of social relations, political conflict, and political institutions in the 1973 and (to a lesser extent) 1978 one-party elections. Candidates, campaigns, voter turnout, central party control, and social and political factors determining results are discussed in detail. The methodology employed is the combination of electoral data and theoretically informed narrative description that is found in all of the Manchester school books.

Having read a number of articles and manuscripts by Baylies and Szeftel, I was surprised that class analysis did not occupy a more central position in the work under review. The probable reason for this is the difference in perspective among the authors that is mentioned in the Preface. One chapter is devoted to "the rise to political prominence of the Zambian business class," but this chapter focuses primarily on the electoral success of candidates with business backgrounds in 1973 and 1978 and does not reveal the sophisticated theory and methodology that Baylies and Szeftel employ elsewhere to determine the nature and influence of this class. Class forces as determinants of political conflict and institutional change are discussed in other chapters, more so than in previous Manchester school books, but remain a secondary theme in most of the book under review.

Almost half of the book is devoted to three case studies of central-provincial relations: Copperbelt, Luapula, and Western provinces. The first of these is shown to have lost much of both its political preeminence and its enthusiastic support for the governing party, UNIP. These trends are accounted for by the economic difficulties of the copper industry, the conflict between the government and the labor movement, the prominence of a partially ethnically based opposition in the province immediately before the introduction of the one-party state, and the substantial

weakening of party organization. Although they have had very different political histories, the Luapula and Western provinces are now quite similar as two of the three most underdeveloped rural provinces. Although support for UNIP has also declined markedly in Luapula and has been weak in the Western province since shortly after independence, the authors do not feel that this rural alienation is as much of a problem for the regime as urban alienation on the Copperbelt because of the greater economic importance of the latter area and the presence there of alternative objects of loyalty—the trade unions.

Although greater theoretical integration and more precise measurement of variables in the study of change in Zambia are to be hoped for in the future, this book makes an important contribution and should be read by all students of the Zambian one-party state.

JAMES R. SCARRITT

University of Colorado, Boulder

Quebec, the Challenge of Independence. By Anne Griffin. (Cranbury, N.J.: Farleigh Dickinson University Press, 1984. Pp. 220. \$27.50.)

The existence of a large and important French-speaking province in the heart of a culturally different Canada offers an attractive subject for comparative scholarship; there is now a growing body of nonpolemical literature on Quebec's political role and—more particularly—on separatism.

This volume by Anne Griffin represents an unusual approach to the topic because it is primarily concerned with the personal responses of Quebecers who support the independence of Quebec. The research question concerns the reasons selected individuals want independence and the way their lives have changed as a result of their political commitment. The main vehicle for participation for most of them is the Parti Quebecois, which was voted into power by the people of Quebec on November 15, 1976 and pursued its quest for autonomy from English-speaking Canada through the next four years. Defeat of the referendum on "sovereignty-association" in May, 1980 was hailed throughout Canada as a statement that Quebecers had firmly rejected separatism but, as Griffin and others point out, it really quieted little of the sentiment for an independent identity. Levesque continued in power and would represent his province at the constitutional talks on patriation, although he would be in the awkward position of attempting to negotiate a constitutional arrangement accep-

table both to the voters of Quebec and the Parti Quebecois.

Griffin carefully documents the uneasy relationship between French and English-speaking Canadians over four centuries of coexistence emphasizing "the politics of survival" and then examines 11 ways of looking at separatism. Intensive interviews and conversations were conducted with 23 individuals, 14 of whom believed in separatism. Eleven of these encounters are described in the case reports that constitute the second part of the book.

Reminiscent of Robert Lane's classic analysis of the political ideologies of "common men," which showed that acts of participation (and non-participation) can satisfy psychological needs, Griffin explores the way in which separatism helps secure the mental well-being of French Canadians. Although she builds a rather elaborate explanation on a slim empirical base, the result is nevertheless valuable.

This book is a useful addition to the English-language literature on Quebec separatism.

KEITH HENDERSON

State University College, Buffalo

The Policy Process in the Modern Capitalist State. By Christopher Ham and Michael Hill. (New York: St. Martin's Press, 1984. Pp. xiii + 210. \$29.95.)

Hugh Heclo's 1972 review article on "Policy Analysis" (*British Journal of Political Science*, 2, 83-108) deserves much of the credit for a booming interest in areas once considered little more than a subset of public administration. Comparative public policy, policy analysis, and program evaluation have since become important research fields in their own right; hence, a state of the art book that also incorporates future directions would be most welcome after a decade of extensive empirical research.

Rather than servicing this need for synthesis and guidance, Ham and Hill's book (which is the result of their involvement with a Master's level public policy studies program at the University of Bristol) amounts to an old-style, albeit fairly well-written, review of the literature. It traces the evolution of organizational sociology approaches by summarily treating a battery of "pioneer works" dating back to Marx and Weber. However, the work tends to neglect the very areas in which the theoretical frontier has expanded significantly over the last five years, for example, in relation to design science and studies of environmentally induced complexity. The authors contend that "there is a need now for good empirical

studies to link the macro and micro levels of explanation" (p. 189) regarding the determinants and consequences of government action; but aside from passing references to "radical organizational theory," they offer few insights as to what form these linkage studies ought to assume. The examples concentrate on intraorganizational dynamics, undermining what would appear to be the main thesis that both the analysis of policy and analysis for policy are constrained by existing external socioeconomic relationships.

The introductory chapter, "Policy and Policy Analysis," sketches distinctions made by earlier theorists with respect to policy content, policy process, policy outputs, evaluation, information gathering, process advocacy, and policy advocacy. We are told that the study of process is the key to understanding the evolutionary character of policy, and that the authors would prefer to see politics prevailing over markets in public decision making. Yet the rest of the text concentrates on the more or less static, self-reinforcing aspects of bureaucratic behavior. Addressing the "Role of the State," chapter 2 discusses Ralph Miliband, James O'Connor, pluralist theory, elite theory, non-decisions à la Peter Bachrach and Morton S. Baratz, and neocorporatism. Chapter 3, "Bureaucracy and the State," makes some effort to link Weberian and Marxian perspectives to elitist and corporatist theory, but it does not explain what a focus on specific actors in the policy process could contribute to our understanding of the transformation of the modern (postindustrial as opposed to the Keynesian?) capitalist state as a whole.

In fact, chapters 4 through 9—"Power and Decision Making," "Rationality and Decision Making," "Implementation Theory," "Organization and Policy Process Studies," "Bureaucrats and the Policy Process," and "Discretion"—lose sight of specifically capitalist constraints altogether: Ham and Hill do not carry out their self-imposed mandate to "give due consideration to the social, political and economic contexts within which problems are tackled" (p. 174). Questions of public finance management are never raised. The lack of attention to specific problems of interinstitutional competition for scarce resources tempts me to paraphrase a once-popular T.V. commercial, "Where's the state?" The cross-cutting influences on the actions of bureaucrats (e.g., conflicts between personal objectives versus organizational goals) described are not limited to a particular ideological system.

The results of the concluding chapter, "Linking Levels of Analysis," are equally disappointing. At best, the authors note their agreement or disagreement with 16 direct quotes and 23 references to other theorists hastily covered in the last 16 pages.

Presumably the new directions for policy analysis rest with the development of "Radical Organization Theory," whereby the most impressive examples of RTO-applied (Charles Perrow, Mary Zey-Ferrell, Michael Aiken, Frank Fischer and Carmen Sirianni) receive no mention in the text. A closing endorsement of other writers' prescriptions (p. 188) is not to be equated with an original contribution: In this case the "art's" progress has been quite understated.

JOYCE MARIE MUSHABEN

University of Missouri-St. Louis

Decolonization in Britain and France: The Domestic Consequences of International Relations. By Miles Kahler. (Princeton, N.J.: Princeton University Press, 1984. Pp. xiv + 426. \$40.00, cloth; \$9.95, paper.)

This book represents a fine combination of scholarship: a meticulous attention to historical sources, a concern for detail and nuance, and an interest in theoretical propositions concerning reciprocal relationships between international events and domestic policies. Its major theme is that Britain and France were not merely actors in their imperial roles, but that their foreign and domestic policies were heavily influenced by the empires they controlled. There is an emphasis on the linkage of a number of variables—international pressures, settler activities, economic forces, and the contagion of developments in other countries—that all help to explain the evolution of party ideologies and government policy choices.

In pursuing his analysis, Kahler clears the field of inadequate or mistaken approaches—these are sometimes straw men he has himself erected—such as Marxism, dependency theory, and the failure of scholars to consider external forces when dealing with domestic changes. Thus, he refers to Machiavelli to bolster his contention that "apart from the literature of imperialism and dependency, societies have for the most part been studied in isolation from . . . each other" (p. 14). Kahler pays considerable attention to the "organizational dimension of ideology" which, in his view, has been consistently ignored (p. 61), in favor of a stress exclusively on the belief system of individuals. Yet his own approach suggests a certain ambivalence. On the one hand, he is influenced by structuralism in emphasizing that policies emerge in a holistic fashion, that is, are responses to preexistent contexts; on the other hand, he has large sections of discussion—and they are among the most interesting parts of the book—of the individual choices made by promi-

nent politicians. His conceptual ambivalence leads Kahler to emphasize both the relative autonomy of the state with regard to its own groups and elites (p. 36) and its lack of autonomy with regard to the external world.

Kahler makes the important point that it was easier for the empire to "penetrate" the French polity than the British one, because the French Right was organizationally more unstable than the British and because its "imperialist ideology" was ambiguous, although later in the book he concedes (but discusses all too briefly) the fact that imperialism was also a component of the Jacobin Left. In dealing with party cohesion, he stresses the notion that the "threat of exit," that is, of adherents leaving one party for another, is greatest in "a multiparty system in which the party face[s] a number of close competitors" (p. 66); this threat presumably explains the relative loyalty of supporters of the Labour and Tory parties, a loyalty that in turn gave these parties a more or less free hand. But the argument can be reversed: In British and other two-party systems, parties tend to resemble each other so that "exit" is not too difficult ideologically, whereas the ideologism that is associated with a multiparty system (as in the Fourth Republic) may be important enough to make switches difficult. Kahler may be on more solid ground in his argument that decolonization was easier for the British than for the French when he cites the fact that (except for Southern Rhodesia) there was much less settler resistance in the British colonies than in the French ones.

There is occasional murkiness of style and logic, for example, (p. 9): "Analysis of the social groups that are particularly sensitive to nationalist appeals and external threat correctly predicts the trials of France at the hands of rebellion within the state." More substantively, there are certain biases that not all readers may share. Thus, in contending that the British political system, in particular the party system, has been more resilient to decolonization than that of France (during the Fourth Republic), Kahler is much kinder to Britain than to France: He is excessively harsh on the SFIO and especially on Guy Mollet, who comes across as an unprincipled opportunist for his alleged failure to utilize his ability to control dissent (p. 225) in behalf of anticolonialist ends.

Conversely, Kahler is rather tolerant of the Gaullists, as when he asserts (p. 77) that Gaullism "did not employ nationalism in its political strategy; it *was* nationalism." This point might be argued, in view of de Gaulle's implied promise—"je vous ai [com]pris . . ."—to the *colons* in North Africa; in fact, Kahler (p. 80) attributes the RPF's opposition to decolonization to its anti-

communism. Similarly, he insists (p. 101) that the Gaullists' embrace of *Algérie française* was not a tactic to bring down the Fourth Republic, but later (p. 102) he refers to "the Gaullist strategy . . . to encourage plans for military invasion of the metropolis . . . in order to cause a collapse of resistance to de Gaulle's return, and, at the same time, to restrain such direct action to ensure that the transfer of power had a semblance of legality." The book contains other interesting details about the evolution of Gaullism from 1958 to 1962 and the efforts at making the movement politically *salonfähig*, to the point where Gaullism as an ideology became "full decolonized" (p. 110).

The treatment of the ad hoc political responses of, and the ideological impact on, the Conservative and Labour parties is detailed in its chronological dimensions, but there are some omissions. Kahler makes only passing mention of Ireland and Palestine; while acknowledging the policies regarding these areas as failures (p. 386), he argues that Britain turned even these into successes by "externalizing" them. There is no discussion of embarrassing instances of imperialist behavior within the Labour party—from the "bleeding heart" paternalism of certain Fabian intellectuals at the turn of the century to the romantic Arabism (and not so romantic anti-Semitism) of Ernest Bevin and other postwar Socialists. (Kahler does mention Bevin's "neomercantilism.") There is only a cursory treatment of the impact of "colored" settlers in Britain on party ideologies and programs or on the pattern of race relations, and there is no discussion of Britain's economic decline—a phenomenon that might possibly be considered a "systemic" failure to adjust to the loss of colonies. Finally, it is curious that in a book devoted to external influences there is no mention of Anthony Eden's resignation as prime minister, let alone of any possible American influence on that event.

But these are minor shortcomings that do not detract from the quality of this comprehensive and engrossing work, which is a major contribution to an understanding of the impact of decolonization of the colonizers.

WILLIAM SAFRAN

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The Decade of Energy Policy: Policy Analysis in Oil-Importing Countries. By Paul Kemezis and Ernest J. Wilson, III. (New York: Praeger Publishers, 1984. Pp. xvi + 271. \$27.95.)

The oil production cutbacks and embargo of 1973 opened a decade of rapid and unpredictable

fluctuations in world energy markets. Kemezis and Wilson describe the consequences as an "energy policy explosion." The governments of the more than 100 oil importing countries had shared objectives: "to obtain secure energy supplies from abroad at reasonable prices in the short term while promoting increased domestic production, efficient use and fair allocation of energy in the long term" (p. 29). Despite the similarity of the objectives, policies aimed at achieving them differed greatly across countries. The goal of *The Decade of Energy Policy* is to describe and explain the variety in policy outcomes.

One example of this variability concerns short-run policies aimed at securing supplies in the short term. Some governments merely expanded their information services; others negotiated directly with producer governments; some took over the purchasing of oil completely. Factors including "relations with the major companies, world political posture, and attitudes toward state-controlled companies" (p. 80) are used to compare the policies of France, Italy, Japan, West Germany, Denmark, Brazil, Korea, and Kenya, among others (pp. 96-102). A deeper theme running through this discussion is that political means emerged to replace economic arrangements rendered unreliable by the volatile world market.

In the latter part of the book, the authors review the array of policies governments used to enhance domestic energy supplies (pp. 118-177) and reshape or restrain demand (pp. 178-214). The closing discussion addresses directly the politics and administration of energy policy. The authors show how in many countries energy policy both became a national ideological and political issue and was itself shaped by other such issues. The lines around which group conflict over energy policy was most likely to emerge included producer-consumer differences, competition between proponents of different energy sources, supply-side versus demand-side philosophies, the issue of whether to shift policy costs or benefits forward to consumers or back to producers. In effect, the "energy policy explosion" reflected in part the politicization and bureaucratization of what had been an essentially technocratic decision-making process (pp. 218-219).

A particular strength of this book is that it is rich in facts, insights, and interpretations concerning the energy policies of oil importing countries during the decade from 1973 to 1983. Clearly drawing on a wealth of first-hand information, Kemezis and Wilson make good their promise to detail the differences among national policy responses to the rapidly shifting energy market in its many economic and political aspects. Also, the book is well integrated. The authors regularly link specific discussions to the overarching theme of

increased government influence in energy.

At the same time, there are certain things the reader will not find. First, the authors choose to review energy policy one issue at a time, rather than one country at a time. The reader interested in a coherent overview of the oil policy process in a single country will have to dig for it. Another point has to do with inattention to details of language; obvious examples appear on pages 40 ("most unique"), 93 ("principle area"), 106 (sentence fragment), 148 (the "affect" of taxes), and 179 ("the . . . impacts . . . is why").

Notably absent is any direct reference to the theoretical literature of organizational theory and policy analysis. Every one of the more than 120 bibliographical listings deals with energy policy. The wealth of fact and analysis in this book asks for integration into a broader conceptual framework, but none is suggested. One candidate might be the typology Gary Wamsley and Mayer Zald offer in *The Political Economy of Public Organizations* (Indiana University Press, 1976). Kemezis and Wilson offer numerous insights into the interconnections of internal and external political and economic factors affecting policy outcomes. Reference to a broader synthesizing framework could extend the appeal of this book beyond audiences concerned mainly with the energy policy process itself.

In sum, the reader seeking to be sensitized to and informed about the interplay of factors shaping energy policy in a range of oil importing countries after 1973 will find this book insightful and helpful.

JOHN HEILMAN

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Wagnerism in European Culture and Politics.

Edited by David C. Large and William Weber.
(Ithaca, N.Y.: Cornell University Press, 1984.
Pp. 361. \$34.50, cloth; \$14.95, paper.)

The figure of Richard Wagner remains on the periphery of this fine book that has as its real subject not Wagner, but *Wagnerism*—the cultural, intellectual, and political movements inspired by Wagner. Loosely based on his music dramas and his pronouncements as self-appointed cultural and social messiah, a significant if diffused Wagnerism pervaded European intellectual life in the last third of the nineteenth century and the first two decades of the twentieth. As the editors point out in their introduction, the individuals and groups attracted to Wagnerism "shared deep reservations about aspects of their society and culture and were looking for a vital new alternative" (p. 16). Disillusioned with positivist

thinking, political liberalism, religious orthodoxy, and conventional sexuality, the Wagnerians saw this alternative embodied in one or another aspect of Wagner's legacy.

This book presents the most thorough survey of Wagnerism available in English. After an opening essay by William Weber on "Wagner, Wagnerism, and Musical Idealism," the core of this survey is a series of essays that explore the personalities, ideas, and institutions of Wagnerism in six nations. David C. Large writes on "Wagner's Bayreuth Disciples," Gerald D. Turbow on "Art and Politics: Wagnerism in France," Marion S. Miller on "Wagnerism, Wagnerians, and Italian Identity," Bernice Glatzer Rosenthal on "Wagner and Wagnerian Ideas in Russia," and Anne Dzamba Sessa on "At Wagner's Shrine: British and American Wagnerians." The image of Wagnerism that emerges from these essays is much more complex than the currently accepted association between Wagnerism and Nazism. David C. Large documents the undeniable connection between Wagner's Bayreuth circle and Nazism, but the other essays suggest a more varied political character in other countries, including a belated infatuation with Wagnerism by the Bolsheviks through the late twenties.

Although this book argues for the appreciation of Wagnerism in any comprehensive account of European ideas in this period, it never overestimates its subject. Wagnerism is depicted as a spent force by the end of the twenties, and its disastrous resurrection by the Nazis will probably preclude any future revivals. Moreover, even in its heyday, Wagnerism was no more than a thread in the fabric of European thought and culture. Wagnerism never supplanted liberalism, Darwinism, nationalism, or socialism, but rather mingled with these ideologies to produce intriguing nuances and combinations.

This book avoids the usual pitfalls of anthologies. *Wagnerism in European Culture and Politics* is not an uneven collection of vaguely related essays, but a well-conceived and well-executed piece of cooperative scholarship. The writing is clear, and the scholarship is scrupulous throughout. The editors' Introduction and Conclusion are cogent and helpful. And the ample notes are a godsend for future work on this and related subjects. I highly recommend this book.

JOHN BOKINA

Pan American University

Ideologi och Strategi: Svensk politik under 100 ar.
By Leif Lewin. (Stockholm: Norstedts, 1984.
Pp. 413. SEK 136.00.)

For several years Uppsala scholars have made studies of Swedish politics "as rational conduct," and this is the final, concluding work. It is an ambitious achievement. It examines eight momentous policy decisions: protectionism (1888), extension of the franchise (1907), parliamentarism's acceptance (1917), the "horse-trade" behind the full employment program (1933), the acceptance of welfare state, mixed-economy planning (1948), supplementary pensions (1958), nuclear power (1976-1980), and wage-earner funds for purchase of corporate stocks (1983).

The choice of cases, one must suppose, did not hinge solely on the wish to test a hypothesis. Lewin chooses matters of historic importance and writes a dramatic account of each to produce a book attractive to the general reader as well as the specialist.

Lewin's choice of analytical tool is game theory, which he applies to each case. He lays out matrices to show choices and payoffs open to the political actors. He strives to show that the choices actually made were what one might have expected of committed, calculating human beings. Thus were the political actors rational and the outcomes explicable.

The explanations have the character of post hoc rationalizations, which means they cannot be tested. Lewin however claims no more for game theory than that it provides an "ideal type" by which to take stock of politics in the real world. The great virtue of this approach is that it advances the cause of seeing politics as politicians themselves do.

Lewin denies his analysis is normative, and indeed it is true that he takes a detached, neutral view of the issues. Nevertheless, he has an obvious liking for the rules of the political game that have evolved in his country. A trace of cultural bias is also evident in the special meaning of "ideologi" in the book's title; "ideologi" here means principles and convictions, not creeds and dogmas. Accordingly, a flexible, pragmatic party that adopts a bargaining "strategy" to fulfill half of its "ideology" (principles) exhibits "rationality" in a way in which an inflexible, uncompromising party does not. This analysis fits the Swedish case, but not necessarily others where cultural traditions differ.

Lewin draws extensively on recent game theory innovations such as Norwegian scholar Jon Elster's refinement of the prisoner's dilemma into a solidarity game. He applies this to show how opposing interests in the late 1940s found it mutually advantageous to cooperate in laying the basis for

Sweden's mixed, semiplanned economy. The account is elegant, and it causes one to think of Robert Axelrod's speculations in this journal on the evolution of cooperation in human society in general.

This book is both simple and profound in the best sense of those terms. It appears naive in that its game theory element is simple and mathematically unembellished. But it is profound in that it treats political choices seriously. In a masterful concluding chapter Lewin discusses the chimeral nature of majority rule, which all revere but which does not work unaided. The credit for getting good results out of majoritarian decision systems is what Lewin (invoking Machiavelli, Max Weber, and Herbert Tingsten) is prepared to give to foxy politicians.

BERTIL L. HANSON

Oklahoma State University

Civil Religion in Israel: Traditional Judaism and Political Culture in the Jewish State. By Charles S. Liebman and Eliezer Don-Yehiya. (Berkeley: University of California Press, 1983. Pp. x + 305. \$24.95.)

This is a book about the divergent elements of modern Jewish nationalism-Zionism. Nationalism, as George Orwell reminds us, can turn into many things:

When nationalism first became a religion, the English looked at the map, and, noticing that their island lay very high in the Northern Hemisphere, evolved the pleasing theory that the further north you live the more virtuous you become. (*The Road to Wigan Pier*, Penguin Books, 1962, p. 100)

Has Zionism become a religion? Liebman and Don-Yehiya believe that in the past, the following "civil religions" dominated: Zionist-Socialism, Statism—and over the last 30 years—a "new civil religion." These three "religions" comprise the main subdivisions of the book, which focus on the impact of traditional Judaism, and on the values, symbol systems, myths, rituals, and heroes in each.

This analysis of the intricate mosaic of Zionism provides, for instance, a look at the conflicting values within Zionist-Socialism or the changing roles of Judaism in Israeli political culture. The authors also observe that recent developments in Israel affirm:

... the notion of public rather than private observance... Whereas Judaism places man's obligations to God at the center of its value

system... the new civil religion places the individual's obligations to the nation at the center.

This is a valuable *political* insight into the so-called "revival of religion" in Israel after 1967.

But why call it a "civil religion"? Civil religion is like traditional religion, we are told, except that at its core stands a corporate entity rather than a transcendent power. Although God has been removed, and "there can be societies without a civil religion" (p. 11), the authors suggest that Israel is a "visionary" rather than a "service state," and therefore "requires a civil religion" (p. 216). This observation could have been sharpened by stating that the history of Israel shows that Israel needs an "overarching, integrative symbol system" (p. 11), and that after experimenting with different symbol systems, the *current* emphasis is on shared Jewishness and Jewish continuity.

Another problem is whether one can explain the development of Israel's political culture in terms of "religious" encounters alone. Both conflicting and identical elements exist in each "civil religion"—without dividing them into artificial stages. Zionist-Socialism has been, from its inception, both universalistic and nationalistic (see, for example, the discussion of May Day celebration); secular and traditional (in the cultural sense); and even "statist" before the state was established, having strong affinities with the disguised *étatist* sentiments of the New Hebrews movement. Statism adopted elements of extreme secularism, but also embraced traditional references and a notion that Israel is the expression of the national Jewish spirit. The new "civil religion" combines old and new elements but, unlike its predecessors, "lacks a coherent ideological formulation" (p. 135). In short, the familiar story of continual groping; the uncertain evolution of Jewish nationalism in the modern era. My argument, therefore, is not just with the artificial stages, but primarily with breaking down the process of Israel's political-cultural evolution into different "civil religions."

Based on the excellent material presented in this book, the central problem of modern Jewish nationalism appears to be the "loyalty blend": how much of the Judaism, Jewishness, and "the Jewish people" concept can be blended with the secular, modern, democratic nation-state concept. In the most important secular movement of the pre-State era, Zionist-Socialism, a combination of universal and nationalistic elements, prevailed. Revisionist Zionists presented a more simplified and romantic notion of nationalism—a combination of tribal Jewishness and *étatism*. The ultra-orthodox Jews completely rejected modern nationalism, while the more moderately religious

have tried to cope with it in different ways (p. 185). After the Jewish State was established, crude state-nationalism prevailed for a while as was to be expected, but a complex "loyalty blend" soon reemerged. Thus, the new "civil religion" is an attempt to reconcile the identities of "Israeli" versus "Jew" by relying more on tradition and on Jewishness than in the past.

But have we resolved the schism? Is there a clear-cut distinction between statism and the current situation? I find this new "civil religion" both strongly statist and nonstatist. Some religious Jews, for example, have become more extreme in their rejection of the State of Israel, while others have developed messianic visions colored by a fanatic chauvinism that is political, that is, tied to the state as the vehicle of redemption.

The authors assert that Israel, like other Western societies, has experienced "a decline in nationalist sentiments" (p. 136). Modern Jewish nationalism is hardly 100 years old, however, and the searching process continues. Recent trends, including the growing importance of traditional Jewish symbols in Israel, are as precarious as the previous ones. After all, God has reentered, but in name only (p. 136), and one may even say as a national hero. The emphasis on Israel as a Jewish state has been accompanied by a crisis within the religious camp itself. Hence, we may yet witness a neo-civil creed, a rediscovery of some of the old, universal elements, a return from chauvinism—religious or secular—to "normal nationalism."

"We are not happy with the new civil religion," the authors conclude (p. 237), and I share their general sentiment. I think, however, that the case could have been argued differently. Nevertheless, this is an important book because it raises some of the most crucial questions about the political culture in Israel.

ITZHAK GALNOOR

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Democracies: Patterns of Majoritarian and Consensus Government in Twenty-One Countries.

By Arend Lijphart. (New Haven, Conn.: Yale University Press, 1984. Pp. xv + 229. \$22.50, cloth; \$6.95, paper.)

Lijphart's study focuses on 21 democracies which, using a variety of formal and informal institutions and practices to turn citizen preferences into policy, have continually existed for the last 30 to 35 years. The primary objective is to present the major institutional characteristics of these regimes in such a way that one is able to systematically compare them, drawing nine criteria for comparison from two abstract models of democracy:

the majoritarian model and the consensus model. On the basis of this systematic study, Lijphart categorizes these democracies into groups of majoritarian and consensus regimes and explains why democracies belonging in one group use institutions and practices which, according to the two abstract models, seem to be theoretically inappropriate.

Although Lijphart achieves this objective, using tables to summarize his findings, his attempt to analyze and compare patterns of government of 21 democracies in 244 pages leads to some difficulties, most of which can be attributed to a lack of detail. For example, because the objective is to analyze the patterns and regularities of the 21 regimes, one might expect that each regime would be given equal treatment in the discussion accompanying the tables. Unfortunately, such is not the case. Relatively little attention is given to some of the democracies, especially Japan and Israel.

A more important problem is found in Lijphart's selection of the 21 cases of successful democracy. Although he recognizes that "France is a somewhat doubtful case because its regular democratic processes were briefly suspended during the transition from" (p. 39) the Fourth to the Fifth Republic, France is still included. In fact, Lijphart includes both republics as separate examples of equally successful democracy even though neither strictly fit his criterion for inclusion in the study. It is not clear why two other democracies that he recognizes as marginal cases, India and Costa Rica, could not have been included in the same way.

The inclusion of France points to the main difficulty in this study: the handling of another theme that is advertised on the back cover as the major theme of the book, "There is more than one way to run a successful democracy." The problem lies in defining successful democracy as any democracy with a longevity of approximately 30 to 35 years. Although this definition provides a clear set of cases for evaluation (with the exception of France), the argument is circular. Lijphart has chosen to analyze democracies which, by definition, are successful instead of demonstrating how the different institutions of majoritarian and consensus democracies contribute to the longevity of a regime. Moreover, as the inclusion of the French Fourth Republic indicates, his definition of success does not seem to be closely related to regime stability. There are numerous examples of failed democracies (some of which are mentioned in the study) which existed long enough to fit Lijphart's criteria of success.

An understanding of why these 21 democracies can be considered to be truly successful requires an analysis of why a large number of democracies have failed to survive, or have tended to be

unstable. An important question that could be addressed by comparing these 21 regimes to failed democracies is, Which tend to be more stable: majoritarian or consensus democracies? If both models appear to be equally stable, why have these democracies survived and others failed? If both are equally unstable, what accounts for the longevity of the 21 democracies analyzed in this study? Lijphart seems to point to some answers early in his study, but those answers have little to do with the institutional patterns that are the focus of his book.

DAVID R. HERRON

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Environmentalists: Vanguard for a New Society.

By Lester W. Milbrath. (Albany: State University of New York Press, 1984. Pp. vii + 180. \$29.50, cloth; \$9.95, paper.)

Milbrath's book is an analysis of a three-nation survey (the United States, England, and Germany) conducted in 1980 and 1982 which focused on the general knowledge, concern, and commitment to environmental issues by elites (political, business, labor, and media leaders) and the general public. Milbrath's fundamental thesis, based upon these surveys and other public opinion polls, is that a silent revolution is taking place in these three nations, with the emergence of an environmental vanguard. Moreover, Milbrath envisions a new, dominant belief structure, labeling it the New Environmental Paradigm (NEP) that in his view is gradually replacing the Dominant Social Paradigm (DSP). Furthermore, Milbrath argues that the most significant dimension of politics is now the constellation of environmental attitudes of mass publics rather than the traditional left-right continuum.

Milbrath establishes a typology of environmentalists (ranging from "The Rearguard" through the "Undecided Middle" to "The Vanguard") and their environmental sympathizers. He argues persuasively that occupational status rather than social class best explains the differences in environmental concern of these various groups. Individuals employed in the service industries in the economies have adopted the NEP while those employed in the "production sector" remain committed to the DSP.

Contrary to most recently published works, Milbrath remains guardedly optimistic about the future of the environmental movement. He cites as evidence past successes such as the population writers, the impact of Rachel Carson's *Silent Spring*, the environmental legislation of the 1970s, the recent electoral success of the Greens in Ger-

many, and the continued support for environmentally sound policies in his survey data. He recognizes that the political mobilization of environmental groups is not sufficient to change attitudes, but sees future ecological "crises" and acute resource shortages, especially in energy, as providing the catalyst to the adoption of the NEP. No doubt, Milbrath would point to the disaster in Bhopal, India in December, 1984 as a classic example of such an environmental catastrophe that will further enhance the coming of the NEP.

Milbrath has produced a book sure to be cited by environmentalists. This book is one of the few survey research studies published that provides the reader with the extensive detail regarding sampling procedures, the complete survey instruments employed, and the elaborate breakdown of the tabulated data so necessary for an accurate assessment of the reported research. In spite of the soundness of Milbrath's methods, many of his inferences about the future success of the environmental movement rest more on his hopes and aspirations for it than on the empirical data. The fragility of the data is of special concern in light of the fact the surveys were completed only in the two years 1980 and 1982. Because I share many of Milbrath's values, I hope he is correct; however, I should like to see his surveys replicated in the near future to corroborate his findings and give more credence to his interpretation.

DAVID D. DABELKO

Ohio University

The Politics of Women and Work in the Soviet Union and the United States: Alternative Work Schedules and Sex Discrimination. By Joel C. Moses. (Berkeley: Institute of International Studies, University of California, 1983. Pp. xi + 181. \$9.50, paper.)

Alternative work schedules have become an important policy option for organizations since the 1970s. Joel C. Moses's book analyzes the development and initial implementation of alternative work schedules in the Soviet Union and the United States. Within the context of comparative public policy, Moses seeks to compare the political and economic factors influencing the development of public policy concerning alternative work schedules. This important comparison explores issue salience, interest groups, political factors, and opposition for two countries which, although supposedly very different in ideological philosophy and political processes, developed similar public policies with respect to alternative work scheduling.

According to Moses, alternative work schedules

(AWS) encompass any modification of labor policy requiring that all regular employees adhere to the same prescribed times and number of hours worked daily or weekly. Common types of AWS include staggered work hours, flexible work hours, compressed work weeks, permanent part-time employment, and work-sharing (p. 7). Moses indicates that there are basically three reasons why organizations (including governments, of course) implement alternative work schedules: increasing efficiency of current workers, surplus in the labor market, and the concerns of women. In addition, Moses mentions four trends that have influenced employment policies in recent years: inflation, increasing percentage of adults in the labor force, policies to conserve resources, and automation. According to Moses, these factors differ in relative importance in the Soviet Union and the United States.

By comparing U.S. federal policy with USSR all-union policy, Moses concludes that policy substance is converging, but that the political processes for policy determination differ significantly. In research based on documents and interviews in both the United States and the Soviet Union, Moses concludes that, while in both countries the debate was relatively nonpolitical and low in profile, policy development in the United States has advanced due to the pressure of women's organizations, whereas policy development in the Soviet Union has advanced due to economic necessity. This conclusion seems suspect for methodological reasons. It is far easier to obtain information about interest group activity from the United States; whereas in the Soviet Union interest group information is lacking, and it is easy to fall back on a residual category of "economic causes."

Moses's technical, yet unconvincing argument rests on a rather free interpretation of demographic data, government reports, and interviews. Although the notes are extensive, the book lacks a bibliography. Although titled *The Politics of Women and Work*, the book might have been titled *The Economics of Employment, with Occasional Reference to Women*. With the changing roles of women, the uncertainty of the impact of employment policies on the well-being of women, and the increasing interdependence and communication among world-wide policymakers, such a book is crucial to our understanding of women and work. Unfortunately, this book presents women more as passive receivers of policy than as active participants in the policymaking process. Even the description of the process in the United States, which is twice as long as the section on the Soviet Union, emphasizes contextual economic considerations more than the roles and interests of women. Moses's book features the U.S.A. at the

expense of the USSR and the economic at the expense of the political. It is an interesting venture in the area of comparative public policy, but it does an injustice to sound methodology and to women and their concerns.

JEANNE-MARIE COL

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Bureaucracy, Education, and Monopoly: Civil Service Reforms in Prussia and England. By Hans-Eberhard Mueller. (Berkeley: University of California Press, 1984. Pp. xi + 301. \$37.50.)

The Making of a State: Württemberg, 1593-1793. By James Allen Vann. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 321. \$29.50.)

The historical growth of government is usually taken to be the inevitable outcome of impersonal forces. These two books challenge those orthodox assumptions of inevitability and impersonality. Governments did grow, and they did change as they grew, but that growth and change required the efforts of many individuals, and their efforts were not always successful.

Vann specifically warns against a linear reading of the history of the state. Because of the time span his book, *The Making of a State*, covers, he has ample opportunity to demonstrate the false starts, the failures, the drifting, and the regressions that occurred. Vann's study of the making of the state of Württemberg emphasizes the development of a professional bureaucracy. Like Mueller, Vann is frequently at odds with conventional interpretations of his subject. On bureaucracy, he argues that the Württemberg case indicates that "the modern bureaucratic state grew in a more uneven fashion than has heretofore been appreciated" (p. 86). That observation is based on events before the Thirty Years War (1618-1648), but it is a theme that Vann returns to later.

By the eighteenth century, a bureaucratic ethos had emerged in the Württemberg civil service according to Vann. The ducal household was gradually separated from the public treasury. Officials came to see themselves as public servants and not ducal servants. To them office was not a property, but a vocation based on service, thrift, discipline, and work. If that account makes these officials sound like members of the bourgeoisie, it is because that is the conventional account. But Vann's research in Württemberg indicates that these civil servants were aristocrats more often than not. "Their participation in" the development of the professional bureaucracy "warns us against making too close an association of

bureaucratic growth with an upwardly mobile, urban, middle class group" (p. 196).

Equally important is Vann's argument that Württemberg absolutism was not a conscious plan of state building as is sometimes said of French absolutism. In Württemberg, it was a maneuver in the constant conflict between the crown, the estates, and the commons. Vann clearly demonstrates that the desire to imitate the Versailles of Louis XIV was one of the duke's many motivations to centralize authority and undercut the estates. The drive of Württemberg's monarchs to reform against the resistance of the estates occasioned Hegel's essay on Württemberg politics in 1815. By that time, Hegel notes, Napoleon had made a few reforms of his own in Württemberg, remarking to a Stuttgart politician that "I have made your master a sovereign, not a despot." On Vann's showing, a great many citizens of Württemberg, including some of those masters, had been working to that end for years.

Bureaucracy, Education, and Monopoly consists of comparative case studies of the civil service reforms in Prussia of Stein and Hardenberg after 1806 and in England after the Trevelyan-Northcote report of 1854. Mueller's argument is that because of the reforms, certain strata in both countries monopolized access to the public service through education. Setting aside the functionalist explanation in his introduction, Mueller favors conflict theory. According to conflict theory, the reforms were the outcomes of a struggle, not the inevitable outcomes. In Prussia, Mueller concludes, the rising bourgeoisie secured a monopoly on bureaucratic offices because education became the main criterion for selection. Of course, some aristocrats were the architects of these reforms. Hardenburg is said to have referred to these wide-ranging reforms as the revolution from above. The reforms were embraced by some aristocrats and by the king as the means to modernize Prussia in face of the challenge of revolutionary France. The numerous universities in the realm had long been the province of the bourgeoisie, as the army was of the aristocracy. In this case, the association of the bureaucracy with the bourgeoisie is borne out.

It was a different matter a generation later in England. There the aristocracy retained its monopoly on bureaucratic office by promoting education as the criterion of selection and recruitment. There were only two universities in England, and they remained the private property of the aristocracy. If all aristocrats were not educated, all who were educated were aristocrats. Political patronage and not reform, Mueller argues, would have made the bureaucracy more representative of the populace more quickly. Extensions of the franchise in the period would have

meant that politicians securing their seats would have had to have appointed commoners. This argument obscures the relationship between the reforms of the bureaucracy and the wider reform movement of the era. At the time of the first English Reform Bill, an observer like Hegel had already concluded, as did John Stuart Mill, that the application of education as the criterion for civil service recruitment and the extension of the franchise were cut from the same cloth. But if that were true in England, Hegel would have to explain why it was not true in Prussia. Mueller concludes that *carrières ouverte aux talents* was not advocated as an end in itself in England, or in Prussia for that matter. Nonetheless, it may have been a good in itself. Mueller closes by emphasizing that the English and Prussia roads to meritocracy had nothing to do with democracy. In England the aristocracy used merit to cling to office, while in Prussia the autocracy used merit to shore up its rule. In both countries whenever there was a conflict between status and merit, the powers that were always favored status. Consequently, the changes that occurred were neither inevitable nor impersonal. There was much stopping and starting, and the decisions of particular individuals often did much to advance or retard the cause of reform.

M. W. JACKSON

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Politics and Dependency in the Third World: The Case of Latin America. By Ronaldo Munck. Zed Books, 1984. Pp. x + 374. \$35.50, cloth; \$13.95, paper.)

Before plunging into this volume in Zed's Third World Series, the outsider might consider a guide to the radical literature: Paul Attewell's "sociology of knowledge analysis," *Radical Political Economy since the Sixties* (Rutgers University Press, 1984).

Attewell maintains that radical scholars pursue three competing tasks: to maintain the Marxist paradigm, to analyze current events, and to engage in a "moral-evaluating critique." His book is too recent to have classified Munck, but Munck's purpose is clearly to incorporate the usable insights of dependency and world-systems theory into the Marxist paradigm—and then apply the result in what he considers an empirical fashion by fitting it to the past, the present, and future possibilities.

That is, Munck's version of the paradigm. He cites bourgeois writers, the ECLA school, dependency theorists, world-system aficionados and an assortment of known and lesser Marxists.

Marx, Lenin, Luxembour, Trotsky are generally quoted favorably, the others when they fit (and debated when they do not). His analysis is meant to be applicable to the Third World, a concept which itself is redefined, but there are few references to areas other than Latin America, and there mainly to Argentina, Brazil, Chile, Central America, and Cuba.

Munck's review of events in Cuba perhaps conveys the flavor of the whole. Cuba can go either way—further along the bureaucratic road or toward more genuine socialist democracy. The moral-evaluatory critique: For Cuba's bureaucratic, less than democratically socialist direction blame the pre-Castro inheritance, reliance on the Soviet Union more than Soviet pressure, and Castro's choices. Moreover, more needs be done for women: Patriarchal relations persist.

Back to dependency theory: Where is it useful, where not? Dependency writers provide a critique of nonradical modernization and development theories (good) but have tended to an abstract conception of the world economy (bad). "At its best," this school "points toward historical analysis focused . . . on social classes" (p. 35). Munck surveys stages of "dependent development" in third-world economies in their changing relations to the central capitalist economies. But external factors need be focused, however, on class relations and struggles in the Third World itself, however externally conditioned.

Brief histories of urban and rural social movements follow—labor in Argentina and Brazil, peasants in Chile—to demonstrate that empirical analysis is properly focused, not on classes in isolation, but in relation to others, that is, class struggle, actual, incipient, or thwarted. This section, together with the next, "Politics," comprise the largest part of the book.

The section on politics serves to illustrate neo-Marxist and non-Marxist concepts worked into Munck's schematic histories: *comprador* regimes (Central America) with their enclave economies and personalist states; nationalist and military regimes in Peronist Argentina and since employing the concept of bureaucratic authoritarianism; and Brazil as a more dependent capitalist state than Argentina with a smaller and less activist working class, in a state consecutively oligarchic, Bonapartist, and military. Each history examines specific classes in relation to others and in specific circumstances, but the state is not viewed as a mere epiphenomenon of a static ruling class.

Who will want to read this book? It is meant to provide a Marxist mode of analysis broadly applicable to the dependent capitalist countries of the Third World, but the reader will have to look elsewhere for extended application to Africa and Asia, to which Munck makes only few references

(which are hard to locate without an index). Those of us who do not ourselves usually plow Marxist fields might well profit by a reminder that we do not live in a classless world and that class still has explanatory clout, even in socialist countries.

ELISHA GREIFER

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Communism and the Politics of Inequalities.

Edited by Daniel N. Nelson. (Lexington, Mass.: Lexington Books, 1983. Pp. ix + 291. \$30.00.)

Daniel N. Nelson, prolific author as well as editor of works on East European politics, has assembled here a group of comparative and case studies dealing with political inequalities in communist states. This is an important topic, yet the resulting volume falls short of meeting its ambitious objective, that being "to address the political antecedents, consequences, and correlates of socioeconomic inequity in communist states" (p. vii). It seems to me that of the probable reasons for this shortcoming, the most basic is that the concept of "inequality" is perhaps too broad and unwieldy. Despite its apparent and intuitively appealing promise of pulling together the essential elements of communist political systems—the official Marxist-Leninist theory, public sentiments of legitimacy, economic performance of these systems, and communist international relations—it at once explains too much and too little. Further undermining analysis is that here the concept is neither developed so as to fit into a theory nor placed satisfactorily in a theoretical (and hence explanatory) context. The sole exception is Susan Woodward's chapter on Yugoslavia, which sensibly links inequality with political instability. Except for a passing reference to Ralf Dahrendorf, there is no effort either to call into service, or else to refute, the theory of class conflict. This is a strange omission in a book on political inequality. There is a similar passing reference to Karl Deutsch, yet no appropriation of theories of ethno-politics to help explain ethnic relations. Consequently, the key term *inequality*, although adequately defined, is used more as a common starting point for the various contributors rather than as a concept in the strict sense. It is understandable that the contributors focus on different facets of (and use a variety of) approaches to study this aspect of communist politics, but it is also understandable that no

palpable answer emerges to the central question concerning what "the politics of inequalities" are.

It is gratifying to see scholars of communist systems attempting to integrate elements from other fields of political science into their approaches. In this volume, for example, Valerie Bunce uses dependency theory to explain the effect of international inequalities on domestic, internal ones; Dan Nelson uses the development theorists' notions of "crises and sequences"; and John D. Robertson and David M. Lampton draw on the public policy literature to analyze Soviet and Chinese welfare policies. This is commendable, because thus we are usually offered better explanations, not just the same old wine in newer, fancier bottles.

There are limitations to this integration, as the authors themselves concede more or less explicitly. The connection, for instance, between dependency and inequality of Eastern Europe with regard to the USSR is not fully established or demonstrated by Bunce, although it is plausible enough. Her conclusions, like those of most of her co-contributors, are very cautious. Resort is made to metaphorical terms to aid in the explanation. Two of the writers concede (pp. 160 and 184) that the end of their chapters should properly be but the beginning of research on the matter in question. Clearly, this book is an advance on what has been done; it is not by any means the last word.

The most systematic essay is by Cal Clark, although more straightforward prose would have helped to convey meaning better. Does Yugoslav decentralization, after all, mean more or less inequality, or more or less equality? What does Yugoslavia illustrate? No such ambiguity haunts Jack Bielasiak, whose chapter on Poland is the most interesting and satisfying of the volume. This is because he not only measures economic inequalities, but he also demonstrates by reference to public opinion surveys that these are perceived as significant. Hence inequality is shown to be politically relevant, something few of the other contributors are able to do, usually because of lack of data. Most of the other authors are able to describe the existing inequalities statistically, but are less well able to assess their political effects.

Other contributions are by Donna Bahry on the political versus economic inequalities of Soviet republics, Mary Ellen Fischer on ethnic relations in Romania, and Sharon Wolchik on the mixed effects of federalism upon relations between Czechs and Slovaks since 1968.

The strict balance between genders among this book's contributors, incidentally, is welcome evidence of an important kind of equality among the practitioners of comparative communist studies, even though a comprehensive explanation

of this vital relationship in the countries they explore as yet eludes their grasp.

BOHDAN HARASYMIW

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Ulster's Uncertain Defenders: Protestant Political, Paramilitary, and Community Groups and the Northern Ireland Conflict. By Sarah Nelson. (Syracuse, N.Y.: Syracuse University Press, 1984. Pp. 219. \$32.00.)

Although social scientists have swarmed over Ulster since 1968, the urban Protestant working class has been rather neglected as a subject of systematic study. This lacuna stems from often quite mutual suspicion, apprehension, and distaste between roving researchers and loyalist communities and organizations. In the absence of careful fieldwork inquiries, the image of Protestant workers in the literature has ranged from that of an undifferentiated mass of bestial bigots (the "authoritarian personality" with a Northern brogue) to the optimistic portrait, drawn by a few Marxists, of a befuddled class recuperating—albeit in violent fits and stumbling starts—from a sectarian epidemic of false consciousness. Dupes, psychopaths, and nascent class-conscious actors are not so difficult to find (or to exaggerate) in the six counties. What is lacking are comprehensive analyses, based on intensive grass-roots contact, which illuminate how loyalist groups come to perceive political events, define their stances, and deem their actions rational.

In *Ulster's Uncertain Defenders*, Sarah Nelson provides a welcome and well-researched examination of the complex tissue of interactions among political parties, paramilitary groups, and (mostly) new community organizations in Protestant neighborhoods of Belfast from 1969 to 1975. The authors lays special stress on the strains to which Orange ideology was subjected "because any general explanation which analyses economy and social structures must be able to fit the subjective reality into its framework, just because social consciousness is the active process in determination of political struggles" (p. 10). In this effort, Nelson produces a worthy companion piece to Frank Burton's earlier probe into the "politics of legitimacy" in republican neighborhoods of Belfast.

In Part 1, covering 1920 to 1969, Nelson appraises the key elements of social ideology and state structure that contributed both to hard-line attitudes toward Catholics and to deferential behavior with regard to the elite in charge of the reigning Unionist party. In Parts 2, 3, and 4, Nelson focuses on three events that tore the

Unionist bloc asunder: the arrival of British troops in August, 1969, the imposition of direct rule in March, 1972 (and its aftermath), and the mass loyalist strike (abetted by some bullying) in May, 1974 that brought down a promising experiment in power-sharing.

Beneath supremacist slogans and despite favorable discriminatory practices, internal dissension simmered throughout the heyday of Stormont rule, but any independent assertion of working-class interests was easily checked. The growth of even the mildest-mannered form of progressive political action was (and is still) precariously reliant, as Nelson notes, on "low cross-border tension" and a "belief that the cake could be made bigger all around, not redistributed in a way that left the majority with less" (p. 47). Conservative Unionist representatives proved unresponsive to the local grievances of loyalist communities on matters of amenities and services—and these areas, as any visitor can attest, are hardly havens of luxury (hence the concern regarding "redistribution" of already frugal living standards).

The Civil Rights movement galvanized a "dual discontent" felt by Protestant workers toward the regime; O'Neill's conciliatory policies of the 1960s seemed a betrayal, and, further, the existing structure appeared to have done little on their own behalf. Nelson's extensive interview material illustrates vividly the deep confusions, resentments, and lack of self-confidence that daunted the "tentative steps toward self-assertion" by breakaway loyalist workers. Paisley's Democratic Unionist party offered an *Escape from Freedom* refuge for many; the Ulster Defence Association (UDA) and Ulster Volunteer Force (UVF) became homes for sectarian assassins and some would-be social reformers whose struggles would send those organizations careening in one direction and then the other (several times); and community action groups absorbed the energies of others. Although Nelson clearly sees the community action groups as a hopeful development—a benign crucible in which to develop political skills and to define progressive goals—the optimism is guarded. All groups are "vulnerable to traditional pressures," and Nelson also acknowledges the cessation of cross-community ties (i.e., with Catholic groups) since the late 1960s. The book provides fair, concise, and sober judgments regarding the range of people attracted to various groups, the constraints under which they operate, and the often subtle relationships among them—particularly between local communities and the paramilitary organizations. One wishes, however, that Nelson would give at least "ballpark figures" to account for the number of breakaway loyalists who joined each group.

Ulster's Uncertain Defenders aids understanding of the ways loyalists maintained the "integrity of their self-images" in the face of contradictions between sub-state violence and a professed devotion to law and order (*whose* law and *whose* order are, of course, the stakes of the conflict), of the motives of the actors, and of their attachment to rather tenuous identities. (One group argues in an interview [p. 114] that they are "even more loyal than most Britons, because they accepted far lower wages than mainlanders!") Overall, Nelson has written an extremely useful book, and one hopes that a second volume dealing with the period from 1975 to 1984 is in the works.

JOHN KURT JACOBSEN

University of Chicago

Interest Groups and the Shaping of Foreign Policy: Four Case Studies of United States African Policy. By F. Chidozie Ogene. (New York: St. Martin's Press, 1983. Pp. 224. \$27.50.)

The visit of Bishop Desmond Tutu, the second South African to become a Nobel laureate, to the White House and American concern with famine in Africa suggest that African issues do obtrude upon the American foreign policy process and evoke official and unofficial responses from elites and attentive publics alike. Just how such issues are placed upon the public agenda and the roles various American interest groups play in shaping and resolving such issues constitute the central focus of this book. Ogene, who is a Senior Research Fellow of the Nigerian Institute of International Affairs and a former member of the Nigerian foreign service, was educated in the United Kingdom, the Netherlands, and the United States, where he taught political science at Central State University in Wilberforce, Ohio.

His book, which is a revised version of his 1974 doctoral dissertation at Case Western University, is arranged in a crisp, logical fashion and is intended for those political scientists interested in comparative and international politics as well as for Africanists. The research is centered upon four particular case studies, two of which are devoted to Southern African concerns (economic sanctions against Rhodesian chrome and import quotas for South African sugar), the third concerns the Nigerian Civil War, and the fourth involves the near-disintegration of Zaire in the period immediately following its independence from Belgium. Ogene introduces his study with three hypotheses, only one of which focuses upon the specific region of Africa. The other two refer to the expected utility of tangible and nontangible

(symbolic) concerns of those interest groups involved in the foreign policy formation process and the relative weight to be assigned to external (situational) foreign policy concerns and internal interest group assets and liabilities.

Ogene then explores briefly the English-language literature on the subject of interest groups and foreign policy before moving on to the task of providing operational definitions for the concepts used in his inquiry. Both in the introductory and concluding chapter he comes to grips with the methodological problems concerning the study of influence and the utilization of case studies. Although he provides no arrestingly innovative solutions to these nagging methodological concerns, Ogene is well aware of the limitations of his study and recognizes that his is not the definitive work on Africa and the American foreign policy process.

Even though he confirms his first hypotheses "that interest groups exert significant influence on U.S. foreign policy on African issues" (p. 191), Ogene does not enable the reader to determine just what "significant" means either in nominal or ordinal terms. He does, however, provide a thoughtful taxonomy of influence derived from his four case studies. His second principal finding is that tangible and symbolic interests are not mutually exclusive categories in terms of group activity. He indicates that "groups with interest in symbols were influential where there were no groups with material interests opposing them or where they could supplement their case by arguments showing that tangible resources could be secured or fostered by their policy preferences" (p. 195).

Ogene's third conclusion is that group-related and situational factors must be considered together, and that the former does not weigh more heavily than the latter. He provides a useful manner of looking at the different types of interest groups operating in the foreign policy setting and suggests how they are linked to the mass public, to external actors and the foreign environment, and to the foreign policy decision makers and their decisions.

This book is a competent treatment of a significant topic in foreign policymaking. However, the revision of Ogene's dissertation did not entail much updating of his sources; only four entries in the bibliography were published after 1974. Moreover, the total number of his interviews in Washington is not given, nor are the reasons for his selection of interviewees made clear, even though he makes explicit the methodological pitfalls involved in using interviews for his study. Ogene's selection of American newspapers as data sources is exceptionally limited and excludes those that cater to black American audiences. Finally,

the care with which the selected bibliography was assembled leaves much to be desired.

RICHARD DALE

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Births, Deaths and Taxes: The Demographic and Political Transitions. By A. F. K. Organski et al. (Chicago: University of Chicago Press, 1984. Pp. xii + 161. \$18.00.)

This book suggests that "the larger the scale of government, the lower the rate of childbearing and the lower the rate of mortality" (p. 2). One test of this hypothesis is a highly disaggregated study of *how*, in a particular spatial/temporal context, the many aspects of government influence the many aspects of demography. This single-case approach sacrifices breadth for depth. Organski et al choose to test the hypothesis with a highly aggregated multiple case approach: They regress, in a cross-national sample of nations, birth rates (Tables 3-6) and death rates (Tables 7-10) on a tax-based measure of political development. Their test supports their hypothesis. By boldly sacrificing depth for breadth, Organski et al have produced a most innovative and provocative work.

But how do the authors relate "taxes" to "political development"?

We establish a maximum possible level of taxation for developing and for developed countries. . . . We index the level of political capacity reached by taking the fraction of total taxes over GDP. After the maximum and taxes are adjusted on the side of expenditures for policy preferences and on the supply side for economic differences in the tax base, we subtract the adjusted taxes from the adjusted maximum. This is the estimate of political costs we shall use in our analysis as the measure of the independent variable, political development. (p. 79)

Organski et al are to be commended for raising a set of theoretical linkages that are at the heart of comparative analysis: These linkages run from the state's power in the international arena, to the political development or the expansion of the state, to the political performance or the political effectiveness of the state, to the state's capacity to extract resources, and finally to the (marginal) political costs the state faces in containing internal resistance to the whole development process.

The authors' approach to the fiscal politics of tax revenues is not, however, completely satisfying because they provide only vague labels rather than conceptual definitions. It is the *conceptual* as much as the *operational* significance of the key

terms, however, that are in dispute. Thus, as one reads Organski et al's careful reasoning about how "political development" is assessed via "taxes," one cannot help but recall the equally well-reasoned counterarguments to the whole political development tradition: capabilities are not activities; more activities are not necessarily better activities; and better is inherently a normative and not a positive issue. Thus the nagging feeling remains that the political development concepts themselves are too illusive, intangible, and imprecise to guide theoretical and empirical thinking.

And how do the authors relate "taxes" to "births and deaths"? The authors' approach to the demography of taxes is, unfortunately, not completely satisfying either. The theory of the state offered to account for the impact of politics on demography is unspecified. The effects of the expansion of central government on fertility and mortality are purported to be mostly "indirect" (p. 4), "unintended" (p. 118), "unmanipulable" (p. 15), "unconscious" (p. 13), and manifested in so many different ways (pp. 27-34) that "the list of such connections can be made long indeed" (p. 4). The theory of the state offered is also eclectic. It is partly functionalist: "It is generally agreed that any state must perform three principal functions . . ." (p. 45). The theory is also partly crisis-oriented: The performance of a government is best evaluated during the crisis of total war (pp. 67-70). And the theory is partly rational choice/predatory: "All governments need or at least want all the resources they can get" (p. 110).

Hence, the key problem with this work: Although the authors' evidence establishes the breadth of their central proposition, the theoretical linkages between births, deaths, and taxes are so amorphous and complex that the finding itself needs an in-depth explanation.

What went wrong with this study of how taxes stand between political development, on the one hand, and demographic change on the other? In sacrificing depth for breadth, the authors have formulated but not dissected the problem. Consider their index. The procedure used to measure political development assumes that (1) each nation's political performance can be reduced to a single number because (2) a single unidimensional index of political performance, which is constructed out of the "indicators" (p. 63) of the key concepts, can represent the concepts themselves. But the really interesting issues turn out to be *how* the index was constructed from its components. And consider their equations. Two (essentially) bivariate regressions, linking taxes to births and deaths while controlling for region and GDP, are estimated. But the really interesting issues turn out to be *how* politics affects

demography. Rather than proposing complicated and ad hoc justifications for estimating a very small number of key parameters, what is needed is an overall model, consisting of a series of inter-related equations, to get at the political and demographic development process.

In summary, Organski et al have raised the issue of a linkage between political and demographic change, but they have not really opened the black box and offered us a convincing and precisely defined causal mechanism of how the development process operates. Their work takes the first crucial step by examining the hypothesis in all its breadth. Organski et al do raise all the right questions. However, as analysts become concerned with the hypothesis in all its depth, new answers that build on Organski et al's approach will have to be offered.

MARK LICHBACH

University of Illinois at Chicago

Politics and Change in East Germany: An Evaluation of Socialist Democracy. By C. Bradley Scharf. (Boulder, Colo.: Westview Press, 1984. Pp. xii + 220. \$27.00, cloth; \$11.95, paper.)

While the German Democratic Republic (GDR) has been the object of growing attention from scholars in recent years, locating it in an appropriate framework for analysis continues to be problematic. Is the GDR to be viewed as little more than an obedient instrument of Soviet foreign policy, as one of several East European nations wrestling simultaneously with the problems of economic development and national legitimacy, or as a poorer and smaller communist brother of the German Federal Republic? Or is it, in spite of its communist political system, more usefully regarded as an advanced industrial society comparable in many ways to the capitalist welfare states of Northern Europe?

In this survey and interpretation of politics and society in the GDR, C. Bradley Scharf tries to view it especially in terms of the last perspective. There is much to be said for such an approach. The GDR is the most prosperous and technologically the most advanced of the communist states. It now has the largest relative volume of trade with the West of any of the Soviet Union's East European allies, which helps to make it in turn the leading supplier of advanced technology to the USSR and the rest of the bloc. Its program of social benefits, given special emphasis under Erich Honecker, offers a range of economic protections to its citizens that in some ways invites comparison to those provided by the Scandinavian

states. As Scharf notes, many of the GDR's problems—environmental deterioration, youth unrest, the pressures on traditional attachments reflected in high divorce and suicide rates—are also those of advanced capitalist societies.

The heart of Scharf's book is to be found in the chapters dealing with the efforts of the regime to promote social integration and with the continuing reality of social differentiation and discontent. The Marxist-Leninist model set forth by the GDR is one of a solidary society in which class antagonisms have given way to mutual cooperation, widespread citizen participation functions not to further particularistic interests but to advance the collective good, and pervasive childhood and adult socialization helps unite people and the ruling party in the building of a new and superior socialist way of life.

In fact, GDR citizens find themselves tempted by the blandishments of consumerism and bourgeois democracy transmitted nightly by West German television, while also facing an internal tension between the regime's ideals and its achievement-oriented practice. The GDR's claims to be creating a more humane and cooperative society do not fully comport with the heavy emphasis on performance—on the job, in sports, in social activism—in its reward system. Visible differences in access to privilege, inconsistent policies of repression and relative permissiveness in matters of personal and literary expression, and the search of many citizens for private "niches" (Günter Gaus) that offer an escape from the demands of the state all tend to subvert the integrative ideal.

Some parts of this book—the chapters on economic and social policy, for example—are admirably researched and executed. The pivotal chapters on social integration and differentiation, however, occasionally drift into excessive speculation and neglect important East and West German studies that might better illuminate the subject. Matters that would seem particularly important for understanding the GDR's attempts to implement its integration model—the actual role played by ideology in social management and the importance of militarization in the socialization process—receive insufficient treatment.

Overall, however, Scharf has given us a thoughtful and frequently novel analysis of a society and political system which is likely, as a model of communism in an advanced industrial setting, as an actor in international politics, or both, to have significant influence on the future of both Eastern and Western Europe.

THOMAS A. BAYLIS

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Minority Rights: A Comparative Analysis. By Jay A. Sigler. (Westport, Conn.: Greenwood Press, 1983. Pp. x + 245. \$29.95.)

Sigler has written a creditable survey of the status of minority rights throughout much of the world. Some comparativists will lament that the work is not really a comparative analysis: The data presented are not compared either quantitatively or in terms of a verbal typology. However, it is refreshingly free of jargon and thus would be a useful textbook for an undergraduate course in comparative rights. (A cheaper, paper edition would be appreciated.)

Sigler begins by defining *minority* and rightly insisting that there is a difference between individual and minority rights, and that liberal theory has emphasized the former to the neglect of the latter. (For example, to Sigler, not only freedom of speech, but also the right not to be discriminated against on account of, say, color, is an "individual right," whereas the right of a minority group to use its own language is an example of a "minority right.") Chapter 3, by James Muldoon, is the most original feature of the volume, providing a history of the development of group rights. Its basic conclusion, that freedom of religion ultimately appeared in Europe because the contending faiths could not destroy one another, is strikingly close to the thesis of writers such as Barrington Moore that democracy developed in certain countries because clashing socioeconomic groups found themselves bogged down in stalemate.

Chapter 4 discusses the protection of minority rights by international agreements, which Sigler concludes is very limited. Chapter 5 mentions instances where the minority has dominated or does dominate the majority, with most time being spent on European colonialism and the Republic of South Africa. Chapter 6 deals with what Sigler terms *minority rights arrangements* in Belgium, India, Switzerland, and the USSR. Subsequent chapters treat affirmative action programs in the United States and the United Kingdom. A brief juxtaposition of the U.S. and UK experiences is the only real "comparison" in the work. The final chapter sums up earlier themes, sensibly points out that demands for independence by minority groups are probably just going to strengthen the position of those who oppose minority rights claims, and provides (under the rubric of "a provisional theory of minority rights") a list of certain rights due minority groups.

As will any global survey of political phenomena or problems, the book presents case studies that merely skim the surface in providing information and analysis. As an information source, it is inferior to Kurt Glaser and Stefan T.

Possony's *Victims of Politics* (Columbia University Press, 1979), which Sigler apparently has not read. Sigler has worked hard, nonetheless, some of his sources need to be updated. For example, in his affirmative action chapter he declares (p. 136) that the "Supreme Court must ultimately confront the group rights issue"; no mention is made of the 1980 case of *Fullilove v. Klutznick*.

The manuscript also contains what will strike the average reader as contradictions. For example, on page 127 Sigler says that "of all the unresolved internal problems of the Soviet Union, the nationalities problem appears most serious." On page 129 we read that there "is no apparent sign of a severe nationality crisis in the Soviet Union . . . defiance [by minority groups] is rare and violence even rarer." On page 145 Sigler claims that "affirmative action in India is a controversial policy still, in spite of decades of experience in developing it. . . . In 1981 the reservation of 25 percent of the places in medical schools for untouchables set off riots in Gujarat State that lasted intermittently from January to April." Yet at the bottom of the same page he optimistically contends that "it is not impossible to blend the nondiscrimination ideal with an affirmative action policy. A few nations, most notably India, have managed to do so." On page 7 he indicates that "religion may not be enough to bind a group into a coherent minority, although small sects . . . frequently are perceived as minority groups." Despite this implication that there is something dubious about considering religious groups as "minorities," quite a few of Sigler's pages are devoted to the rights of religious groups.

Some readers will be annoyed by other facets of this essentially solid book. For example, in his discussion of church-state separation in England, Sigler omits the significant fact that denominational schools there get considerable public financial assistance. On page 194 he maintains that "there is a difference between joining the AFL-CIO and being a Jehovah's Witness, and this difference accords more rights to the religious group member than to the unionist." It is unclear whether this is a statement of fact or value, and no justification is provided for it.

DANIEL C. KRAMER

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The State as Terrorist: The Dynamics of Governmental Violence and Repression. Edited by Michael Stohl and George A. Lopez. (Westport, Conn.: Greenwood Press, 1984. Pp. viii + 202. \$29.95.)

This volume represents a first step toward filling a long-standing need in the terrorism literature for conceptual analyses to deal with the use of violence by governments, and it offers some promise of moving the analysis of terrorist violence beyond the bias in favor of incumbent authorities that characterizes present conceptual frameworks. Students of terrorist violence learn very quickly that the adage "one man's terrorist is another man's freedom fighter" is as true in the literature as it is in the political arenas. Ideological diatribes and narrowly focused analytical perspectives on terrorism fill library shelves, as do analyses that lack any real conceptual or theoretical underpinning.

In the Introduction, Stohl and Lopez note the fact that the systematic study of terrorism is a relatively recent development and that official or state terrorism, including vigilante terrorism, has been an overlooked aspect of the literature. They go on to note the failure to arrive at a workable legal definition that would permit international action to outlaw terrorism and conclude that the problem lies in focusing on the identities of the terrorists and their causes rather than on the nature of the acts themselves.

In terms of the chapters that follow, John McCamant offers an analysis of the iconic models that have characterized social science research in recent decades and concludes that the interest group, systems, Marxist, dependency, elite, and geopolitical models and theories have been ill-equipped to deal with the phenomenon of political repression. The remaining chapters deal with terrorism from a variety of orientations. Michael Stohl discusses the international aspects of state terrorism from the applications of terror in diplomacy and nuclear strategy to the uses of terror by government agencies in clandestine attacks on foreign governments or persons and the subsidizing of surrogate terrorism.

George Lopez then offers a "scheme for the analysis of governments as terrorist," including the precipitants, the institutionalization, the ideological bases, and the character of state terrorism, to include the types of violence, the targets/message complex, and the technologies available. The beginning point for much of his analysis is the state's inherent capacity for coercion and the relative ease with which it can use that power to repress opposition and dissent.

John W. Sloan examines state terrorism in Latin America, providing an overview of the

problem, the precipitants, particularly elite dominance, and the influence of political culture on the choice of repressive policies. David Pion-Berlin examines state repression in Argentina in terms of the impact of economic factors, including land reform and labor unions, on the repressive activities of the state. Jim Zwick looks at the links between increased militarism in the Philippines and the propensity of the Marcos regime to use repressive violence. Using theories and models of revolutionary violence, Robert A. Denemark and Howard P. Lehman analyze the costs that may be exacted because of continued repression in South Africa. Frederick D. Homer describes and explains government terror in the United States in terms of "containment policy" or the tendency to reduce real or imagined challenges to government authority, public safety, or civic order, through persuasion, intimidation, or violence. Lopez and Stohl conclude with a proposed research agenda and a bibliographic essay.

The volume is uneven, ranging from essays to empirical analyses, from country studies to epistemological theories. Nonetheless it attempts an important conceptualization. The terrorism literature can be frustrating for one looking for conceptual clarity and objective analysis. The serious student will find this volume useful and stimulating.

WILLIAM L. WAUGH, JR.

Georgia State University

The Immortal Atatürk: A Psychobiography. By Vamik D. Volkan and Norman Itzkowitz. (Chicago: University of Chicago Press, 1984. Pp. xxv + 374. \$37.50.)

The new genre of psychologically sophisticated biography, of psychobiography, is developing as rapidly as the Third World. What is so special about this new book is that, like Edgar Snow's *Red Star over China* (1938), it relates a remarkable people to a remarkable leader in the Third World. Like China, Turkey has had a long and proud past. The Ottoman Empire began in the fourteenth century, peaked when Turks almost took Vienna in the sixteenth century, declined, and at last expired when the new republic was founded in 1923. Often rightly renowned for their ferocity, Turks as late as 1915 began to relocate about 1.75 million Armenians, hundreds of miles from their homeland on the Russian frontier. Close to half starved or were slaughtered en route.

Such aggressive efforts stopped after the war. Mustafa Kemal Atatürk is the man who singly was most responsible for the Turkish victory in 1915

over the British at Gallipoli and for the defeat in 1922 of the Greek invasion that penetrated nearly to Ankara in the heart of the once overweening Ottoman Empire. But he then turned his people's attention to internal reforms. By Romanizing the alphabet he facilitated literacy; by denationalizing the Islamic priesthood he diminished the political hold of that religion. He helped Turks manumit themselves from their past while he remained a pragmatist and a teacher.

The most remarkable element in his domination of the country was the degree of restraint in the use of force; the scale of repression there was perhaps much less than in Germany or the Soviet Union in the 1930s.

This book is a major effort at explaining how modern Turkey came about and how Atatürk ("father of Turks") became its founder. Modernization had already commenced in the last century of the Ottomans. In the latter half of the nineteenth century, there was a rapid rise in population. But the economic and political elite—seemingly like ancient regimes in all developing nations—regarded the public as ignorant instruments of their efforts to aggrandize themselves.

Kemal was the son of a literate, entrepreneurial, and free-thinking lumber merchant and of an illiterate, strong-minded, devout young Muslim. He was overprotected and adored by his mother, even more after his father died when Kemal was seven and his mother only 27. He became prodigiously active sexually (and in every other way) but repeatedly in his public career said "I am in love with my mother."

The authors reiterate themes of mother-son ties, in Atatürk's passionate devotion to moving his backward country forward. At the same time, they report his enormous energy and his very high opinion of himself—when he was not in one of his occasional periods of profound depression. It is hard to overestimate his passionate fidelity to his people.

He may best be characterized by an entry he made in his diary while recuperating at a spa in Germany during the war:

Why, after my years of education, after studying civilization and the socialization process, after spending my life and my time to gain pleasure from freedom, should I descend to the level of common people? I will make them rise to my level. Let me not resemble them: they should resemble me. [Quoted on p. 104.]

To understate the relationship, he never tried to distance himself from people, whether in personal or political affairs.

This is an altogether absorbing book: It gets into the psychodynamics of a developing nation

and leader that have in many ways been at once unique and universal. Indeed, both the strong and the weak points of the book relate to Turkey's uniqueness. The strength lies in the effort to relate modern Turkey to its past, and to relate Mustafa Kemal Ataturk to both his own past and his vast public following. The weakness derives from the impression the authors give that Turkey and he are indeed *only* unique and not comparable to other nations and leaders.

There is not enough consideration of the social history of these hard-formed and conservative people, whose numbers have tripled in the two generations since the Republic was founded. Nor is there adequate consideration of the new demands they were making for at least two things: recognition by their own leaders and by other nations of their dignity as human beings and the right to participate in significant political action. The authors tend to use straight historical analysis of gross national events that are more complex than battles and to use conventional Freudian concepts to explain a personality that is more than just Oedipally complex. There is not much discussion of the modernization process as Durkheim, Marx, and Daniel Lerner examined it.

Napoleon was one of the earliest modern rulers to understand and exploit ordinary people's wish to get respect and get in on the action. In a strange way, so did Hitler. The difference between these two rulers and Ataturk is that he helped his countrymen to get politically involved and to gain respect without turning to genocide, slave-labor camps, and war or subversion abroad. Other nations, both developing and developed, should be so lucky.

JAMES CHOWNING DAVIES

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Political Violence, Crises, and Revolutions: Theories and Research. By Ekkart Zimmermann. (Boston, Mass.: G. K. Hall, 1983. Pp. xvi + 792. \$45.00.)

Ekkart Zimmermann's encyclopedic assessment of contending theoretical approaches and cross-national empirical studies of political violence represents an ambitious attempt at analyzing three major areas of concern: systemic crises, military coups d'états, and revolutions. The mammoth character of this work, its demonstration of both depth and breadth with regard to the literature on political violence, clearly establishes it as the most comprehensive source of knowledge in this field of study. Yet the content of the book is somewhat deceptive. The actual text is only 434 pages, followed by 354 pages

of notes, bibliography, and index. Nonetheless, Zimmermann makes a formidable contribution at the levels of theory, methodology, and empirical research.

This study begins with a preliminary examination of conceptual problems, experimental studies of aggressive behavior, and theories of social comparison processes (group theory). Zimmermann adopts a working definition of political violence based upon H. L. Nieburg's behaviorally oriented "bargaining model" while rejecting social-psychological explanations of aggression (both frustration-aggression and social learning theories). Following this, he launches into a detailed analysis of the cross-national quantitative approach to the study of political violence. Here the work of Ted Robert Gurr and Douglas Hibbs is paradigmatic for Zimmermann. Gurr's studies of civil strife and Hibbs's research on mass political violence are rigorously dissected, analyzed, critiqued, and praised, with primary attention paid to methodological issues of research design, formal modeling, operationalization, measurement techniques, coding procedures, sample selection, and multivariate analysis. The research of Ivo and Rosalind Feierabend is also reviewed, but their "frustration-aggression nexus" as it relates to systemic frustration is found to be of questionable explanatory worth. Finally, a section on the riots of American blacks during the 1960s is included not only due to the wealth of empirical data on this phenomena, but also to illustrate the many methodological problems encountered in doing political violence research. Throughout this exhaustive account of varying approaches and quantitative studies, Zimmermann impressively combines thoroughness with precision and a keen eye for details.

Systemic crises and their relationship to political violence are approached by way of an evaluation of structural-functionalism and systems theory as well as a critical commentary on most of the mainstream literature on political development and modernization. Leonard Binder's "crises and sequences of political development," the work of Raymond Grew and associates, Seymour Martin Lipset and Stein Rokkan, Gabriel Almond and Sidney Verba, and Samuel Huntington are all scrutinized. Zimmermann's general critique of "the underdevelopment of crises research" (p. 189) is unfair however, because it masks a deeper frustration on his part over the fact that the often-used historical case study approach does not neatly fit into his particular quantitative-behavioral orientation. In the end, Harry Eckstein's political performance decision-making approach and Gurr's causal model explaining "persistence and change in political systems" are chosen as the most fruitful

frameworks with which to understand political crises. Huntington's theory of praetorianism informs the chapter on military coups d'états. Here post-World War II empirical findings are examined and summarized in the form of an elaborate causal model that seeks to represent the synthesis of numerous crucial variables. The work of Huntington, Barrington Moore, and Theda Skocpol provides a broad theoretical background for the cross-national study of revolutions. This examination culminates in a careful look at the empirical work of Charles Tilly on collective violence. Zimmermann is especially critical of Moore, arguing that in the final analysis his comparative-historical analysis of modernizing revolutions proves nothing. The fault here, as with most of Zimmermann's criticisms, is that everything is reduced to a problem of methodological precision. Grand theorizing and statistical manipulations mix very poorly. Continuing with an inquiry into the work of Eric Wolf on peasant revolutions, a brief overview of guerrilla warfare and political terrorism, and a review of George Rude's studies on revolutionary crowds, a "raw causal model of revolutions" based upon a revised version of Tilly's "power-contending model" is proposed in summation.

Zimmermann is correct in concluding that the

subheading of his book should have read "Trends Report," for that is precisely what it is. Although his exposition and critique of a vast array of theories and research is unquestionably a valuable contribution, no new major theory or synthesis is presented. Rather, a strategy of "incremental model specification" is employed; a tinkering with existing approaches, models, and findings culminating in modest reconstructions and a call for more research. Yet his own quantitative approach systematically relegates to the periphery of political violence research the importance of ideological factors as well as the significance of neo-Marxist analyses of systemic crises, revolutions, and counterrevolutions. Also, the phenomenon of contemporary terrorism, worthy of an entire chapter of its own, is dealt with only in a cursory fashion. Despite these shortcomings, Zimmermann's work is a solid effort and should stand for some time as essential reading for the serious student of political violence. With this book as a foundation, Ekkart Zimmermann should continue to make significant inroads in the comparative analysis of social conflict and political violence.

WAYNE GABARDI

University of California, Santa Barbara

International Relations

Conflict in World Society: A New Perspective on International Relations. Edited by Michael Banks. (New York: St. Martin's Press, 1984. Pp. xxi + 234. \$27.50.)

This book contains a collection of essays in honor of John W. Burton and his World Society approach to international relations. The book is divided into three sections. The first explores the rise of the World Society perspective, discussing the evolution of international theory, the World Society approach as a new paradigm for the study of international relations, and the manner in which theory is molded by the perceptions of researchers. The second examines the structure of World Society, addressing such things as the World Society view of the relationships among international actors, the role of decision makers, conflict in World Society, and psychological processes and crisis prevention and resolution. The final section seeks to apply the World Society approach, investigating subnational community participation in World Society, problem-solving workshops, terrorism and its resolution, national

foreign policy behavior, and the interaction between academics and practitioners.

Although the praise for Burton is well deserved, and the essays generally are well written, there are several difficulties associated with this book. The first has to do with its organization and the integration of the essays. Beyond the vague references to World Society that are found in most of the essays, there is little effort to tie the chapters together. There is no introductory chapter summarizing the World Society approach, nor is there a concluding essay that discusses the basic points made in the book and relates them to the World Society perspective. Moreover, only Chadurch Alger, Christopher Hill, and Richard Little are explicit about how the points they discuss relate to World Society. Consequently, the reader is left with a collection of essays that often appear unrelated, either to one another or to the World Society perspective.

A second problem relates to the nature of the essays. In general, each chapter consists of a summary of a selected aspect of international relations; however, the essays report no new research,

nor do they either attempt very extensive reviews of existing literature or aim at further theoretical development or refinement of the World Society perspective. Exceptions are found in Alger's clear and concise discussion of his Columbus study; in Little's useful comparison of what he calls the radical, pluralist, and World Society views of international affairs; and in Hill's critique of the World Society approach. Clearly, considerations of space preclude the reporting of extensive research or the attempting of thorough literature reviews, but the failure at least to attempt some theoretical refinement is disappointing, especially in light of the discussion in the first section of the book of the World Society approach as a possible new paradigm for the study of international relations. If the World Society conception is to be considered a useful new paradigm, then surely its advocates must convincingly demonstrate its theoretical and practical superiority to other potential approaches. The discussion of conflict prevention and resolution is a case in point. There is a general consensus among the contributors that a World Society approach is superior to others, but there is no clear-cut indication as to precisely what such an approach entails beyond the vague notions that incorrect perceptions exacerbate conflicts, that increased interactions (of the proper type) among antagonists may be useful, that mediation efforts are valuable, and that a variable-sum view of conflict situations is preferable to a zero-sum view. Such ideas are hardly new and are not unique to the World Society approach. Moreover, Frank Zagare (*Game Theory: Concepts and Applications*, Sage Publications, 1984) has demonstrated that it is incorrect to view variable-sum conflicts as inherently less intense and more amenable to solutions than zero-sum situations.

As it is, this book points to the need to consider alternative approaches to the study of international relations. Future efforts should focus on being more precise as to the unique contributions that this particular approach offers.

JOHN M. ROTHGEB, JR.

Miami University

Economics and World Power: An Assessment of American Diplomacy since 1789. Edited by William H. Becker and Samuel F. Wells. (New York: Columbia University Press, 1984. Pp. xvi + 474. \$35.00, cloth; \$10.00, paper.)

It is timely, as we debate the nature of recent American leadership in world politics, to review our past growth, expansion, rise to world prominence, and decline from hegemony. We must

understand not only the domestic and foreign economic sources of U.S. power and prestige, but also the way in which economic interests directly affect the nature of U.S. world leadership. Is it true, for example, that short term and domestic economic interests are more likely to prevail in U.S. foreign policy when domestic or foreign economic position is relatively weak?

Economics and World Power (which should be titled *Economics and American Diplomacy*) may also indirectly help advance thinking about other important themes in politics. U.S. leaders and coalitions have often used economic interests and objectives to justify noneconomic purposes. This strongly demonstrates the necessity of carefully weighing official statements to sort out economic arguments from real economic motivations, for example in questions of economics and national security. Closely related is the history of how the United States applies economic means to achieve political and strategic ends, including the pervasiveness of "reciprocity" as a guiding principle and diplomatic instrument of U.S. foreign policy. Finally, analysts of international politics, U.S. foreign policy, and comparative foreign policy will also find documentary material of importance for weighing the balances between market forces and government planning on the one hand, and between domestic and international sources on the other.

This is an edited book in the finer sense. It draws on the individual expertise of nine experts, allowing each to focus on his (with an unfortunate neglect of women historians) special historical period in U.S. diplomatic history within the framework provided by the editors through a set of overarching questions. Although clearly focusing on economic influences, the authors successfully carve out a more precisely defined middle ground between the over emphasis of economic factors by revisionists and the under emphasis by realists. It is most valuable not for new findings but for the consistent, careful, and comprehensive treatment, under one cover, of economic influences at all levels of analysis in American foreign policy over some two centuries. For a scholarly volume, it is surprisingly well written. The work does, however, merit a serious conclusion rather than a brief epilogue.

The book's principal flaw is imprecision and redundancy in the analytical framework. The editors should have demanded less emphasis in their focusing questions for the authors on disproving revisionist arguments (despite their promise in the Preface not to revive the debate between revisionists and their critics) and more precision in explaining where disaggregation of economic, diplomatic, and security factors is, and is not, possible. The editors only complicated the

authors' difficult task of assigning relative weights to economic, diplomatic, and security influences. The final chapter (1961-1980s) is the worst on this dimension (too heavy on economic analysis and factors; too light on domestic and international politics), but several other chapters also leave the reader with little overall sense of what was most, less, and least important, except for general statements about "economic" and "noneconomic" factors underlying a particular event or period.

DAVID A. DEESE

Boston College

Reason and Realpolitik: U.S. Foreign Policy and World Order. By Louis Rene Beres. (Lexington, Mass.: D.C. Heath, 1984. Pp. 143. \$20.00, cloth; \$9.95, paper.)

If the United States as the major actor on the world stage wants peace, then it must prepare for peace.

In *Reason and Realpolitik*, Louis Rene Beres presents a passionately argued critique of American foreign policy, written with the sound and committed scholarship that characterized his earlier works on international politics and world order.

He warns that the world is walking on the edge of a precipice and that only a dramatic turnabout in our understanding of the current crisis and the implementation of appropriate policy changes can avert imminent disaster.

Beres begins by tracing the origins of realpolitik back to the 1648 Peace of Westphalia, which led to the formation and consolidation of centralized nation states in Western Europe. He decries the competitive power politics and balance of power strategies that failed historically to create international peace and that in today's world lead to self-destructive behavior. He maintains that outmoded ways of thinking and acting both underlie much of America's current search for a just and durable peace and dominate U.S. policy toward the USSR and the Third World. America therefore must begin to fashion its foreign policy on a new set of premises based on an "understanding that this nation coexists with others on this endangered planet in a perilously fragile network of relationships. Racked by insecurities, poverty, and inequality, this network can no longer abide the conflictual dynamics that have shaped international relations" for over three centuries (p. 2).

The central chapters of this disquieting book deal with the issues of nuclear weaponry and human rights. Beres holds that U.S. policies have led to the proliferation of the nuclear arms race and that the prevailing strategies could lead to omnicide. He joins the growing number of critics calling for a drastic rethinking of nuclear policies and is a strong advocate of nuclear arms control with the USSR.

The author contends that current administration policies on nuclear strategy and human rights are interrelated, and he decries the present U.S. tendency to overlook human rights violations in certain anti-Soviet states.

Although holding no brief for the Soviet Union, Beres is deeply disturbed by President Reagan's "evil empire" imagery. "Although Soviet behavior in world affairs hardly meets the test of 'goodness,' the alleged contrasts between the forces of evil and the forces of good represents little more than a childlike caricature" (p. 127).

Beres calls for a new definition of national interest, one that moves away from the long-standing adherence to Social Darwinism in world affairs. He calls upon the United States to help develop a more harmonious system of planetary political life—to seize the initiative for our common survival while there is still time. If it did so, "the U.S. could confront the dying world of apocalyptic militarism with wisdom and hope."

In his concluding chapter, "From Paradigm to Planetization: The Journey Begun," Beres agrees with Thomas Kuhn regarding the need for transformation of U.S. foreign policy behavior from old forms of power politics to a world order orientation. Before the behavior can change, there must be a widespread "revolution of consciousness." An end to realpolitik requires an expanded awareness of global interdependence. Beres cites teachers over the centuries, from Marcus Aurelius to Teilhard de Chardin, who are in the tradition of this wholistic mode of thought.

Although Beres is wise enough to refrain from trying to lay out a detailed blueprint for change, he makes a number of pertinent general suggestions. Calling upon Americans to return to the Enlightenment principles on which the nation was founded, he urges that militaristic nationalism be replaced by a recognition of the right of self-determination of all peoples, as codified in the U.N. Charter.

Although occasionally a bit turgid in its prose, *Reason and Realpolitik* is a most insightful and challenging book. It is both a manifesto for human dignity and a plea that the "business as usual" attitude with which we often study international relations be replaced by an incisive urgency. It can be recommended as "must read-

ing" for policymakers, teachers, and students of international politics.

ARTHUR STEIN

University of Rhode Island

Multidisciplinary Perspectives on Population and Conflict. Edited by Nazli Choucri. (Syracuse, N.Y.: Syracuse University Press, 1984. Pp. xi + 220. \$30.00, cloth; \$13.95, paper.)

This is a noble attempt to make explicit some of the most enduring, but rarely confronted, assumptions about the relationship between population variables and conflict dynamics. Nazli Choucri heads a list of senior academicians from a number of disciplines in the social sciences in an attempt to sensitize us to the "potential political effects of population factors and their possible impact on conflict behavior . . . [and] to place demographic variables in a broader socioeconomic and political context and show ways in which they interrelate" (p. 1).

In a promising introductory essay, Choucri relies heavily on her previously published work to establish, although never quite unequivocally, that although population size is not a critical factor itself, other intervening variables often make demographic variables salient. She then proceeds to argue that four attributes of population—size, change, distribution, and composition—play a particularly significant role in international disputes, especially when they operate in concert. However, instead of demonstrating how this relationship might work in reality—a task that Davis B. Bobrow accomplishes rather successfully in a subsequent chapter—Choucri relies on lists of "crisis factors," "linkage factors," and "conditions for international conflict."

Although the introductory chapter leaves one with the distinct impression that the book will be about the place of population variables in *international* conflict, the volume's next three chapters focus almost exclusively on internal societal conflict. In fact, this is one of two ambiguities that seem to permeate the entire volume and that I find confusing and frustrating; the other is the intended audience of the work. It is clearly not appropriate for class adoption; nor does it report on new research or advance new theories; nor is it likely to be of much use to specialists either in the area of population or conflict; nor, finally, is it directed to policymakers at either national or international level. In fact, the policy lessons made explicit in almost all chapters are at best elementary with two exceptions: Bobrow's lucid synopses and Robert C. North's learned overview.

The volume never quite manages to come to

terms with the complexity of the relationship among population growth, development, and internal and/or international conflict. In fact, I feel that, in terms of the explicitly stated central theme of the book, the individual essays do not take one much further than the 10 propositions summarizing theory and evidence on population and conflict that are succinctly stated in the Introduction (p. 15).

One is also left puzzled by the many issues that belong in a book on population and conflict but that were ignored. Surely, the questions of internal and international migration and forced population movements would be natural candidates for a book on population and conflict. Furthermore, the role of the state in population matters—both in its ability to influence demographic variables and in its capacity to influence internal and international conflict directly and indirectly—might have received considerably more attention than it does. Finally, why do the essays by Harold M. Proshansky, William R. Kelly and Omer R. Galle, and John R. Harris and Vijaya Samaraweera fail to come to terms with the reality that this was, or should have been, a volume about addressing population-induced conflict in types of societies where the issue is most acute: the less-developed countries.

In spite of a strong introductory chapter, the volume never delivers on its promise. An opportunity to address an important topic involving a putative relationship of enormous significance (see the World Bank's *World Development Report*, Oxford University Press, 1984, especially Part 3) has been lost.

DEMETRIOS G. PAPADEMETRIOU

Population Associates International

The Problems of Plenty: Energy Policy and International Politics. By Peter F. Cowhey. (Berkeley: University of California Press, 1984. Pp. xiii + 422. \$36.00.)

This study is an impressive piece of scholarship on a very difficult subject. It captures the complexity of world oil markets through a carefully considered set of concepts that enrich our understanding of energy as such and improve our general understanding of the way that politics and markets intersect. Students of international politics and political economy would profit from reading this work, because its approach is applicable to other substantive areas.

Cowhey casts his analysis at the level of strategic interactions of private firms, government agencies, and international organizations. These actors design self-interested strategies of competi-

tion and cooperation within a structured and politicized market that shapes their choice of actions.

The Problems of Policy criticizes regime analyses that try to explain major international outcomes in terms of the power of the world's leading country or hegemon. Cowhey nicely pinpoints the political reductionism of this perspective that places undue weight on the political and the structural to the detriment of the dynamic process of constant mutual adjustment that is the hallmark of markets.

Cowhey also takes on neoclassical economic assumptions about the primacy of efficiency and automatic adjustments of profit-maximizing firms in markets. Instead, he finds that actual oil market outcomes vary from what the economic model would predict. How should we account for the variance?

The author explains these outcomes, unanticipated by the pure economic or the pure political models, by showing us how politics shapes market structure (the number of firms and their market shares) and the way market structures mold the subsequent choices—or "International Management Strategies"—of commercial firms and states acting as individuals and within coalitions. The mental image here of constant mutual adjustment among public and private actors is quite different from the more purposeful, directed, and dominant actor perspective that we associate with international regimes.

The core of this argument of strategic calculation within a coalition hinges on Cowhey's identification of two key aspects of this process. One is the degree of intervention into markets; the second is the degree of flexibility in implementing interventions. The former can range from detailed accords that directly allocate resources (such as the Red Line Agreement), to less interventionist means that set operational standards, to merely setting targets. A coalition's coordinating style in turn may involve strict "rules," more flexible "plans," and still more flexible agreements of "mutual adjustment." Cowhey applies these categories to show how influential governments seek to use firms as their surrogate managers of world markets (p. 79), even as firms press governments to control market rules and outcomes to private advantage. This approach allows Cowhey to avoid the false dichotomy between pure cartel and pure competition. In reality, there are elements of each in different activities at different times in different places. Three examples are offered as a loose "test" of his theory.

But the richness and value of the book lie not only in its theoretical ambition and its elaboration of concepts of corporate and political strategy and the conditions under which different mixes of

strategy will be employed. The book also provides a useful description of key events and processes in the history of world energy markets—oil, nuclear, and coal. For example, Cowhey reminds us that the principal market management dilemmas have historically been managing energy surpluses, not shortages.

As with all works, its strengths are its weaknesses. A net cast wide enough to capture the many details and dimensions of oil market complexity runs the risk of overwhelming the reader with an escalating number of analytic categories. At several points Cowhey asks the reader to negotiate a seemingly endless string of strategies, goals, targets, rationales, or resources that together have the effect of diluting the argument's central thrust—the relationship between coalitional structure and choice. Cowhey recognizes this danger and provides charts and useful summaries along the way. These summaries are helpful and partly overcome the limitations of the category proliferation.

Despite these limitations, *The Problems of Plenty* is probably the most successful single work on the world oil market that tries to combine an unusually wide and largely realized theoretical reach with historical and empirical richness.

ERNEST J. WILSON, III

University of Michigan

International Law and the Superpowers: Normative Order in a Divided World. By Isaak I. Dore. (New Brunswick, N.J.: Rutgers University Press, 1984. Pp. xiv + 202. \$30.00.)

Despite its slim size, this study is an important and ambitious work. As Dore states, it aims "to catalog in detail, norm by norm, the substantive content of the autonomous regime operating at the 'bloc' or hemispheric level and to identify its unique characteristics" (p. xiii). In substantial measure, it achieves this end, and in the process, furnishes a better understanding of the operation of superpower bloc politics in general and of the evolution of relevant international normative considerations in particular.

Dore appropriately begins his study with a treatment of where and how international law fits into the scheme of Marxist-Leninist ideology, with a view to explaining the significance of peaceful coexistence as a guiding concept for socialist legal thinking in the international arena. The thrust of his analysis is directed at applying a situational, case study approach (or as he terms it, a "behavioral approach") to assess the superpowers' conduct in crisis management, particularly within their own respective regional blocs.

Somewhat unrelatedly, the latter part of this work treats arms control efforts made by the superpowers in demilitarizing the ocean floor and outer space.

The study's main contribution is to be found in the chapters dealing with the superpowers' intrabloc behavior. Specifically, here Dore seeks to investigate two fundamental questions: (1) Are the superpowers' extraterritorial interventions merely acts of lawlessness in a normatively anarchical international society earmarked by ideological rivalry? Or, (2) Do these occasional interventions reflect something more rational about the dynamics of superpower tensions and interbloc relations? To address these questions, Dore first evaluates the legal conceptualization of aggression within both contexts of socialist and Western perceptions of the just war doctrine. He then applies these perceptions of "aggression" to intrabloc "trouble-situations" experienced by each superpower since World War II (i.e., for the United States, the cases of Korea (1950), Guatemala (1954), Cuba (1962) and the Dominican Republic (1965); for the Soviet Union, Hungary (1956) and Czechoslovakia (1968). Importantly, Dore avoids the polemics of adjudging the precise legality or illegality of specific interventions. Instead, he concentrates on how either United States or Soviet representatives employed international law to bolster their respective legal defenses of action.

Based upon these limited case studies, Dore's findings and conclusions are significant indeed, as they indicate strong evidence that certain rules (or an "emerging law of interbloc reciprocity") have grown up affecting superpower conduct. Among the more notable of these "laws" are that: (1) mutual noninterference should govern superpower relations in bloc affairs; (2) a breach of bloc solidarity remains a matter solely for bloc determination; (3) regional bloc preservation takes precedence over U.N. Charter provisions; (4) bloc members have rights of individual and collective self-defense against "indirect" aggression (read to mean subversion) in order to preserve ideological purity in the region; and (5) members of a bloc cannot withdraw voluntarily from bloc jurisdiction. To be sure, the study can be faulted for drawing such broad-based and far-reaching conclusions from so small a number of case samples. Yet, the policy patterns and conduct of the superpowers strongly suggest that these rules have been operative in their relationship during the past three decades, hence aptly denoting the realization that mutual respect for bloc solidarity has become a norm of superpower existence. This observation prompts Dore to make a more provocative general conclusion, namely that ideological conflict, far from ob-

viating legal norms and lawful behavior, "is in fact the *reason for* the emergence of law" (p. 94). Certain norms have evolved, not from superpower fiat or dictate, but rather from the political realities of conflictive existence. In the sense that law is seen as a series of prohibitions that establish and reinforce expectations of what should or should not occur, much truth may lie in Dore's normative characterization of intrabloc superpower conduct. Even so, in extrahemispheric interventions and adventures (e.g., Angola, Zaire, and Afghanistan), these rules tend to break down under the weight of power politics and ideological rivalry. That realization should not be lost in the search for a law of interbloc reciprocity predicated on status quo regional arrangements.

There is much to be learned from a careful reading of this work. Dore's style is lucid, cogent, and well articulated, and his arguments are amply documented to substantiate his contentions. Further, Dore supplies a reasoned, sober, calculated analysis of superpower politics and their normative implications. In doing so, he sheds light on the role of law in the Marxist construct of international society, as well as how overriding concern for bloc preservation by both the United States and the Soviet Union has worked historically to circumvent law when bloc solidarity appeared threatened. Perhaps most important, the behavioral approach employed in this study underscores the nature and substance of "the living law of power politics." Yet, while force often may represent the antithesis of legitimacy, the use of force in international relations does not necessarily imply that there is no law. That is the chief lesson to be gleaned from this work, and in an age dominated by such seemingly volatile ideological rivalry, it remains a lesson well worth remembering.

CHRISTOPHER C. JOYNER

The George Washington University

Assumptions and Perceptions in Disarmament.

By Daniel Frei. (New York: United Nations Publications, 1984. Pp. xiii + 321. \$14.00.)

The premise of this study is that subjective factors dominate the prospects for serious arms control and disarmament. The Soviet Union and the United States are found, by way of an extensive qualitative content analysis, to be locked into a pattern of "conflictive cognition." Frei concludes: "The two sets of fundamental assumptions define the room for maneuver available, in principle, in disarmament negotiations. As they

are almost mutually exclusive, the potential left for a common understanding seems at first glance to be rather small" (p. 278).

There is much to be admired in this study. Frei examined a large volume of literature. He focused on recent writings. He did not let his hopes dictate his conclusions. Most importantly, he organized his research and presentation around a common checklist of cognitive dimensions, which added rigor to a method too easily and too often misused for polemic ends.

On the other hand, the study is not without problems. The description of the U.S. perspective distinctly exaggerates its hawkishness. Why did this happen? For one thing, the search for a viewpoint biases one to find one. In the context of the Reagan administration, executive pronouncements become the operational definition of U.S. views. Granting executive branch dominance of national security affairs, nevertheless, nonexecutive institutions, especially the Congress, are important parts of the policymaking system. And the views of many of those actors are decidedly less hard-line than the administration's. U.S. hawkishness also is exaggerated by the sampling of writings. Official documents are supplemented by articles and books by such past officials as Kissinger, Brzezinski, and Harold Brown, but not from Vance and Warnke. The review of the academic literature seems, too, to be less than systematic and hence overly hard-line in the composite picture portrayed.

The description of the USSR seems also to be overdrawn. Frei presumes a unity of views: "The specific origin of all officially published material in the Soviet Union also suggests that looking for discrepancies reflecting latent tensions between different 'wings' may probably be nothing but a futile task. Western observers sometimes try to identify different and conflicting perspectives. . . . However, there are compelling reasons to assume that interpretations of this kind are ultimately based on wishful thinking and may be simple projections of the pluralist Western approach on to Soviet reality" (p. 26). This presumption can be just as blinding as the myopia he criticizes.

The degree of pluralism or uniformity of Soviet views is, ultimately, an empirical issue; it may vary according to the generality of views. Fundamental axioms, which are Frei's main concern, may very well be adhered to with considerable homogeneity. Their application to specific problems may give rise to the considerable debate across Soviet schools of thought and interested organizations.

If this is correct, then Frei has clearly shown that a general halt to the military competition is virtually impossible. He has less clearly shown the

inevitable frustration of moderate arms control efforts.

These quibbles notwithstanding, Frei's analysis seems fundamentally sound. This sobering book is a useful, even important contribution to the arms control literature.

PATRICK CALLAHAN

DePaul University

The Soviet Union and Terrorism. By Roberta Goren. Edited by Jillian Becker. (Winchester, Mass.: Allen & Unwin, 1984. Pp. xii + 232. \$25.00, cloth; \$11.50, paper.)

At a time when senior policymakers have expressed their concern over the threats to U.S. interests overseas posed by "state-sponsored terrorism," Roberta Goren has written a work that places a major element of that threat into a rich historical perspective and current analysis.

In *The Soviet Union and Terrorism*, she has provided both policymakers and academics interested in the topic with a valuable book. It is regrettable that the book was written as an academic thesis that was completed only a few weeks before the author died, because based on her initial research, it is obvious that Goren had a great deal more to contribute to the field. However the work is a fine and enduring contribution to the literature on terrorism.

The book is essentially an excellently researched descriptive study of how the Soviet Union has systematically employed terrorism as a weapon of political warfare and armed conflict since the time of Lenin.

After presenting the two categories of terrorism to be studied—subrevolutionary and revolutionary—and effectively dealing with the problem of definition in the Introduction, Goren makes a trenchant case in chapter 2, "Marxist-Leninist Ideology on Terrorism," for her view that the ideology is far more than "mere rhetoric" used to justify the use of terrorism; that it provides the motivation, organization, and strategy that makes terrorism an offensive weapon in Moscow's conduct of its international affairs.

Chapter 3, "Legal Aspects," is particularly germane to an appreciation of how the USSR has developed the capacity to sponsor various terrorist groups. By defining and making operational the concept of "indirect aggression" to meet its own goals, the Soviet Union increasingly engages in "surrogate warfare [that] has . . . increasingly become an alternative mode of conflict" to conventional military confrontations (p. 40).

Chapter 4, "Historical Perspectives," provides a detailed analysis of how the Soviet Union has

pragmatically employed terrorism in conjunction with traditional diplomacy in achieving foreign policy objectives. She notes that even when an emphasis was placed on the use of diplomacy as a result of the United Front policy against facism in the 1930s, "this new emphasis . . . did not herald any reversal in the use of terrorism. International terrorism was in fact never abandoned; it simply receded into deeper secrecy" (p. 64). Furthermore, Goren correctly observes that despite an ideological "abhorrence" against the use of "individual terrorism," there was no reluctance to resort to employment of "organized and centrally controlled terror methods and groups" (p. 87).

How the Soviet Union developed the ability to utilize highly diverse terrorist groups in the conduct of its foreign affairs is the subject of chapter 5, "Contemporary International Terrorism." Goren's study of the complex development of the relationship between the PLO and Moscow is particularly penetrating because it provides a basis to appreciate how "the links of the PLO with all major terrorist groups operating in Western liberal democracies, and some Third World countries, have allowed the Soviet Union a means of having global impact" (p. 139). These linkages are discussed in a very readable account of major terrorist groups that is reminiscent of Claire Sterling's fine treatment of the topic in *The Terror Network* (Holt, Rinehart & Winston, 1981).

In the final analysis, this book has substantiated and illuminated the first sentence in the Introduction. "This book is about war: the war which the Soviet Union is waging on the West—indirectly. In practical terms, international terrorism is power projection. It is the physical manifestation of a bid for power" (p. 1).

One can only regret that we will no longer have the opportunity to share the insights of a promising scholar.

STEPHEN SLOAN

Air University; University of Oklahoma

China's Foreign Relations in the 1980s. Edited by Harry Harding. (New Haven, Conn.: Yale University Press, 1984. Pp. xv + 240. \$18.50.)
China and the World: Chinese Foreign Policy in the Post-Mao Era. Edited by Samuel S. Kim. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 354. \$42.50, cloth; \$16.95, paper.)

Although both these books are edited collections dealing with Chinese foreign policy, they are different in intention and substance. *China's Foreign Relations in the 1980s*, despite its title, is a collection of articles approaching post-1949 Chinese foreign policy from several perspectives.

Although there is no common argument to integrate the essays, the differing perspectives highlight the significant issues in studying Chinese foreign policy and compel the reader to reach his or her own conclusions.

The introductory chapter by Michael Hunt observes that there have been multiple foreign policy traditions during China's long history—ranging from the Middle Kingdom approach to participation in an amoral interstate system characterized by "Machiavellian cunning." History has left China with a security concern for peripheral areas and the search for a response to the humiliation by Western countries. Alternative responses have been "cosmopolitan" or "populist," entailing "dependency" or "autonomy." Although Hunt argues that Mao reduced tensions by combining strategies, Kenneth Lieberthal, addressing the role of domestic politics in Chinese foreign policy, reveals continued tensions between three contending policy packages. Lieberthal reveals that different foreign policies affect leadership stability and that leaders' domestic goals have encouraged particular policies. He argues that Mao promoted the Sino-Soviet dispute to enhance his power base, suggesting that the strategic triangle resulted primarily from domestic forces.

Bruce Reynolds examines Chinese international economic policy, distinguishing the factors shaping policy, including the restraints of the domestic economy and the international system. Of these factors—economic efficiency, ideology, and political strategy—the last has been most influential. The key of Chinese development lies in the reform program; if implemented, China will be able to take advantage of the opportunities in the international economy.

Steven Levine addresses a recurring issue in international politics: Are conflicts regional or global, and does the state's response accurately reflect the real situation. He argues that although China is a regional power and the issues it faces in Asia are regional, it has treated them as global, that is, as Sino-Soviet issues. China thus needlessly exacerbates conflict, particularly concerning Vietnam's role in Indochina. Yet insofar as Sino-Soviet conflict is primarily waged in Asia, regional and global may be identical in this case. Jonathan Pollack analyzes China's global role, emphasizing the international restraints on policy toward the superpowers. The strategic triangle emerged when China played balance of power politics to offset the Soviet threat. In contrast to Levine, Pollack asks what accounts for China's crucial role in international politics despite its relative weakness. Pollack's approach also contrasts with Lieberthal's emphasis on domestic politics in foreign policymaking.

Harding concludes the volume with an overview of China's changing role in international politics since 1949, periodizing the history into five eras and observing the changes and continuities in Chinese policy. He argues that in contrast to past cyclical movement between extremes, the current independent, moderate policy will be relatively stable. Together, these essays provide much intellectual stimulation for students and scholars of Chinese foreign policy, and are useful for those seeking a general understanding of Chinese policy.

China and the World has more ambitious goals. It tries to approach Chinese foreign policy within the context of political science theory, bringing together the work of China scholars and specialists in international politics. The case studies, however, for the most part fail to incorporate the theoretical or methodological approaches called for in the first part of the book. This weakness underscores the necessity for further work of this kind.

The introductory essay by Samuel Kim delineates the various approaches to Chinese foreign policy and points the direction for fruitful research by disaggregating the various aspects of the study of Chinese foreign policy. Essays by Davis B. Bobrow and Steve Chan and by Michael Ng-Quinn argue for methodological rigor and systemic analysis. Ng-Quinn argues that the world is bipolar and that Chinese policy should be viewed from this perspective. Unfortunately, he examines primarily the theoretical merits of bipolarity over multipolarity, giving little attention to actual Chinese policy. Susan Shirk's essay discusses domestic politics and foreign policy. She offers an excellent analysis of the regional, sectoral, and bureaucratic politics of foreign trade policy. Yet this is a unique issue area, and its implications cannot be generalized.

The second and third sections include many first-rate essays, equal in quality to those in *China's Foreign Relations in the 1980s*. For the most part, however, they do not deal with the issues raised in the first section. Steven Levine observes the limits to U.S.-China cooperation and analyzes various explanations for continued conflict. Chi Su explains improved Sino-Soviet relations in the 1980s from both Soviet and Chinese perspectives, and Donald Klein discusses policy toward the "Second World." Only Samuel Kim distinguishes between the reality and Beijing's own description of its third-world policy and between domestic and external sources of policy change, arguing that China's policy is more self-serving than its media indicates and that the source of its recent renewed interest in the Third World is primarily domestic politics.

The final section deals with various policy

issues. Paul Godwin considers the fears of Beijing's defense establishment concerning Moscow and its resultant dissatisfaction with current defense spending and China's "independent" foreign policy. William Feeney discusses China's role in international financial organizations, emphasizing PRC impact on the organizations and China's use of them in its development plans. Dennis Fred Simon places China's quest for foreign science and technology in the context of overall PRC foreign policy, arguing for the continuity in policy, regardless of instability in bilateral relations. He also considers the various international and domestic obstacles to Chinese science and technology acquisition. Only Bruce Cummings explicitly uses theory, arguing that the international capitalist system, with the United States as the hegemon, dictates PRC international economic policy. PRC policy has conformed to U.S. desires and is the result of U.S. pressure. Allen Whiting considers the implications of the various authors' conclusions for the discipline and uses the various analytical perspectives to consider future Chinese behavior on issues likely to be of continuing concern to the international community.

The essays in this volume are highly informative. Moreover, this is an important volume because it represents a significant step in integrating the study of Chinese foreign policy with the international politics discipline. It should be read by both graduate students and by scholars interested in Chinese foreign policy.

ROBERT S. ROSS

Columbia University

The Illogic of American Nuclear Strategy. By Robert Jervis. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 204. \$19.95.)

Even though this book, like many other well-reasoned, logical presentations, may have little impact on America's headlong rush to acquire all kinds of irrelevant and dangerous nuclear weapons systems, it should be read by as wide an audience as possible in the forlorn hope that reason may yet prevail. Although Jervis is at times almost Thomistic in his method of argument with objections, replies to objections, and counter replies to replies, the book does make a great deal of sense and can be understood by the intelligent layperson willing to devote undivided attention to the argument. The subject matter is obviously crucial, and Jervis brings to it extensive knowledge in the area as well as impressive analytical rigor.

Jervis starts from the assumption that nuclear weapons have made traditional ways of viewing international relations and the use of force irrelevant. Unfortunately though, much of the thinking in this area still uses categories and methods of analysis that might have been appropriate to the world of von Clausewitz but are no longer so. The problem is graphically presented in the opening line, where Jervis says: "A rational strategy for the employment of nuclear weapons is a contradiction in terms" (p. 19). Never before in history has the potential loser of a war had the capability to totally destroy the potential victor. In other words, nuclear weapons offer no defense, no matter how much either side spends on weapons.

The tensions and inconsistencies generated by weapons that do not fit into previous ways of viewing things have led to a variety of what Jervis calls "escapes." Briefly dismissing three of these escapes—defense, total nuclear disarmament, and the nuclear freeze—the author then focuses on "conventionalization," which underpins much of current American nuclear doctrine. This view regards nuclear weapons as if they were essentially like, but simply bigger than, conventional ones. Jervis devotes one critical chapter to a major implication of conventionalization: the argument that American security requires the establishment of escalation dominance, or the ability to defeat Soviet aggression at all levels of violence short of all-out war.

Having presented some of the problems and inconsistent assumptions prevalent in much strategic thinking Jervis then devotes two chapters to an analysis and critique of the present countervailing strategy which builds those fallacious assumptions into full-fledged incoherence and contradiction. Although some early indications of the policy can be found in the 1950s and 1960s, it is usually dated from the late 1970s under Secretary of Defense Harold Brown and has been continued with only cosmetic modifications by the Reagan administration. Although these chapters require close reading, they are worth the effort because of the vigorous way in which they demolish Brown's argument.

Having demonstrated that countervailing strategy is not necessary for deterrence, Jervis concludes by showing that the dangers and costs of the strategy outweigh any supposed gain that it might have. These costs include the expense of the weapons required, the strain on the European alliance, the fact that the strategy might be perceived by the Russians as a threat to their security, and the possibility that decision makers might begin to believe the fallacious assumptions of the strategy leading them to suppose that they could control high levels of conventional violence

and even limited nuclear wars. In addition, more general political problems might result from this strategy including the tendency to believe ourselves that we must deploy the necessary forces to be able effectively to deter and then—when the forces are not forthcoming—believing that we do not have effective deterrence capability when in fact we do. Finally, there is the problem that the Soviet Union might become so desperate because of internal and external pressures as to perceive that war is necessary or inevitable. Although Jervis offers little in the way of alternatives, that is not his purpose. Pointing out the problems and dangers in current American nuclear policies is a sufficient accomplishment for any book, and this one does that so well that I would recommend it as must reading for everyone concerned with the fate of the world.

PHILIP W. DYER

University of Nebraska-Lincoln

The Dynamics of Latin American Foreign Policies: Challenges for the 1980s. Edited by Jennie K. Lincoln and Elizabeth G. Ferris. (Boulder, Colo.: Westview Press, 1984. Pp. xvi + 325. \$35.00, cloth; \$14.95, paper.)

As the editors of this volume suggest, scholars have generally treated Latin American countries as an object of U.S. foreign policy attention rather than as a set of autonomous or semiautonomous national units capable of formulating and implementing foreign policies of their own. This is partly a reflection of historical reality in that Latin America's economic dependence created a set of structural constraints for national policy makers that encouraged reactive behavior rather than active initiatives taken within a coherent foreign policy framework. The editors further suggest that when the foreign policies of individual Latin American countries have been studied, little use has been made of the comparative theoretical perspectives available in the broader foreign policy literature. The case study method has predominated, with scholars at best analyzing the impact of select national attributes such as capabilities, level of national development, and ideology.

Lincoln and Ferris attempt to move regional foreign policy analysis beyond these narrow confines by examining changes in the international systemic context that have encouraged the formation of more active and coherent regional policies and by presenting case studies to document these

changes. Changes in the international system including increased multipolarity after World War II are viewed as having encouraged new adaptive foreign policy strategies aimed at maximizing national autonomy. A central focus of their analysis is the question of whether this movement toward greater autonomy will extend through the 1980s in light of the deepening regional economic crisis and increased U.S. propensity to intervene (e.g., the Falklands/Malvinas, Grenada, Central America).

The body of this book consists of a series of essays that further specify the nature of the regional systemic context and examine the foreign policy behavior of individual countries. Jack Child provides a thought-provoking examination of emerging conflict patterns in the region, arguing that the early post-World War II period of Latin American interstate relations was unusually tranquil. He sees it as being rapidly replaced by a more difficult period of ideologically motivated conflict in the Caribbean Basin and geostrategically motivated middle-power conflict in South America. Yale Ferguson traces the sporadic and perhaps reversible movement toward regional integration, and Paul Sigmund examines changing patterns of U.S. behavior toward Latin America.

Of the 10 essays that deal with individual countries, the most valuable in light of the book's stated purposes are those on Argentina, Brazil, Bolivia, and Mexico. Dennis Gordon convincingly argues that Argentina's post-Malvinas political and economic problems will result in a limited regional role through the 1980s. Wayne Selcher notes the increasing institutionalization of a broad-based and complex foreign policy community in Brazil and suggests that this community may resist central control and direction. Waltraud Queiser Morales places Bolivian foreign policy decisionmaking within the context of a regionally fragmented polity wracked by political instability. And co-editor Elizabeth Ferris notes six internally contradictory facets of Mexican foreign policy that explain external behavior as primarily a function of a deepening domestic political crisis.

Of the two general tasks implied in this volume's title (explaining the dynamics of area foreign policies and specifying the nature of these policies in the 1980s), the editors and chapter authors more successfully accomplish the latter. In spite of the inclusion of an excellent final essay by Ferris which suggests some hypotheses that might move us toward a truly comparative theory of Latin American foreign policies, the chapters are not written with such a set of hypotheses in mind. Rather, they represent some of the best that the traditional case study method is capable of producing. However, while we await the more theoretically interesting book that Ferris's con-

cluding essay suggests will someday be written, we can benefit from a close reading of this one.

STEVE C. ROPP

The University of Wyoming

The British Nuclear Deterrent. By Peter Malone.
(London: Croom Helm Ltd., 1984. Pp. 200.
\$27.50.)

The quixotic history of Britain's independent nuclear deterrent, and particularly of U.S. involvement with those forces, is nicely captured by two quotes. The first, from 1962, is Robert McNamara stressing the then widely held view of the American government that national nuclear forces are "dangerous, expensive, prone to obsolescence and lacking in credibility as a deterrent." By contrast, following discussions between President Carter and Prime Minister Thatcher in 1979 on British acquisition of the Trident SLBM that would maintain the U.K. independent deterrent well into the twenty-first century, the two leaders issued a joint communique in which they "agreed on the importance of maintaining a credible British strategic deterrent force and U.S.-U.K. strategic cooperation."

Granted, the U.S.-U.K. special relationship has not been the dominant factor in Britain's acquisition and modernization of an independent deterrent. As Peter Malone persuasively argues in this excellent analysis, the dominant feature of British policy has been precisely to avoid sacrificing "operational independence upon the altar of Anglo-American cooperation." Thus, despite the very tangible economic and technical benefits of the American connection, as well as the more symbolic political benefits of having an "independent" deterrent, British nuclear weapons policies ultimately have revolved around fundamental security considerations.

The history of these evolving strategic issues, beginning in 1940 when Britain became the first country to explore the military potential of atomic power, has been well documented before. Nonetheless, Malone brings to the subject both a comprehensive analysis, covering the relevant technical, political, economic and doctrinal issues, and a lively narrative style.

Where the book could be most interesting to those familiar with the topic is Malone's analysis of the current relationship of U.K. nuclear forces to Britain's overall strategic goals, and the conventional forces underlying those goals.

Interestingly, the debate now going on in Britain over the merits of a "maritime" versus a "continental" strategy is similar to that occurring in the United States. For the U.K., of course, even

the appearance of such a debate is a marked departure from a centuries-old tradition of maintaining naval supremacy. Despite the decline of British resources in the postwar period and the retrenchment of Britain's military commitments, symbolized by the 1968 "east of Suez" withdrawal, the Royal Navy remained preeminent among the military services. As Malone points out, Minister of Defence John Nott was "dismayed" to learn in 1981 that more money was being spent on fleet air defense than on air defenses for Britain itself.

Yet the fact that the navy was forced to bear the brunt (57%) of defense spending cuts in 1981 proved only a temporary setback. As if in answer to one naval officer's supplication that "what we need now and quickly is a small colonial war requiring a lot of ships," the Falkland Islands crisis erupted in April, 1982. Following the Royal Navy's excellent performance in the south Atlantic, a December, 1982 review of defense priorities reemphasized capabilities for intervention missions, not only for the navy, but also for the army and air force.

Central to the debate on whether Britain can afford to shoulder both a continental option (British Army on the Rhine) and a maritime/quick response intervention capability is the expense of acquiring Trident SLBMs. For its part, the Thatcher government has argued that deterrence is better served by maintaining U.K. nuclear forces than by buying the two armored divisions and 300 tanks that cancellation of Trident could provide. As Malone notes, however, the fate of Trident should the Labour party return to power will not hinge on opportunity cost considerations alone. Strategic considerations of how Britain's military power can be maximized in a period of constrained governmental budgets, escalating costs of conventional weaponry, and antinuclear political sentiments will continue to foster a lively debate on the whys and wherefors of the U.K. independent deterrent.

JEFFREY BOUTWELL

American Academy of Arts and Sciences

International Studies and Academic Enterprise: A Chapter in the Enclosure of American Learning. By Robert A. McCaughey. (New York: Columbia University Press, 1984. Pp. xviii + 301. \$28.00.)

This is a stimulating and revealing social history of an important sphere of American intellectual life. What Robert A. McCaughey has written about international studies may be somewhat discomforting for many who are associated with

the field. But his thoughtful, if critical, scrutiny may also provide the basis for fruitful discourse over appropriate adjustments and changes that can maintain international studies as a thriving concern.

Part 1, "In the Land of the Blind" (1810 to 1940), traces the origins of American international studies as an "intellectual enterprise" and its gradual emergence as an "academic enterprise." Complementing the account are insightful career profiles of some of the individuals who played major roles in the initial period of this process (Archibald G. Coolidge and James H. Breasted, among others). McCaughey skillfully delineates the process by which international studies changed from being the realm of "gentlemen and scholars" to the "enclosed" preserve of academic specialists. It was a process that had its costs, along with the undeniable advantages. When intellectual pursuits came to be seen as primarily the business of universities, and recognition and status was conferred upon research-oriented professors, the full enclosure of international studies as an academic enterprise was inexorably brought to completion.

Part 2, "The Years that Were Fat" (1941 to 1966), considers the phenomenal transformation of the established academic enterprise resulting from the impact of World War II, the funding operations of the Ford Foundation, and the enormous expansion of American universities. The war and the immediate years afterwards brought the enterprise of international studies to its "takeoff" stage. Of seminal importance was the appearance of the Gaither Report, with its pervasive internationalism. Now international studies as an enterprise was truly on its way. The very magnitude of the Ford Foundation's funds—McCaughey discusses the foundation's struggle to match grants with its staggering income—required rapid expansion. Ultimately, however, its support concentrated on the universities with established reputations in international studies, while international studies academics were allowed to determine what areas to support. The results were not altogether laudable. McCaughey finds relatively little progress after 15 years of the International Training and Research Program (ITR) and about \$250 million.

The analysis reveals a gap between needs and capacity. The massive intervention into the life of the enterprise hastened its full enclosure within the universities, thereby contradicting the explicit purpose that had prompted that intervention in the first place. The greatly reduced dependence on outside sources lessened the need to write for popular audiences. Indeed, popularization became "inappropriate." An epilogue addresses the so-called "crisis" in international studies

following the termination of ITR. The nicely executed tallies of Ph.D.'s reveal some interesting information, not the least of which is that the end of ITR may have been in some respects a blessing in disguise. The data suggest, for example, a growing dispersion of production capacity and a leveling of institutional reputations after 1966.

McCaughey concludes that international studies is a "saturated activity." If, as an academic enterprise, it is beset by a "crisis," it might be mindful again of its earlier constituencies. That, however, would oblige the enterprise to be a little less self-centered. Hopefully, this well-written and illuminating study serves as a spark for serious reassessment. It is a work deserving a readership far beyond that of the international studies community, for it entails much that is of interest for anyone concerned about the future of higher education.

MANFRED GROTE

Purdue University Calumet

Foreign Policy and the Modern World-System.

Edited by Pat McGowan and Charles W. Kegley, Jr. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 287. \$25.00, cloth; \$12.50, paper.)

One of the most exciting developments in recent international politics research has been the "rediscovery" of history. Spurred first by Immanuel Wallerstein's "capitalist world-system" model and later by George Modelski's "long cycles of political leadership" approach, many scholars are now reconsidering their research agendas and approaches. The essays in this book, Volume 8 of the *Sage International Yearbook of Foreign Policy Studies*, take these ideas and explore their relevance for the comparative study of foreign policy.

In their introduction, McGowan and Kegley assert that the comparative study of foreign policy has suffered because of an overabundance of narrowly focused, quantitative studies. They hope that consideration of a world-system perspective can result in "a more judicious balance between quantitative and qualitative, historical and current, case-study, comparative, and statistical research designs" (p. 9).

The first two essays focus on theoretical issues. After pointing out potential weaknesses in Wallerstein's model, James Lee Ray notes some of its potential advantages. Beyond increasing historical awareness, it offers more easily operationalized variables, greater parsimony in theorizing, and a potential way to integrate traditional international politics and the study of international political

economy. William R. Thompson's essay directly compares the Wallerstein and Modelski models and notes their disagreement regarding "system time." For example, the contemporary period falls in different phases of the two models, producing different predictions and explanations. Like Ray, Thompson calls for a synthesis of the two models because Wallerstein's focuses primarily on economics and Modelski's on military and political issues.

Three essays are offered as historical studies. Michael S. Kimmel notes the domestic constraints faced by core powers in the sixteenth and seventeenth centuries. Harriet Friedmann addresses the interaction of economic logic and state power. She argues that a focus on price structures can marry the logic of economics to that of state roles. Michael Stohl examines the negative economic consequences of hegemony, which include a widening of income disparity and a weakened domestic economy.

Three particularly strong essays address the contemporary period. John Boli examines the paradox of Swedish foreign policy. Although a member of the capitalist core, Sweden often undertakes anti-core foreign policies. While noting the role of Olaf Palme and Sweden's history of neutralism, Boli says Sweden has the advantage of membership in the core without the financial or political responsibilities of supporting the capitalist world-system. Boli's emphasis on the role of Sweden as a subcore power makes possible generalizations to other subcore powers like Norway, Denmark, the Netherlands, Belgium, Switzerland, Austria, and Canada.

James Hawley attacks the reductionist element of Wallerstein's model, saying state actions are understandable only on the context of their times and are not economically determined. After studying recent American capital control programs, Hawley argues that the Wallerstein model ignores or underestimates the impact state actions have on the structure of the world-system and incorrectly assumes that state policymakers are responsive only to the dominant economic classes in their society.

In a frightening essay, Albert Bergesen asserts that we have passed through his first two phases of system change: the decline of the hegemonic state and the hegemonic state's reliance on treaties and alliances to preserve its power. Bergesen says we are entering the third phase of open friction and international incidents. The final phase is systemic war. Bergesen predicts that World War III will begin between the superpowers' third-world clients. If nuclear holocaust does not result, both the Americans and Soviets will be too economically weakened to be the postwar hegemonic power. Japan will assume this role

based on its structural position; similar to that held by the United States in 1914, and its leading role in the merger of state and firm, which Bergesen sees as the economic transformation of the future.

Overall, two essays do not fit well with the others. Christopher Chase-Dunn offers an idealistic prescription for a socialist world-system, and Michael V. Esler presents a comparative foreign studies bibliography for the period from 1978 to 1981. The book suffers from the lack of an integrative conclusion, but the stimulation provided by the essays more than offsets these weaknesses. This work should be read by both researchers and graduate students.

RALPH G. CARTER

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Latin American Nations in World Politics.

Edited by Heraldo Muñoz and Joseph Tulchin.
(Boulder, Colo.: Westview Press, 1984. Pp. ix + 278. \$30.00, cloth; \$12.95, paper.)

Latin American Nations in World Politics is a welcome addition to the nascent field of Latin American foreign policy study. It departs from previous works by taking preliminary steps toward greater theoretical clarity. The introductory chapter provides a set of theoretical perspectives marking differences between external and internal sources of foreign policy. Following is a series of chapters that place interesting country and regional case studies within the conceptual categories outlined at the beginning. The volume rightfully emphasizes the enormous diversity of foreign policy options available to Latin American states. For too long, it was presumed that variations were not present given Latin America's dependence on the United States. As the book demonstrates, countries (like Brazil) with strong economic ties to the United States have shown remarkable diplomatic and political independence from—indeed defiance of—their North American neighbor.

External variables, including power politics and geopolitics, are integrated into useful discussions of the Caribbean Basin and the Southern Cone. Two authors connect on the declining U.S. military presence in the Caribbean region under President Carter with the rising influence of mid-level powers like Mexico, Venezuela, and Cuba. National frontiers and territorial expansion became important issues in relations between Argentine and Chilean dictators. On one hand, authors point out the interaction between objective boundary disputes and the heightened sensi-

tivity to such disputes that arise with a change from democratic to authoritarian regimes.

On the other hand, the authors show how regime change is not always critical, particularly where the diplomatic core retains autonomy from political leadership and maintains its historical continuity. An interesting contrast is drawn between the highly independent foreign ministry in Brazil, which formulated progressive policy despite a conservative regime, and the Chilean foreign ministry under General Pinochet, which became an ideological tool of a president who never trusted the career diplomats. Other internal factors discussed in this volume include the role of political parties, presidential styles, bureaucratic politics, and dominant national projects.

Comparative foreign policy analysis is an important field of inquiry, and this book makes a contribution in that area as well. Chapters on the Caribbean link foreign policy differences and similarities between countries to the nature of the party system (one party in the case of Mexico and Cuba, and two party in the case of Venezuela and Colombia) and the relation of each nation to the superpowers (whether it is strong as in the case of Cuba and Colombia, or weaker as in the example of Mexico and Venezuela).

Competing explanations are often offered for the same country and regime. Thus, the foreign policy of the military junta of Argentina is explained both by the dominance and the failure of its national project. This is hard to avoid in a volume of this sort where so many different variables are discussed by numerous scholars. Perhaps the addition of a concluding chapter would have been helpful, in which the editors tried to synthesize what had come before. What is needed is a multivariable approach that parcels out the independent *and* interactive contributions of a set of factors to the development of Latin American foreign policy. It is to the editors' credit that they are conscious of their own limitations, calling for future conceptual schemes that "are sufficiently broad to allow for multicausal explanations" (p. 16). The volume is a very nice contribution to the field because of its attention to theory, the breadth of its focus, its comparative emphasis, its sense of its own limitations, and its reflection on the need to go further in the direction of theoretical rigor.

DAVID PION-BERLIN

The Ohio State University

Negotiating Peace: War Termination as a Bargaining Process. By Paul R. Pillar. (Princeton, N.J.: Princeton University Press, 1983. Pp. 272. \$25.00.)

Negotiating Peace is a skillful effort to employ the theory of bilateral bargaining to understand the terminal phase of wars from the initial agreements to beginning negotiations to final settlement. It is a book with many strengths and a few weaknesses. A central theme is that the size and shape of the bargaining set, in particular whether it is convex, will influence two crucial elements in negotiated conclusions to war. First, it is persuasively argued that non-convexities in the bargaining set will produce unstable one-sided dynamics in the process of reciprocal concession between negotiators. Second, these non-convexities will accentuate the effectiveness of military violence on the battlefield as a lever in the diplomatic negotiating process. For both phenomena, non-convexities lead toward extreme or corner solutions. Next, Pillar argues that relevant non-convexities occur when the object of the war, the prize for which it is fought, is indivisible. For these, there must be a winner and a loser, compromise is infeasible, and thus mixed interior solutions unattainable. On the other hand, the spoils of war may be partially or wholly divisible or even partitioned in such a way that the aggregate benefit is higher than if one side grabbed everything; in this case, the dynamics of concession should be reciprocal, cost-imposing military ventures should exercise less leverage over the outcome, and the most likely negotiated solution will be compromise "interior" solutions. Taking this idea to a limit, a salient solution to the bargaining problem may bear resemblance to joint public good consumption.

As one travels conceptually from the one extreme of total indivisibility to the other of total jointness of consumption, questions of measurement and interpersonal (international) comparisons of gains from settlement become especially important in the middle ground between extremes. Disappointingly, Pillar has rather little to say on this question except for the apt and correct observation that the slope of the bargaining set is logically derivable from the comparative importance of outcomes as between opponents relative to the comparative importance of costs of disagreements (p. 155). And the almost exclusive concentration on the Nash solution to the bargaining problem is a further source of disappointment. The opportunities to threaten actual damage may differ between opponents so that asymmetries in threat deserve more treatment. Similarly, better development of the consequences of side payments, of the ability to bind oneself in

advance to commit to actions one will later regret having to perform, and various other Schelling-type maneuvers would have been profitable.

The book nevertheless repays careful reading. A limited empirical section with casual regression analysis yields partial support for the Cournot-Nash theories on which the work is founded. A more special strength, however, is the impressive assemblage of anecdotal material on war terminations in the nineteenth century through the Korean, Algerian, and Vietnamese conflicts. This is impressive scholarship relating the histories of internal decision calculus and of battlefield developments to the course of negotiations, with comparisons drawn between the predictions of bargaining theory and the actual course of events.

MARTIN C. MCGUIRE

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Hanging Together: The Seven-Power Summits.

By Robert D. Putnam and Nicholas Bayne. (Cambridge, Mass.: Harvard University Press, 1984. Pp. x + 263. \$17.50.)

The interface between international and domestic politics is perhaps nowhere clearer than in the annual seven-power summits bringing together the chiefs of government of the most powerful advanced industrial states. Initiated in 1975 at Rambouillet in France (Canada was not yet a participant), the summits were convened every year, Bonn being the venue for the latest occasion in May, 1985. Considering the enormous significance that these annual meetings of the leaders of the United States, Great Britain, France, West Germany, Italy, Japan, and Canada can have for the solution of international problems and for possible cooperation, the historical account, analysis, and evaluation of the summits as an evolving process in international politics provided by Putnam and Bayne fill an important need.

The book is especially valuable for international relations scholars because it seeks to apply theoretical concepts to the summit process. It focuses on the reasons for the institutionalization and bureaucratization of the summitry, examines the interplay between economic interdependence among the individual democracies and the exercise of national sovereignty, explores the problem of reconciling the hegemonic role played in varying degrees by the United States since 1945 and the need for the collective management of economic activity by the summit countries, and discusses the well-known awesome abil-

ity of the bureaucracies to stymie the wishes of political leaders in connection with summit objectives.

The book covers the summits from 1975 at Rambouillet to 1983 at Williamsburg. The authors consider the summits as useful exercises, although they give the Versailles meeting of 1982 a failing mark and the London summit of 1977 a minimal success grade. Although some observers are doubtful about the value of the annual exercise, the authors believe that if the summits were to cease, "it would seriously weaken the cohesion of the West" (p. 214). Moreover, the personal ties established through the summit process between the governmental leaders of the major powers of the West "can lubricate international relations" (p. 197).

Not surprisingly, the summits have changed since 1975. They began as general and relatively informal discussions but quickly became bureaucratized and assumed a high degree of institutionalization. With months of preparatory meetings, large delegations drawn mainly from the finance, economics, and foreign ministries became involved. In addition, a number of relevant IGOs participated in the preparations of the summits, most prominently the OECD, GATT, and IMF. The reason for the extensive involvement of these organizations and their bureaucracies was that certain very important themes cropped up in all summits: problems of macroeconomic policy coordination, international monetary issues, and international trade. This raises the question of whether the summits, by establishing a large planning staff under the guidance of so-called sherpas (named after the Himalayas guides), were not duplicating the functions of existing organizations. Although Putnam and Bayne concede that it is "necessary to hold back the growth of bureaucratic apparatus," the right framework must be created "for the leaders to exercise their unique capabilities—for integrating varied subjects, balancing domestic and external pressures, and exercising high-level political authority" (p. 215).

The summits have become media events. Although in 1981 the number of reporters at the Ottawa summit was estimated to be about 1,500, at the 1985 Bonn summit nearly 4,000 print and television journalists had been accredited by the West German government (*Time*, May 13, 1985, 21). This is an unwelcome development, because the media have tended to present the summit as a "kind of international jousting match, and the summiteer as a national paladin, vigorously (and usually victoriously) defending national interests against obtuse and even malevolent foreigners" (p. 176).

Putnam and Bayne have made a significant

contribution to our knowledge of and theoretical insights into an important international development. Their book is well worth reading by international relations scholars, historians, and those generally interested in the problems of bureaucracy. For theory construction, the implications of *Hanging Together* may be far-reaching and may stimulate further empirical research. What, for example, is the extent and effect of frequent transnational bureaucratic interaction on middle and upper levels of the summit governments and involved IGOs? What are the parameters of educational and socializational processes spawned by the summit procedures and events? In which way do the personal interactions between the leaders enhance their empathy regarding the concerns and culture of the people in the other summit countries? Would it be theoretically feasible to regard the series of summits, because of their institutional development, as an evolutionary international regime for the coordination of national policies and other activities?

WERNER J. FELD

University of New Orleans

The Soviet Union and the Czechoslovak Army, 1948-1983: Uncertain Allegiance. By Condoleezza Rice. (Princeton, N.J.: Princeton University Press, 1984. \$37.50.)

Condoleezza Rice has written a first-rate book; one that not only fills a major gap in our knowledge of civil-military relations in a key country in Eastern Europe, but one that also provides useful insights into the impact of the Soviet factor on Czechoslovak civil-military relations.

In essence, Rice argues that there is a built-in tension in Czechoslovak civil-military relations as a result of the need of the armed forces to be loyal to two masters—the USSR and the Czechoslovak Communist party. As long as Moscow and Prague's interests coincided, this tension was manageable. When they began to diverge, however, problems developed. For example, Khrushchev's decision to permit greater emphasis on national identity led to a search for the armed forces' national traditions. The events surrounding the Prague Spring intensified this process and led to a push for rapprochement between an increasingly independent party leadership and a military that was looking for Czechoslovak solutions to the country's political-military problems. The Soviet invasion reversed the rapprochement process and in the crushing purge that followed in 1969, the armed forces were decimated. As a result, the tension of the sixties disappeared. But

the cost was high. Not only is the military estranged from the populace (which sees it as a tool of the USSR), but the quality of officer and non-commissioned officer cadres also remains low.

Rice's attempt to take Czechoslovak civil-military relations out of their purely domestic context and place them within the broader perspective of Soviet-East European relations is to be welcomed and marks an important step forward in the analysis of civil-military relations in communist systems. It will take time and many more studies—as Rice acknowledges—before we will find the proper balance between the domestic and Soviet factors, but clearly Rice's book opens a fruitful path for further analysis.

Rice also provides a wealth of useful data on Czechoslovak civil-military relations. This is especially true of the 1968 events. She provides invaluable analysis of splits in the military (with the reformers coming primarily from the Main Political Administration), of the attempt of the senior military leadership to find a Czechoslovak way for dealing with the country's problems, and of the attempt to Sovietize the military in the aftermath of the Soviet invasion.

This book could have been improved by a more critical use of certain concepts. In speaking of Sovietization in the 1969 to 1970 period, for example, Rice focuses on three areas: doctrine, political education, and integrated (i.e., multinational) training. Although few will argue that the move against the development of a Czechoslovak military doctrine was anything but a clear sign of Sovietization, the other two indicators have much broader significance. The content and effectiveness of political education has as much importance as its frequency. Furthermore, despite the prominence that some writers have assigned to Soviet efforts to coordinate political education throughout the Warsaw Pact, its value should not be overestimated. It is noteworthy, for example, that many East European writers continue to lament its effectiveness. Likewise, the push for more integrated training has little to do with Sovietization. It is also a prerequisite for coalition warfare. It would be hard to imagine a combined arms operation without the necessary integrated training exercises beforehand. The book would also have been strengthened by more discussion of the Czechoslovak-Warsaw Pact connection, that is, What is the nature of the interface? Greater emphasis on the ethnic factor would have been useful (the appointment of Colonel General Milan Vaclavik, a Slovak, to succeed Martin Dzur as Defense Minister suggests that the ethnic factor remains a major concern in the military). Finally, the chapter on the period since 1975 strikes me as a bit superficial, especially when compared with the rest of the book.

Despite these criticisms, this book is a must for anyone interested in civil-military relations in communist systems. It is, as Timothy Colton has noted, "by far the best work yet written on military politics in contemporary Eastern Europe."

DALE R. HERSPRING

Washington, D.C.

An International Law of Guerilla Warfare. By Keith Suter. (New York: St. Martin's Press, 1984. Pp. x + 192. \$27.50.)

Hans J. Morgenthau commented in a 1946 article in the *Yale Law Journal* that "the questions which the law and the lawyer answer are largely irrelevant to the fundamental issues upon which the peace and welfare of nations depend." Keith Suter has elaborated upon this notion in an excellent multilayered study of the 1977 creation of the two additional protocols to the Geneva Convention directed at the question of guerilla warfare. The book describes the "long journey" that began in the mid-1960s in the mind of Sean McBride, general secretary of the International Commission of Jurists, who sought to raise the issue of human rights in armed conflict. The journey continues with a resolution from the 1968 Geneva NGO Human Rights Conference and another from the 1968 Tehran International Conference on Human Rights requesting the Secretary General to initiate efforts to secure the application of humanitarian conventions in armed conflict and to assess the need for new conventions. The scene shifts to the United Nations at this point, where in 1968 the General Assembly passed Resolution 2444 (XXIII) affirming a review resulting in a series of reports from the Human Rights Division in 1969, 1970, 1971, and debates in the General Assembly. All of these efforts were to culminate in the 1974 Diplomatic Conference.

Suter is not content to merely chart the progress of the efforts to create the 1977 protocols. On the contrary, at each step of the way he analytically assesses the strengths and weaknesses of the various drafts and resolutions, arguing stoutly, for example, that the human rights approach to armed conflict confused a human rights regime and the laws of armed conflict. Further than that, he explores the variety of roles and pressures exerted by the NGOs, especially the International Committee of the Red Cross (ICRC). Suter notes that "the International Red Cross movement had the rare distinction of being simultaneously one of the best known international movements and one of the least known" (pp. 85-86). The Geneva Protocols of 1949 are the particular province of the

ICRC as they monitor observance of the provisions and periodically make efforts to update them. The ICRC had made such efforts before but were particularly vigorous in 1970 not to let the initiative and control of the process of drafting new conventions slip from their grasp. Through conferences held in 1971 and 1972, the ICRC developed two draft protocols to the four Geneva Conventions. The Geneva Conventions, as Suter points out, were sacrosanct and not subject to revision.

The culmination of the initiatives of McBride, the United Nations, and the ICRC was the series of diplomatic conferences held in 1974, 1975, 1976, and 1977. True to patterns observed elsewhere in massive multilateral diplomacy, the first gathering was a political ritual of third-world condemnation of racism, imperialism, Zionism, and disputes over procedure and recognition of liberation movements. The latter, of course, was particularly contentious as it was in fact part of the substantive concern of the meeting. The conference almost broke down but reconvened in ensuing years and produced the two protocols. As of 1983, only 31 states had ratified Protocol 1; only 25, Protocol 2.

Suter concludes his study with an assessment of the new law and asks what went wrong. A 20-year effort to update the law of armed conflict to make it applicable to guerilla forces produced protocols that would have minimal impact in such conflicts. Suter elaborates a number of reasons for failure, such as the third-world ambivalence on such conflict in which they wish to help national liberation movements but certainly not within their own borders. Suter suggests that the ICRC's unwillingness to go to the foundations and reassess the Geneva Conventions despite their "near moribund" state resulted in grafting overly complex protocols to an overly complex set of Conventions that speak little to the dominant form of contemporary armed conflict. The conservatism of the legal specialists contributed as well. Suter notes: "the Protocols are too long, too complicated, too full of qualifications and esoteric legal language to be practical ways of regulating conflicts" (p. 182). The new protocols were directed at both international and "non-international" level and provided neither the clarity, nor more importantly the incentive, for either governments or guerillas to grasp the provisions and to obey them.

Suter's study of the protocols is a fascinating parallel to the Law of the Sea conferences which produced a contentious but much more substantive result. Armed conflict cuts closer to the security and survival of states, and the weakness of the protocols may only be that it is, as Stanley Hoffmann said, "a magnifying mirror that reflects faithfully and cruelly the essence and logic

of international politics" (in L. Scheinman and D. Wilkinson [eds.], *International Law and Political Crisis*, Little, Brown, 1968, p. xvii).

RICHARD B. FINNEGAN

Stonehill College

Dangerous Relations: The Soviet Union in World Politics, 1970-1982. By Adam B. Ulam. (New York: Oxford University Press, 1983. Pp. vi + 325. \$7.95, paper.)

This book is the long-awaited and fitting update of Ulam's *Expansion and Coexistence: Soviet Foreign Policy, 1917-1973* (Holt, Rinehart & Winston, 1974). According to Ulam's preface, the book under review deals with the rise and fall of detente, the problem of nuclear weapons in world politics, how Soviet domestic politics in the 1980s is liable to affect Soviet foreign policy, and what the West might do to arrest its decline as a powerful actor in world politics. The latter two issues are dealt with briefly in the final chapter, and the reader cannot help but be disappointed that Ulam does not elaborate on the remedies to decline beyond noting the need for European integration and Western economic-political cooperation. The great strength of the book lies in its analysis of misperception in U.S. foreign policy regarding Soviet activities and objectives in world politics and its masterful depiction of the West, and particularly the U.S., as seen through Soviet eyes. One of the themes running throughout the book is the great fear the Soviets have of a nuclear China and the various means (almost always oblique) the Soviets have used in an attempt to get the United States to cooperate in the nuclear containment of China. For instance, Ulam relates, at the signing of SALT I in 1972 in Moscow, Brezhnev hinted that the USSR might help the United States extricate itself from Vietnam in return for U.S. cooperation in restricting the "nuclear aspirations of Beijing" (p. 75). In the SALT II agreement, the Soviets succeeded in inserting a provision that would have prohibited a Sino-U.S. strategic military relationship, and in the preliminary discussions on SALT III alluded to the China problem. Ulam states that the China problem contributed to the arms race: Concern with China "was one of the main reasons why the USSR accelerated its strategic as well as conventional arms build-up in the mid-sixties" (pp. 44, 50).

The main reason for the Soviet military build-up, though, even beyond distrust of a democratic (and hence unpredictable and erratic) U.S. and a revisionist China, is the inherently expansionist nature of Soviet foreign policy. In *Expansion and*

Coexistence Ulam related a minister's advice to Catherine the Great that sheds light on Russian foreign policy motivations: "That which stops growing begins to rot." During the Soviet period, and during the period examined by the book under review, the Soviet leadership could point to successes in foreign policy as legitimizing socialism, even though domestically socialism has not alleviated social, economic, and ethnic problems. Ulam stresses that during the 1970s Soviet expansionism achieved a new momentum which, once started, led to Soviet actions that destroyed U.S.-Soviet detente, even when a more restrained posture may have yielded similar results for the Soviets. Ulam warns that even though a younger generation is coming to power in the Soviet Union, the momentum of expansion will be hard to brake. He also points out that members of the younger generation, instead of being less expan-

sionist in foreign affairs, may see expansionist policies as natural, having spent their formative years witnessing the growing might of the USSR and the weakening of the West.

Ulam's analysis of the rise and fall of detente emerges as superbly crafted, well-written, and even humorous, based on examples of ludicrous Soviet propaganda and posturing and on American leaders' acceptance of ludicrous Soviet propaganda as reality. Ulam has an encyclopedic grasp of the events of the 1970s that makes his book seminal both to the student who is reading his or her first book on Soviet-U.S. relations and to the Sovietologist who will appreciate the broad overview of U.S.-Soviet relations.

JAMES P. NICHOL

Library of Congress

Normative Theory

The Rise and Fall of Structural Marxism: Althusser and His Influence. By Ted Benton. (New York: St. Martin's Press, 1984. Pp. xv + 259. \$27.95.)

In closing his vigorous interrogation of Louis Althusser, his followers, and his critics, Ted Benton writes, "Like many other socialists of my generation, my own ideas and political activities have been influenced over the past decade and a half by the ideas and arguments presented in this book, sometimes beneficially, sometimes in ways I now regret" (p. 227). This observation illustrates the need for Benton's accessible critique of "structural" Marxism and Benton's qualifications for executing it. Although Althusser and his influence have pervaded European debates in one way or another (in the case of Marxists to the point of being virtually second nature) American social theory has remained largely unaffected. Benton's "kind of settling of accounts with my own intellectual and political biography" (p. 227) enriches socialist praxis and can inform American social science.

Benton's contributions can be summarized by three adjectives: contextual, incisive, and engaged. First, Benton situates Althusser's "social practice" (p. 1) within the philosophical and political context that figures in Althusser's project and in "the vicissitudes of an intellectual tradition arising from it" (p. 1). That context, specifically Humanist Marxism and Stalinist Marxism, poses

the "decisive question" (p. 20) for socialists today: How to link "Marxist science" (p. 20) and human liberation? How, in other words, can socialists realize Marx's Eleventh Thesis on Feuerbach: "The philosophers have only *interpreted* the world in various ways; the point is to *change* it." Benton's formulation of the decisive question reveals why realizing this link—the source of Marxism's *elan vital*—may be its fatal flaw. He writes, "Fail to resolve [the link] at all and theory has no relevance to socialist practice ('theoreticism,' Althusser calls this). Resolve it through imposition via a coercive party or state apparatus and you have Stalinism" (p. 20).

In Part 1, Benton incisively examines Althusser's "third way" (p. 19), the "project of rigorous problematisation and reconstruction of some of the most basic concepts and propositions of historical materialism" (p. 111). Benton's assessment strikes a cogent balance, identifying problems with and achievements of Althusser's early work (chaps. 2-4) and Althusser's subsequent self-criticism and revision (chap. 5). In Part 2, Benton trenchantly scrutinizes the work of several people influenced by Althusser. Focusing upon "a small number of selected fields . . . of importance both for the viability of historical materialism in social science, and for social theory as such" (p. 112), Benton offers discerning treatments of debates among Claude Meillassoux, Emmanuel Terray, P. P. Rey, and others concerning pre-capitalist societies; the work of Barry Hindess and Paul

Hirst on transition and articulation of modes of production; and of Perry Anderson, Charles Bettelheim, and Juliet Mitchell (chap. 6). In chapter 7, Benton discusses Nicos Poulantzas' work "both in the field of state-theory and in the class analysis of contemporary capitalist formations" (p. 143).

Chapters 8 and 9 contain Benton's reexamination of Althusserian Marxism in light of *Les Nouveaux Philosophes*—"The Rebellion of Subjectivity" as he puts it—and criticisms leveled at Althusser's project by E. P. Thompson, Anthony Giddens, and others ("Crusaders and Sociologists").

Benton's contextual, incisive inquiries are thoroughly animated by his engagement with the issues raised by Althusserian Marxism and responses to it. Althusserian Marxism and its ramifications are among the most important theoretical undertakings of the late twentieth century. Even readers who will not be drawn to accept Benton's conclusions will be drawn into his "settling of accounts" (pp. vi and 227) with these significant social phenomena. Readers working within a Marxian framework will find Benton's book a searching intervention into perennial socialist concerns via his discriminating engagement with Althusser and Althusser's influence. Non-Marxist social scientists will find Benton's book a helpful means (the book provides valuable notes, references, and indexes, including an index of Structural Marxist terms) of becoming conversant with a school of thought, a set of issues, and an ongoing debate about which no student of contemporary capitalist society can afford to be uninformed.

JAMES C. FOSTER

Emory & Henry College

Liberalism Proper and Proper Liberalism. By Gottfried Dietze. (Baltimore: Johns Hopkins University Press, 1985. Pp. vii + 282. \$27.50.)

Dietze moves from the uncontroversial position that individual liberty is the essential emphasis of liberalism ("liberalism proper") to an exposition of "proper liberalism" as understood by these classical eighteenth-century liberals: Montesquieu, Adam Smith, Kant, and Jefferson. Utilizing his enormous knowledge of their writings and the commentaries on them, Dietze follows a similar format for each thinker. He shows that although liberty was a predominant value to each, they all thought the rule of law, a "moral minimum," must first constrain the liberty of individuals and then prevent the arbitrary and excessive authority of government. Each writer was

thus a "man of measure" committed to a middle ground between anarchy and despotism.

The basic shortcoming of this book is that Dietze does not successfully show why the thought of these four writers constitutes "proper" liberalism. He is struck by their basic agreement in spite of their different circumstances. They wrote during "a period of moralism," and although they represented no "liberal establishment," their writings were "more mature" than earlier liberals (p. 50). Yet, among eighteenth-century writers, Bentham was arguably much more influential intellectually than Jefferson. A discussion of utilitarianism would raise important issues about liberty but would also weaken the agreement that Dietze finds among the writers selected. Most importantly, why should "proper" liberalism be found by examining only eighteenth-century writers? It is not clear why they are more authoritative than John Stuart Mill or T. H. Green or John Dewey. Moreover, although there have been important recent attempts to specify and to defend liberal principles, Dietze does not refer to such thinkers as Rawls, Nozick, and Dworkin.

At the end of the book we gain insight into Dietze's choice of the four writers. They had great respect for property rights and for the restraints on government necessary to protect these rights. Such rights and restraints have, Dietze believes, been weakened as contemporary Western societies move toward both anarchy and despotism. The despotic tendency is toward "a government that considers the unequal distribution of property an anomaly and will regulate economic rights through social legislation"; the anarchist tendency is toward "license and problems of law and order" (p. 266). That we are already far along the path to anarchy is clear in the Warren Court's "expansion of civil rights in an expanding democracy," which led to "people's assuming they could take the law into their own hands and turn against law and order" (p. 265). The trend toward despotism is established by Hayek, who "shows how liberal democracy became replaced by social democracy and how under the banner of democracy proper liberalism was discarded and the road to serfdom taken" (p. 260). Dietze offers little argument, however, in defense of his perspective on political events.

Dietze often does not examine problems concerning the interpretation of the four writers. For example, he states that Kant thought "freedom grows with autolimitation made in the awareness of duty" (p. 137), but he offers little explanation or defense of Kant's position. Nor does he respond to the argument that Kant's ethical theory is too formalistic to offer specific guidance for behavior. Dietze also does not explore the dis-

pute as to whether Jefferson was caught within the individualistic "Lockean consensus" that Hartz thinks is dominant in American political thought or rather was committed to the ideal of virtue of classical republicanism, as Pocock argues. In conclusion, this largely untheoretical, highly expository book is most helpful as a source for statements on liberty and the rule of law by undoubtedly important eighteenth-century thinkers.

ROBERT B. THIGPEN

University of New Orleans

The Politics of Socialism: An Essay in Political Theory. By John Dunn. (New York: Cambridge University Press, 1984. Pp. xviii + 107. \$29.95, cloth; \$8.95, paper.)

This is a reader's joy: a brief but powerful book on an extensive and important topic whose scope is a reconsideration of the problem of socialist politics in developed nations, concentrating on why socialist politics remain attractive in such societies and whether in the future they can "still constitute a rational and civilising form of political enterprise" (p. xvi). Dunn's account is divided into three main sections that analyze what a successful socialist political theory would look like, how democratic socialism has in fact worked in practice, and whether a socialist politics is actually possible given the widespread opinion that practice has failed to fall even remotely short of the demands of theory. Dunn argues that, as a variety of political theory, socialism ought to explain reality satisfactorily, show us what forms of society are desirable, and give us some idea of how to realize in practice what we desire (while maintaining the better aspects of the societies we now inhabit, a salutary emphasis to which Dunn returns). His chief example in accounting for socialist success or failure is the 1983 defeat of the British Labour party, although his discussion of the debate accompanying this in Britain throws light upon many aspects of modern European politics.

Much of Dunn's first section is taken up with a discussion of socialist inabilities to speak clearly about political organization and about the assumptions and weaknesses of the Leninist model of the party, whose peacetime concentration of power and rigid conception of human nature and class representation are also attacked. Socialist politics nonetheless remain no less tentative and ambiguous than other forms of politics, and Dunn seems to imply that a liberal conception of party, with greater thrust placed in the participants and in the process of political education,

could readily be assimilated to socialist goals.

In Part 2, Dunn distinguishes between three forms of modern social democracy: the first assumes socialism to develop automatically from capitalism, a second virtually disregards the need for any fundamental economic change (e.g., the modern German SPD), and a third places greater emphasis upon political democracy as a means of asserting the superiority of socialism over capitalism. Although the broadest popular appeal has often lain with the third type, it is the question of socialist economic organization and reform that has most bedeviled past socialist political strategies and that continues to present the most pressing problems. Yet it is in the realm of politics that electorates are persuaded that alternatives are available and that reform and restructuring are not utopian myths, and here it is political and rhetorical skills, rather than economic programs, that will provide the basis for any socialist future. Here the question is not whether more efficient forms of socialist economic organization might exist where "culture rules economics" (p. 69), but the fact that this is still widely regarded as implausible.

To persuade most that it is not, a goal so necessary to the securing of the trust of the working classes which Dunn sees as the main goal of socialist politics, it is necessary for socialists to reconsider how society might actually be improved, a far more difficult task than merely criticizing its present inadequacies. The complete socialist conception of the good, the utopian ideal that Dunn calls "true socialism," has essentially been lost; indeed, it is unclear that any institution might adequately realize it. This is not to insist that socialism must inevitably be tyrannical if its goals are even to be attempted properly, but it is massively to discredit the notion of socialism in which intellectuals impose their views upon society *carte blanche*, as Pol Pot most recently attempted. Correspondingly, too, it is to recommend socialist attention to nineteenth-century liberal emphases upon constitutional balances, the rule of law, political accountability, and the like. Without reverting to such mechanisms, and providing a far more compelling description of the advantages of economic planning, socialism does not have a future to which many will want voluntarily to subscribe. These, as Dunn puts it, "are not inspiring conclusions" (p. 93), but they demand our attention insofar as socialism is not likely to cease being seen as an alternative to capitalism for some time to come.

This is a very wise and judiciously reflective book on a topic of momentous importance. It repays reading and rereading, not only by students of socialist politics, but also by all observers of twentieth-century political develop-

ment. In a large and too often dogmatic or pedantic literature, no other account as soundly reasoned and penetratingly observant is available.

GREGORY CLAEYS

University of Hanover (West Germany)

Hegel, Heidegger, and the Ground of History. By Michael Allen Gillespie. (Chicago: University of Chicago Press, 1984. Pp. xv + 217. \$22.00.)

This thoughtful and stimulating work boldly takes on the task of assessing the thought of both Hegel and Heidegger. Gillespie seeks to explain how these two philosophers have tried to understand what history means when taken as a whole, and what significance history has for illuminating our essential characteristics, goals, and limits. Is history coherent, possessing some intelligible design, order, or goal? Or is it, as much human experience seems to attest, a story significant in parts but overall riddled with folly, slaughter, and caprice? If the former, then maybe history can be the source of answers to our most fundamental questions about ourselves and about society. If the latter, then we must seek an understanding of the fundamental questions by looking more to our ideas of nature, religion, or science rather than to the ups and downs of political events. Much recent thought holds that history is indeed more important as a guide for thought than nature, religion, or science. Gillespie wants to examine the deepest attempts to elucidate this idea of the importance of history, and so he rightly turns to those philosophers who explicitly face it.

Gillespie's book provides both a comprehensive overview of the political and philosophic orientation of Hegel and Heidegger and then also a more specific treatment of their attempt to fathom whether there is a "ground of history," whether it is based in something intelligible and coherent. Gillespie's account of the general outlines of the thought of Hegel and Heidegger is a marvel of clarity. The comments on Hegel (chaps. 2-4) seem particularly penetrating. Noteworthy is the vivid explanation of Hegel's political views (pp. 33-47, 85-103) and of Hegel's response to the problems suggested by Kant's work, and also Gillespie's rather detailed commentary on the "Introduction" to the *Phenomenology* (pp. 63-84). Almost equally acute and illuminating is the single lengthy chapter devoted to Heidegger, which contains a lucid account of Heidegger's reading of the history of thought. Gillespie's method throughout is to explain Hegel and Heidegger from within their

own set of terms and questions. The book proceeds with no concessions at all to Anglo-American positivism; it does not acknowledge the distance that seems to prevail between the issues of concern to Hegel and Heidegger and the ones frequently uppermost in contemporary American philosophy or political science, and it does not attempt to bridge this gap.

Two observations might be made about the general story told in this book. First, it seems that we have today a better grasp of the origins and import of Hegel's work than of Heidegger's, and that scholarship handles his work more securely (which does not necessarily mean a complete understanding is readily available). Second, Heidegger's ideas seem somehow closer to our experience today than those of Hegel, even if strangely remote from politics; Heidegger emerges from Gillespie's comparison as the more compelling figure, although by no means wholly attractive.

Gillespie's special theme is the issue of whether there is an intelligible ground of history. "Ground" seems to mean a fundamental unity (pp. 55, 95); something is grounded when it is unified or whole in an intelligible way. Heidegger argues that it is Plato who first pursued the issue of a ground and made it "the central question of metaphysics and the West" (p. 142). In Gillespie's view, Hegel thought there was a "ground to history," and that history must be understood to be rationally ordered. After presenting the arguments very thoroughly, Gillespie suggests that Hegel's view is questionable (pp. xiv, 113-115), either because it is naively optimistic in its rationalism or, under the surface, darkly fatalistic. His remarks suggest that Heidegger is closer to stating in philosophically persuasive form what is found in the modern experience, when optimism, historicist progressivism, and rationalism seem all in decay. But from Heidegger we learn that there is no "ontological ground" for history or for anything else, indeed that the whole issue is an error, because the search for an absolute ground obscures an ostensibly more primordial, fertile insight into Being. This recognition, according to Heidegger, also helps to free us from the tyranny of technological ways of thinking, among which he includes the opinion that historical studies are somehow a key to interpreting what we are. Gillespie notes the strengths of Heidegger's view, while rightly calling attention to the almost inhuman distance from the "everyday realm of inauthentic life" that Heidegger establishes and that has much to do with his "woefully weak grounds" for teaching us how to deal with "the capricious and often demagogic claims of false prophets" (pp. 170-171). Gillespie also rightly ends this study by noting that the great questions

are illuminated but not solved by Hegel and Heidegger and remain for us to address.

DONALD J. MALETZ

The University of Oklahoma

Hayek on Liberty. By John Gray. (New York: Basil Blackwell, 1984. Pp. x + 230. \$29.95.)

This book offers a thorough explication of one of this century's most prominent classical liberals. Its basic theme is that Hayek's various specialized contributions to legal, social, and economic theory have emerged from a coherent system of thought, a specific philosophical outlook.

What is the system of ideas that characterizes Hayek's thought? Gray at first labels it a "sceptical Kantianism" that explores the limits of reason through immanent criticism. However, most of the discussion focuses on other influences on Hayek's views. Prominent among these influences are Popper, Wittgenstein, J. S. Mill, and especially, Hume. Indeed, by the time we reach the end of the book references to Kant are all but forgotten, and Hayek's contribution is to have returned "thought about man and society to the great tradition of the Scottish Enlightenment. . ." (p. 140).

Several concepts are found to be central to Hayek's system of ideas. One of Hayek's basic beliefs is that social order is spontaneous. It is not the product of conscious control or rational design; it is instead a fortuitous consequence of human action. Allied with the concept of spontaneous order is an epistemology that views most of knowledge as tacit or practical in character, and some of it as inarticulable. Attempts at social planning (based as they are on abstract, theoretical knowledge) will thus be limited in success, if not entirely self-defeating, for they run counter to the wisdom embodied in our practices.

For Hayek, then, social institutions and rules of conduct are the vehicles and repositories of human knowledge. Complexes of practices and rules (i.e., traditions) are in a competition that filters out error and brings us closer to an approximation of truth. Hayek ultimately evaluates social orders by means of an indirect utilitarianism that applies the principle of utility "not to specific practical questions, but to whole systems of rules or codes of conduct" (p. 96). Such applications of utility have led Hayek to seek the economic and juridical frameworks necessary for ensuring the growth (and to some extent, the prevention) of decentralized, practical knowledge.

Gray's book offers us a valuable comprehensive look at the philosophical basis of Hayek's social thought. It offers relatively detailed illustrations

of how Hayek's views on law, morality, and economic theory are related to a well-grounded philosophical system. Gray's concluding chapters also provide interesting comparisons between Hayek and other social thinkers, as well as an extended discussion of some common criticisms of Hayek's positions. And, not least among the book's accomplishments, Gray gives us a useful, extensive bibliography of works by and about Hayek.

However, there are important defects in what is otherwise an excellent synoptic glance at Hayek's thought. First, Gray shows little awareness that theorists other than classical liberals have also held organic and/or evolutionary views. Diverse thinkers as Comte, Hegel, Marx, and Toulmin have all had such perspectives but have derived substantially different conclusions regarding preferred social orders or the growth of knowledge. As it is, Gray explores only those theoretical heavyweights who support the classical liberal perspective, which results in a sanguine appraisal of Hayek's political and economic recommendations.

The book also suffers from apparent incompleteness. For example, political theorists seeking Hayek's views on the preferred political system will find only a handful of pages devoted to one of Hayek's chief recommendations for guaranteeing liberty, namely, bicameralism. But more importantly, Gray provides little sense of what is to be gained from viewing Hayek's thought as a "synthesis of Humean indirect utilitarian argumentation with a Kantian test of universalizability" (p. 96). Indeed, the only payoff discussed is a paradigm shift that abandons the dead ends of conceptual analysis and social choice theory in favor of Hayek's perspective on spontaneous social order and evolutionary epistemology. In that case, however, other currents of contemporary thought have crossed the finish line ahead of Gray's interpretation of Hayek.

LEONARD WILLIAMS

Manchester College

Prophets of the Left: American Socialist Thought in the Twentieth Century. By Robert Hyfler. (Westport, Conn.: Greenwood Press, 1984. Pp. x + 187. \$29.95.)

Robert Hyfler has written a tightly argued, at times eloquent, study of American socialist theorists. His book is not intended as a comprehensive history of twentieth-century American socialism or even of all significant socialist thinkers in this century. He somewhat arbitrarily selects a small group of thinkers who espoused different versions

of a socialist vision while working for realizable goals. Hyfler pays some attention to the biographical details of his "prophets'" lives and political activities. His main interests, however, are their theoretical contributions and the contribution of their theories to the failure of American socialism.

Hyfler does not, of course, blame that failure entirely on theoretical shortcomings. "Wilsonian repression" (p. 90) helped destroy the promising leadership of Eugene Debs. When antiradical forces again grew strong after World War II, they greatly weakened the organized Left and cut off potential new socialists from their theoretical heritage. Thus, even the student Left of the 1960s could not readily build on or react to the theories of earlier American leftists.

Hyfler's most interesting argument is a complex analysis of the links between labor activism and socialist theories. He contends that the typical view of Samuel Gompers as the conservative leader of America's nonsocialist labor unions misses an important element of his role. Gompers urged workers to seek economic gains through their unions rather than political gains through a socialist party. However, Gompers's opposition to a politically oriented labor movement often reflected his belief that government served the interests of capitalists more than it reflected anti-socialism per se. Hyfler further contends that Gompers and other "working-class accommodationists" emphasized "the self-liberation of the working class" (p. 59), even while they believed that workers were not strong enough to control the state.

Hyfler argues that those socialists who shared Gompers's belief in conservative tactics held a sharply different theoretical perspective. For example, Morris Hillquit, a founder of the United Hebrew Trades and leader of the Socialist party, favored tactics as moderate as those of Gompers. Yet the elitist and statist nature of his socialism distanced his theories from Gompers's beliefs. Hillquit believed that socialist leaders must dominate the working class, which could not be expected to liberate itself. Moreover, he believed that socialists could peacefully influence or capture the state and use it in the interest of workers.

Other socialists had theoretical beliefs that partially paralleled Gompers's, but they did not support his conservative tactics. Debs did not reject political activism but shared Gompers's suspicions concerning the state in a capitalist society. He also believed that the working class had a great potential for self-liberation. Yet Debs thought this potential should be used to develop socialist class consciousness. He believed that Gompers's American Federation of Labor was impeding the formation of class consciousness because it was

organized along narrow craft lines. Debs favored industrial unions that would unite workers organizationally and help them see their common interests.

Hyfler gives briefer treatment to more recent developments in socialist thought. He does note the schisms that emerged on the Left after the initially broad approval of the Bolshevik Revolution gave way to disillusion on the part of many socialists. Hyfler also devotes a short chapter to Norman Thomas that minimizes his theoretical significance. He concludes by questioning whether Michael Harrington's democratic socialism is sufficiently socialist. This last argument is not entirely convincing, but Hyfler's generally perceptive analysis of his prophets deserves attention.

SANFORD N. GREENBERG

Austin, Texas

The Islamic Conception of Justice. By Majid Khadduri. (Baltimore: Johns Hopkins University Press, 1984. Pp. xvi + 256. \$25.00.)

This latest of many works by Khadduri is an attempt to "inquire into the nature and scope of Divine Justice and to reconstruct from the diverse writings of Muslim men of learning the Islamic theory of justice in all its aspects" (p. 11). In so doing, he has provided us with a study of great breadth as well as depth; not only is justice a central concern of Islamic thought, but it also has been inextricably linked with other persisting issues. It would thus be difficult to identify many major Islamic thinkers—particularly from the medieval period—that have escaped Khadduri's attention.

One unique facet of this study is the author's classification of the aspects of justice dealt with by Islamic thinkers. He begins with "political justice," which relates to the question of legitimate succession that split the Islamic world into sects and also to the doctrines of free will and of the right to accuse proclaimed Muslims of being unbelievers, both of which were rejected by supporters of impious Umayyad rulers.

Under the rubric of "theological justice," Khadduri proceeds to the more elaborate later debate between the Mu'tazilites and their opponents over free will and determinism and over reason and revelation, as well as to the positions of various sects and of Sufi writers on these issues. Then he focuses on "philosophical justice," defined as the effort to understand "justice as presented in the works of Greek philosophers and to make it intelligible to believers" (p. 78)—particularly as demonstrated by four great thinkers,

al-Kindi, al-Farabi, Ibn Sina, and Ibn Rushd. Another chapter deals with a rich body of literature on "ethical justice," involving standards of conduct that go beyond the minimal requirements of the law and that are characterized by an effort "to harmonize Islamic with foreign notions and values" (p. 106). Under the heading "legal justice," the underlying principles of both substantive and procedural justice are identified, while a chapter on "justice among nations" is devoted to the medieval theory of an Islamic world that is in a state of war with the rest of the world and particularly to an equation of the concept of "*jihad*" with the Western tradition of "just war." A final category is "social justice," which is said only sometimes to include distributive justice and the essence of which is identified with promotion of the "public interest" by improving social conditions and consequently the health of the state—an idea whose fullest development is credited to Ibn Khaldun.

Most of the book deals with the medieval period. Although Khadduri emphasizes the continuity of basic questions, he somewhat incongruously relegates treatment of the modern era to the last chapter. Stress is put on conflict between Revivalist and Modernist schools of thought and on the alternation of "complementary" trends, with "so-called Islamic upsurges" not necessarily opposing innovation but providing "a breathing space for society to assimilate" change (p. 229).

Only at the risk of being considered picky can one single out even minor substantive flaws in this admirable work. As cases in point, Khadduri's attribution of the difficulty of "adjustment to secularism" to Islam's being a young religion (p. 196) is hardly satisfactory, and "separation of church and state" (p. 202) is an imprecise description of what Atatürk carried out in Turkey. Also, Khadduri contradicts himself with regard to whether Khumayni rises above "denominational differences" (pp. 226-227). Both the high quality of this volume and the absence of any other comparable broad studies ensure its long-lasting value to students of the Islamic world and to those in many disciplines who are concerned with conceptions of justice.

GLENN E. PERRY

Indiana State University

The Radical Politics of Thomas Jefferson: A Revisionist View. By Richard K. Matthews. (Lawrence: University of Kansas Press, 1984. Pp. ix + 171. \$22.50.)

Because Jefferson expressed his thoughts over a very lengthy course of time and in a body of writ-

ing that is as unsystematic as it is voluminous, there is both license and evidence to read back into the expression of his ideas any tradition that one might want to. Most of the time, Jefferson is depicted as one kind of a Lockean liberal or another. Richard K. Matthews wishes to revise this estimate by showing how Jefferson's thinking is indicative of an altogether different tradition. What results from his effort is a decidedly "New Left" Jefferson. Others have argued that Jefferson is a far more radical political thinker than the conventional literature on him, or the dominant culture's image of him, acknowledges. But no one, to my knowledge, has searched out this alternative Jefferson in order to state his own quarrel with liberalism with the care and forcefulness that Matthews shows in this well-written book.

To wrest Jefferson from Locke, Matthews reads the former with the assistance of Gary Wills and J. G. A. Pocock. He also makes some very interesting uses of Hannah Arendt's views on Jefferson's ward republic and of Leo Marx's "middle landscape" concept from Marx's classic study of technology and the pastoral idea in America. However of all the guides to whom Matthews acknowledges indebtedness, the most influential is C. B. MacPherson.

Carefully examining familiar bits and pieces of Jefferson's writing from the diverse angles of perspective that he borrows from those cited previously, Matthews uncovers what he regards as the central elements of Jefferson's political thought: civic-humanism, communal anarchism, and radical democracy. Civic-humanism is based upon what is shown to be Jefferson's republican critique of market society relations and his preference for that "middle landscape" in which one could be antimarket without being antimodern. Communal anarchy takes form from the anthropological lessons Jefferson learned from firsthand study of his own Indian neighbors. These lessons regarding man's naturally social constitution, combined with Jefferson's unflaggingly optimistic appreciation of the unique import of New World conditions, convinced him of the possibility of living out from under the shadow of the leviathan without reverting to primitivism. Finally, accepting Jefferson's insistence that the earth really does belong to the living, Matthews demonstrates certain affinities between Jefferson's thinking and more recent concepts and expressions of participatory democracy and permanent revolution.

The argument regarding each of these chief elements in Jefferson's political thinking remains close to the text while deftly drawing on and melding the insights of others. Each of these arguments is closely articulated with the other two. Along the way, Matthews makes some very nice distinctions

between Jefferson's thinking on the one hand and that of Rousseau, Paine, Madison, and Hamilton on the other. Madison is shown to be closer to Hamilton than Jefferson, and Jefferson, in turn, to the left of Paine.

It will take a far longer book to convince some that Jefferson belongs to and shaped that tradition of politics from which the New Left springs. However, after reading this book, no one will dispute the fact that Matthews demonstrates the value of Jefferson for those who might wish to fashion an American alternative to Lockean liberalism.

DARRYL BASKIN

Elmira College

Fortune Is a Woman: Gender and Politics in the Thought of Niccolò Machiavelli. By Hanna Fenichel Pitkin. (Berkeley: University of California Press, 1984. Pp. ix + 345. \$24.95.)

In this multifaceted book Hanna Pitkin offers us a fundamentally new reading of Machiavelli. Through primarily a close and often brilliant exegesis of Machiavelli's writings (including his plays, poetry, and correspondence), she offers us an account focused on the gendered nature of Machiavelli's central images and on the problems that these images create in his treatment of the issue of human autonomy.

To complement her outstanding skills in exegesis, Pitkin brings two other methods to bear on her interpretation: (1) a cautious application to Machiavelli's work and times of lessons to be learned from selected psychoanalytic literatures; and (2) an examination of the relevant research concerning sexual relations, marriage, and child-rearing in Renaissance Florence. In addition, she closes the book with a powerful, extended meditation on the present relevance of some of Machiavelli's insights, a meditation on what she sees as the tasks and themes of contemporary political theory.

In the Introduction, Pitkin identifies the problem of autonomy as Machiavelli's "central preoccupation" (p. 7). It is also her own main preoccupation, as becomes very clear in her concluding "Meditations on Machiavelli." What Machiavelli "at his best," as she puts it, is able to teach us about politics is the need for an autonomy "that acknowledges our necessary interdependencies" (p. 324), an autonomy that involves the development of our "capacity for mutuality within difference" (p. 301). However, for Machiavelli autonomy is all too often "intertwined" with manhood—and hereby hangs the tale. If autonomy is synonymous with manhood,

that which threatens autonomy comes to be identified as female. But the female cannot be expunged from the human condition, nor can it be expunged from the individual male psyche. To define autonomy against the female thus becomes self-defeating. The outcome of what Pitkin calls the "misogynistic" definition of autonomy (p. 236) is the creation of strong father images and military discipline—that is to say the destruction of the community of autonomous and mutual citizens advocated by Machiavelli "at his best."

Machiavelli's conflicting images of manhood include the "Fox," the "Founder," and the "Citizen" (all traced in Part 2). It is the interplay of the last two that provides Pitkin's focal point. Founder images are misogynist, whereas Citizen images could be interpreted without gender bias. The Founder, or restorer, is an "armed prophet." Generally a motherless foundling, he brings about manly *virtù* in the citizens by his ruthless but necessary cruelty, which alone can defeat the female forces of corruption within and without. But in so doing he "destroys his sons" (as Brutus literally did), reducing them to a passive obedience that is the opposite of autonomy. By contrast, the Citizen image of manhood implies "peer mutuality" and "fraternal rather than paternal and filial" relations (p. 90). But it seems impossible that the citizens of Republican *virtù* that Machiavelli calls for could emerge from the actions of the Founder.

The all-powerful female forces that must be resisted are examined in Part 3: women as sexual corrupters of men, sowers of discord, malicious shrews—the female personification of Ambition and Avarice as well as of Fortune herself. Pitkin goes on (Part 4) to point out that such images were pervasive at the time. Evidence from historians suggests that for Florentine women life was becoming increasingly confined to the home, and psychoanalytic theories suggest that increasingly intense mother-child relations probably resulted, leading to heightened male ambivalence towards women. Returning to Machiavelli's texts, Pitkin then shows us the operation of this ambivalence in Machiavelli's accounts of the "birth" and "rebirth" of Rome and Florence—accounts of *men* giving birth to cities, accounts in which autonomy is conceived as *male* autonomy, "won from and sustained against female power" (p. 230).

Pitkin's final "meditation" (Part 5) attempts to rescue Machiavelli from himself: There is a vision of universal autonomy and mutuality in the Citizen image, even though Machiavelli never manages to sustain it. It is upon this image that Pitkin claims we must build. Machiavelli originates a meditation on the relations of human autonomy to action, to membership in political

communities, and to principles of judgement. In Pitkin's reading, Machiavelli becomes an anticipator of Kant (and, it seems to me, more generally of the liberal tradition), identifying autonomy with choosing one's own principles of action (p. 315) and advocating the recognition and resolution of conflict within mutually agreed limits.

There is plenty to disagree with in this book, particularly in Pitkin's own vision of politics in the final "meditation." Equality, for example, is not, I think, adequately addressed within the concepts of membership and mutuality. Machiavelli scholars will doubtless find fault with Pitkin's interpretation of various passages. But this is a book of such power and breadth that it will not be diminished by such criticisms. It is also a book that stands as an important indicator of the profound impact that feminism has now had on the writing of political theory. It demonstrates that by using new lenses developed by feminist scholars, by focusing on themes that would once have seemed irrelevant or subordinate to more explicitly "political" matters, we can in fact profoundly and fruitfully modify our vision of the political theory of a major figure in the Western tradition.

SONIA KRUKS

New School for Social Research

Locke's Education for Liberty. By Nathan Tarcov. (Chicago: University of Chicago Press, 1984. Pp. viii + 272. \$22.00.)

"Politics," John Locke wrote in 1703, "contains two parts very different the one from the other, the one containing the original of societies and the rise and extent of political power, the other, the art of governing men in society." Without acknowledging authorship, Locke went on to recommend his *Two Treatises of Government* as one of several books on the first part of politics. As to the part "which concerns the art of government," he said, "that I think is best to be learned by experience and history. . ." (*The Educational Writings of John Locke*, ed. James Axtell, Cambridge University Press, 1968, p. 400).

Nathan Tarcov's argument is that Locke offers more guidance on "the art of governing men in society" than the bare admonition to look to experience and history. According to Tarcov, Locke's educational writings, especially *Some Thoughts Concerning Education*, supplement the *Two Treatises* by supplying insight into Locke's views on this subject. "Although one must be very careful in this regard," Tarcov says with characteristic caution, "one can conclude that Locke's account of the art of governing children aids in the task of understanding what he might

have written on the second part of politics" (p. 7).

Before he turns to the *Thoughts Concerning Education*, though, Tarcov analyzes at length (65 pages) and in detail (407 notes) Locke's defense of the natural freedom of mankind against Filmer's patriarchal arguments. Indeed, Tarcov directs his scrutiny not only to Locke's *First Treatise* and Filmer's writings, but also to other writers in whom Filmer detected the notion that men are born free from all subjection—Bellarmine, Calvin, Buchanan, Barclay, and Hobbes among them—in order to clarify the meaning of the doctrine of natural freedom. This circuitous route leads to the conclusion that Locke sharply distinguishes between the proper roles of civil government and the family. As Tarcov puts it, "Education, which Locke tells us in the *Essay* and the *Thoughts* is more responsible for making men what they are and even exercises more influence over their conduct than do civil laws, he entrusts exclusively to the family, the authority that lacks the power of life and death. One is tempted to say that that is the fundamental separation of powers" (p. 72). The task of government, then, is to preserve life, liberty, and property, while the family acts as "the agency of Locke's education for liberty" (p. 76).

In what is this education for liberty to consist? To answer this question, Tarcov devotes the remainder of his book to a close reading of the *Thoughts*. There he discovers a Locke who proposes to use the innate desire to be free to instill in the gentry the virtues of self-denial, civility, and liberality. Locke is a moralist, it seems, one who provides both a "complex account of how to build concern for the life, liberty, and property of others on a solid basis in human nature" and a "subtle account of the nature of the human attachment to liberty and how to nurture and direct it" (p. 183). He is, moreover, a moralist whose morals may well be pertinent today. "His emphasis on the harsh virtues of self-denial, courage, hardiness, and industry may offend our easy-going self-gratification," Tarcov writes in his conclusion, "but these virtues may still be necessary to the individual liberty and comfort that we join with him in valuing" (p. 210).

There is much to admire in *Locke's Education for Liberty*. Tarcov's command of the relevant literature is impressive, his reading of Locke is both sensitive and sensible, and his views are expressed clearly throughout the book. My only complaint is that Tarcov attends so assiduously to Locke's texts that the reader loses sight of the fact that Locke lived and wrote, for the most part, in seventeenth-century England and Holland. One can turn to James Axtell's fine edition of the *Thoughts Concerning Education*, however, to gain an appreciation of the context. For an appre-

ciation of the moral and political implications of Locke's writings on education and the family, and of what they reveal about his views on "the art of governing men in society," we can ask for no better guide than Nathan Tarcov.

RICHARD DAGGER

Arizona State University

The Political Thought of Martin Luther. By W. D. J. Cargill Thompson. Edited by Philip Broadhead. (Totowa, N.J.: Barnes & Noble Books, 1984. Pp. xv + 187. \$26.50.)

This book, an edited version of an almost complete manuscript found among the author's papers after his death, is the work of a first-rate scholar. Cargill Thompson was a distinguished ecclesiastical historian, and his extensive knowledge of the reformation's theological and political controversies allowed him to interpret Luther's always polemical "tracts" in their proper context. More than that, he knew how and took the time to read Luther carefully, to follow his arguments, and take them seriously.

For Cargill Thompson, Luther's thought is worth understanding because he was the Christian extremist *par excellence*. Luther viewed himself as an Augustinian, radically distinguishing between spiritual and temporal, faith and reason, Church and politics, voluntary and coercive, word and sword, inward peace and external peace, and so forth. The idea of the two realms of human existence or "the two Kingdoms, with all of its ramifications, is the key to Luther's mature social and political thought" (p. 42). Luther's articulation of this idea was considerably more clear, complex, and concrete than that of Augustine. For Luther, this fuller articulation was necessary to oppose the medieval, Thomistic propensities to synthesize and order hierarchically, to show why true Christianity stands radically in opposition to the comprehensive rationalism of Aristotelian ethics and metaphysics.

Luther always claimed to have deduced his political thought from his theology. This constancy gave his diverse political opinions a certain inner consistency, but this consistency is perhaps only clear from Luther's own perspective. His political opinions did change markedly over time.

Luther's early thought is based on a simple and powerful faith in the word as *the* effective means for the reformation of human affairs. Political compulsion is useless. It only produces the hypocrisy of outward conformity without genuine reformation or change of heart. Consequently, the early Luther was a partisan of complete religious toleration.

He held that the political realm was charged with only the production of external peace. It has no concern with the soul. As a result, Luther taught that "secular princes *qua* princes had no authority in religious matters and ought not to impose faith on their subjects or to legislate in religious matters" (p. 56). But, following a powerful medieval opinion, Luther also believed that the Ten Commandments were all in accord with reason or natural law. It is clear they include a prohibition against blasphemy, the public expression of false belief. Political experience seems to have made it clear to Luther, in spite of his theoretical extremism, that the word's effectiveness might be indebted to political power. He gradually expanded his definition of blasphemy. Eventually, "the dividing line between public blasphemy and private error had become exceedingly narrow and Luther had, in effect, completely abandoned the remarkable 'liberalism' of his original position" (p. 162).

Students of political philosophy will wish that Cargill Thompson had said more about the contribution of Luther's thought to the development of modern liberalism. I find that what he says about Luther is extremely suggestive in this regard. I note, for example, that Locke agrees with Luther that reason or natural law is limited to understanding what is temporal or "external." Consequently, Locke teaches that political rulers have no competence in religious matters. He goes on to reject Luther's medieval prejudice in favor of the reasonableness of all the Biblical commandments. Locke, with Luther, concludes that Christian extremism properly understood is the doctrine most favorable to the understanding and achievement of true liberty.

As the founder "of a distinctly protestant political theory," Cargill Thompson says, "Luther's influence on man's attitudes toward the state" in some ways was "far greater than that of Machiavelli" (pp. 6-7). For this reason, both Luther's theory and Cargill Thompson's book deserve our serious attention.

PETER AUGUSTINE LAWLER

Berry College

The Nursing Father: Moses as a Political Leader. By Aaron Wildavsky. (University: University of Alabama Press, 1984. Pp. xi + 262. \$25.00.)

The title of this remarkable book is taken from the eleventh chapter of the Book of Numbers where Moses complains that he must carry the Lord's people "as a nursing father beareth the sucking child." Wildavsky uses the image of

Moses as nursing father to present an analysis of political leadership based on the Torah. The book has two theses: "first, that understanding of the Mosaic Bible may be enhanced by treating it as a teaching on political leadership; second, that our understanding of leadership may be improved by considering it as an integral part of different political regimes" (p. 1).

An introductory chapter explains the book's plan and enables Wildavsky to position himself with regard to both the social science literature on leadership and contemporary biblical scholarship. The next five chapters pursue the theme of leadership as a function of regime. Wildavsky discerns four distinct regimes for the Israelites during Moses's lifetime: the regime of slavery under Pharaoh, the regime of anarchy once they are out of Egypt, the regime of equity in response to the idolatrous worship of the Golden Calf, and the regime of hierarchy after Korah's rebellion. Each regime is presented as a model with its own salient characteristics. For example, in the regime of anarchy, the religious principle is "different gods for different purposes" and leadership is "meteoric"; in the regime of hierarchy, leadership is autocratic and religion monotheistic. Wildavsky shows how Moses leads in a different way in each of these regimes.

A concluding chapter critically reviews leadership literature and finds that "there is almost no discussion of types of political systems as productive of different kinds of leadership" (p. 190). Wildavsky deplores this neglect and argues that biblical studies will redeem leadership literature from its erring ways. The book closes with an appendix that speculates on how Judaism survives and finds the answer in a judicious blending of the characteristics of the regimes of equity and hierarchy.

As a study of leadership, *The Nursing Father* is a useful addition to the literature, but it falls far short of Wildavsky's ambitious goal to "supersede [leadership literature] with a perspective in which leadership becomes a part of regimes" (p. 26). As social science, this book will interest and inform the reader; but no more.

A quite different judgment is in order on *The Nursing Father* as an effort to enhance our understanding of the Pentateuch. Here the book will charm, delight, and challenge the reader. Wildavsky's warmth, wit and obvious affection for his topic attest to the vitality of the biblical tradition and its enduring capacity to address serious issues of any day, including our own. The book's dazzling success in offering a political interpretation of the Torah may cause the reader to forget whatever he might have learned about leadership. It is a small price to pay. The strength of this book lies in Wildavsky's profound political meditation on

the biblical heritage. He does not teach the reader about the Bible, but how to learn from it. *The Nursing Father* is an eloquent bulwark against the tendency to collapse the rich relationship between religion and politics into trivial quibbles over church and state. Wildavsky's robust treatment of religion and politics provides hearty fare to replace the watery gruel of shallow legalism.

JOHN A. ROHR

Virginia Polytechnic Institute and State University

Kant's Political Philosophy. By Howard Williams. (New York: St. Martin's Press, 1983. Pp. xii + 292. \$30.00.)

Howard Williams has two objectives in this book: "to provide an introduction to Kant's political philosophy," and to provide a "picture of Kant's political theory as a whole, [showing how Kant's] views on politics square with the major tenets of his critical philosophy" (p. vii). Williams accomplishes the first objective with great success, but his approach to the second goal seems less convincing, particularly in light of recent Kant scholarship.

Williams provides a good overview of the corpus of Kant's political writings. His discussions of Kant's views of property, women, and punishment are especially interesting. Williams is successful in demonstrating the link between Kant's ethics and his politics by way of his discussion of Kant's philosophy of history. Williams shows that the moral lesson in Kant's philosophical view of history is best understood when we look at man's relationship to history and nature as if this relationship were the ground for man's realization of the moral objectives of practical reason, the hope of a society where persons can be treated as ends and not solely as means. This relationship is characterized by man's rejection of nature's domination (or the domination of impulse) as he asserts his desire for self-discipline and for the deliberative qualities that permit him to create circumstances (republican government and the hope of an international order dedicated to peace) compatible with the goals of morality. "By seeing history as embodying a purpose the ethical person can enter the seemingly amoral quagmire of politics, and take advantage of its arbitrariness and unpredictability, to bring about a moral end" (p. 22).

But as Williams demonstrates, Kant paints a picture of citizens who are not motivated by the objectives of Kant's ethical philosophy, and yet Kant's politics is still committed to realizing these objectives. The question one could ask is how a

politics that seeks ethical goals is possible in a setting where persons do not make ethical motives central to their lives?

Williams would reject as an answer to this question a view that makes Kant's philosophy of history historicist (p. 263). Williams understands Kantian citizens as holders of interests, and he shows how Kant's politics are formed in response to them. On this view, citizens, not history, shape politics. Williams's account centers on the nature of the interests that citizens hold. For Williams, Kantian citizens act and think as "egoistic commodity owners" (p. 194). Kant is said to be the proponent of *laissez-faire* liberalism (p. 194), where the state secures the right of a person to the "single-minded pursuit of his own self-interest" (p. 194).

Still, Williams's account is balanced, and he demonstrates that, for Kant, the right to pursue individual interests is sanctioned by the society and thereby this right always implies an obligation to certain social rules. Thus Williams correctly points out that Kant "realizes . . . that the individualist pursuit of liberty and equality has to be tempered by social restraints if it is not to lead to the destruction of civil society itself" (p. 194). The state requires that citizens respect certain constraints to guarantee equal liberty. At the same time, the state itself is organized to be a "balanced mechanism" that can insure citizen compliance with laws beneficial to the society as a whole because it (the state) is made subject to various

limitations as well (p. 195). These limitations prevent the state from becoming a force that seeks to aggrandize power at the expense of citizen liberty.

But where citizens are motivated by non-moral concerns, would the state structure be sufficient to protect the liberty of all citizens? Patrick Riley (*Kant's Political Philosophy*, Rowan & Littlefield, 1983, pp. 9, 12, 14) argues that for Kant, as citizens act within the law of republican states even from motives of self-interest, they nonetheless manifest a commitment to support laws that have as their objective a concern for respecting the freedom of others. On Williams's view the basis for attaining the objectives of morality is the inherent optimism central to Kant's thought. "Finally, the success of Kant's whole political philosophy rests on the hope and moral presupposition that the human race will improve ethically" (p. 278). Still, Riley's view is more compelling because, unlike Williams, Riley does not make Kant's case for a politics that moves toward moral goals wholly dependent on Kant's feelings of optimism for the future. Instead, Riley demonstrates Kant's reasons for this optimism; namely, that in republican states persons act as moral beings even if for non-moral motives. Here a moral politics is thinkable even without the ethical advance of the human race.

STEVEN M. DELUE

Miami University of Ohio

Empirical Theory and Methodology

From Knowledge to Wisdom: A Revolution in the Aims and Methods of Science. By Nicholas Maxwell. (New York: Basil Blackwell, 1984. Pp. vi + 298. \$34.95.)

A work concerned with a revolution in the aims and methods of science requires an author who possesses a command of a number of disciplines. On the basis of this criterion, Nicholas Maxwell—a lecturer in the history and philosophy of science at University College, London, who also publishes in journals of physics and philosophy—is up to the task. I have presented his academic credentials because few political scientists are likely to be familiar with Maxwell's work and to point to a paradox in the final chapter, "The Revolution Is Under Way," of this call to action. This paradox is that although one might argue

that Maxwell has been quite successful within the structures of both the university and academic philosophy, he is highly critical of the basic intellectual assumptions upon which opportunities and rewards are distributed in these structures. In his own words, "I am myself well aware of just how potently the philosophy of knowledge, as a result of being built into the institutional structure of academic inquiry, operates to censor out of existence work that fails to conform to its edicts. I have encountered this again and again in my own work—above all in my attempts, during the past ten to twenty years, to communicate and publish the proposals and arguments of this book!" (p. 277).

Maxwell's criticism is not that one methodological or theoretical paradigm (e.g., Marxism, behaviorism, quantitative analysis) has hegemony

within the academy, but rather that the basis upon which each of these is built, the philosophy of knowledge, is irrational and must be replaced by a philosophy of wisdom. The basic difference between these philosophies is: "Whereas for the philosophy of knowledge the fundamental kind of rational learning is acquiring knowledge, for the philosophy of wisdom the fundamental kind of rational learning is learning how to live, learning how to see, to experience, to participate in and create what is of value in existence" (p. 66). The stress on objective knowledge rather than the pursuit of human values emerged in the sixteenth and seventeenth centuries with the rise of modern science and dominates scientific inquiry today.

The dominance of the search for knowledge rather than wisdom has a number of the implications for the social sciences. The term *social science* is a typical philosophy of knowledge misnomer. The proper term, from the philosophy of wisdom perspective, is *social inquiry*. The primary goal of political inquiry is to help us to articulate political problems and problems associated with government and to propose and criticize possible solutions, thus helping us gradually to realize how we can approach these problems in a more cooperatively rational manner. In a similar manner, the basic task of the study of international relations is to help humanity to articulate global problems and to propose and criticize possible solutions, so that gradually we may develop rational approaches to global and international relations. The primary problems are not the disciplinary based intellectual problems, for example, sociologists will be more concerned with social problems than with sociological problems. Finally, the philosophy of wisdom holds that social inquiry, with its concern with problems of living, is more intellectually fundamental than the natural sciences, which are concerned with secondary problems of knowledge.

Although Maxwell argues that the development of a rational inquiry based on the philosophy of wisdom is a necessary condition for the emergence of a saner, more just and humane world, he recognizes that it is not a sufficient condition. Interest and power groups, both internal and external to the academy, are likely to resist the change called for in the revolution from knowledge to wisdom. Maxwell's position here is that the ideology of the philosophy of knowledge is not likely to lead to a questioning of the aims, rationality, or morality of science, and that "a society in which there is a tradition of rational discussion of its problems has open to it all sorts of desirable possibilities—in particular the possibility of democratic, non-authoritarian, cooperative action—not open to a society in which there is no such tradition" (pp. 161-162). It is worth a try.

However, Maxwell never does come to grips with the issue of the interests of the wealthy and powerful in determining the prevailing philosophy of our age. Marx's observations concerning the relationship between the ruling ideas and the ideas of the ruling class may be a good starting point for one interested in the politics of knowledge and wisdom.

This book may remind the reader of some of the concerns that attracted him or her to the world of ideas in general and to political science in particular. My one specific recommendation for potential readers is that they should have a familiarity with the work of Karl Popper and Thomas Kuhn before they read Maxwell.

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The Spiral of Silence: Public Opinion—Our Social Skin. By Elisabeth Noelle-Neumann. (Chicago: University of Chicago Press, 1984. Pp. xi + 184. \$20.00.)

Elisabeth Noelle-Neumann's *The Spiral of Silence* begins with a relatively simple task: how to explain an apparent anomaly in data on the voting intentions of the German electorate. It concludes as an elegant theory of public opinion. The book is an important contribution to the literature on voting behavior in particular and to public opinion research in general. It ought to be on the "must read" list of any social scientist interested in public opinion formation. Whether the spiral of silence constitutes a sufficient paradigm for public opinion research, as Noelle-Neumann suggests, will need to be determined by empirical applications.

While analyzing data for the German election of 1965, Noelle-Neumann, the director of the Public Opinion Research Center in Allensbach, Federal Republic of Germany, observed that voters began to believe that one party would win before their own vote intention changed. Why would voters who were intending to vote for one party come to believe that the other party was going to win the election?

Noelle-Neumann concluded that the question could be answered by reference to what she calls "the spiral of silence." In essence, a climate of opinion developed in Germany in which voters who supported the SDP felt increasingly uncomfortable about expressing public support for their party. As SDP supporters felt increasingly isolated, they expressed support for their party even less. And, ultimately, the threat of isolation and

pressure to conform led voters to change their vote intention.

After a very few pages dealing with the electoral data that first piqued her curiosity—those pages are really more in the nature of an introduction—Noelle-Neumann turns to a much broader inquiry into the nature of public opinion. Her obvious intention in this enterprise is to provide a basis for the conclusion that the threat of isolation is the primary driving force in public opinion formation.

I felt most comfortable with the author's references to empirical data. The former involved questionnaire items that were replicated in a number of surveys as well as a quasi-experimental design called "the train test." The results of this test are of interest given a debate going on today in the U.S.: "After being threatened verbally, smokers who had defended their right to smoke in the presence of nonsmokers showed noticeably less interest in taking part in a discussion on this topic in a train compartment" (p. 44).

Noelle-Neumann claims that the train test also demonstrates the other side of the spiral of silence—a perceived positive climate of opinion will cause individuals holding the socially accepted point of view to be more likely to state their opinion publicly: "Nonsmokers tend to be less self-assured and consequently less inclined to try to make their point of view stick. When . . . [something in their environment] shows them, however, that they are by no means alone in their views, they become noticeably more inclined to join in the conversation" (p. 45).

Noelle-Neumann's hypothesis is simple but has broad applicability. For instance, the spiral of silence provides an explanation for the phenomenon referred to as floating voting: "Repeated questioning of the same people before and after the 1972 election revealed to us that those who feel they are relatively isolated from others . . . are the ones most likely to participate in a last-minute election swing. Those with weaker self-confidence and less interest in politics are also likely to make a last-minute switch" (p. 6).

The lengthiest section of the text is devoted to a review of the classical literature on public opinion ranging from Locke to Lippmann. The purpose of this review is to establish a theoretical basis for the idea that public opinion rests on man's social nature and the resulting fear of isolation. This review may be valuable to those who adopt the book as an undergraduate or graduate text. I would have preferred that it be balanced by a like review of the relevant literature in attitude theory.

The Spiral of Silence deserves a place on every public opinion professional's shelf. The scope of the book makes it entirely appropriate as reading for any advanced course in public opinion or vot-

ing behavior. It may also be appropriate as text for introductory public opinion courses.

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Language, Form, and Inquiry: Arthur F. Bentley's Philosophy of Social Science. By James F. Ward. (Amherst: University of Massachusetts Press, 1984. Pp. vii + 270. \$24.00.)

Failed university teacher, newspaper editor, orchardist, and philosopher, Arthur Bentley was also the sideline genius of our methodological contests. James Ward's book, valuable for its detailed exposition and careful cultivation of the many volumes of Bentley's writings, will be a standard work for Bentley scholars. Ward does an excellent job of interpreting Bentley in his philosophical and scientific context.

Bentley pursued postpositivist pragmatism to its furthest reaches. Ward details the journey and gives an account of Bentley's frustration. Chapters 3 and 4 on *The Process of Government* shed light on the dynamism and philosophical subtlety of Bentley's view of groups, the shallowness of many current uses of the concept, and the operational problems Bentley left unresolved. Ward illustrates again the problem that Paul Kress pointed to in *Social Science and the Idea of Process* (University of Illinois Press, 1970), "After all, [Bentley] had not only to persuade social scientists of the futility of their present course; he also had to win them to a vision which he could not yet articulate except by primitive example" (p. 214).

Ward differs with Kress in assessing the major significance of Bentley's work as the development of a version of scientific formalism in the style of Georg Simmel. "Formalism for him is best understood as the methodological elimination of ontological claims." Take out all presuppositions and form remains. "Well-developed sciences are bundles of practices that have at their cores sets of agreements having to do with truth and other matters" (p. 226). Ward identifies Bentley's ventures into such terminology as simbiotaxiplasm (combinations of men and "assimilated things" that form society) as examples of his struggle to characterize a suitable formal core for social science. Less specific attention is paid to Bentley and Dewey's formulation of "transactionalism" in *Knowing and the Known* (Beacon Press, 1949), where aspects of a potentially useful formalism are noted, as in the "conditions for naming" (*Knowing*, . . . , pp. 48-49), the glossary of "suggested experimental namings" (pp. 72-74), and the use of transactionalism to reinterpret natural-

ism (p. 104). Ward makes clear the philosophical context of transactionalism. It would be interesting to have an analysis of applications of transactionalism as a further test of the utility of Bentley's formalism.

In Ward's view, Bentley's failure is that his "methodological teaching does not liberate itself" from the subject of study (p. 232). Ward characterizes this as "contemplative resignation." The phrase suggests that a "liberation" is possible—and desirable. Ward adopts the cause of moral philosophy, and of science, as enterprises having intrinsic value.

Bentley's relentless skepticism made him wary, committed though he was to making the world intelligible. As Bentley remarks in a chapter he wrote for *Knowing and the Known*, "Logics of the types we have been examining flutter and evade, but never attack directly the problem of sorting out and organizing words to things, and things to words, for their needs of research. They

proceed as though some sort of oracle could be issued to settle all puzzles at once, with logicians as the priests presiding over the mysteries" (pp. 38-39). Ward rightly argues that Bentley never made it to the priesthood either. Someone who holds that "knowings are behaviors" (*Knowing*. . . , p. 74) is a poor candidate in anybody's religion. Yet, as Ward himself demonstrates, Bentley is at least the most creative agnostic of the twentieth century—and that has intrinsic value in a field where neither science nor moral philosophy have yet secured an indisputable claim.

The final chapter, a tightly written review of the argument and critique of Bentley, provides an excellent guide to this intricate and stimulating exploration of Bentley. Ward's scholarship is exemplary.

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